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**PUBLICATIONS AND SERVICES:** The *American Historical Review* is published five times a year and sent to all members. It is available by subscription to institutions. The Association also publishes its *Annual Report*, the *AHA Newsletter*, a variety of pamphlets on historical subjects, and bibliographical and other volumes. To promote history and assist historians, the Association offers many other services, including publication of the *Employment Information Bulletin* four times a year. It also maintains close relations with international, specialized, state, and local historical societies through conferences and correspondence.

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**CORRESPONDENCE:** Inquiries should be addressed to the Executive Secretary at 400 A Street, S.E., Washington, D.C. 20003.

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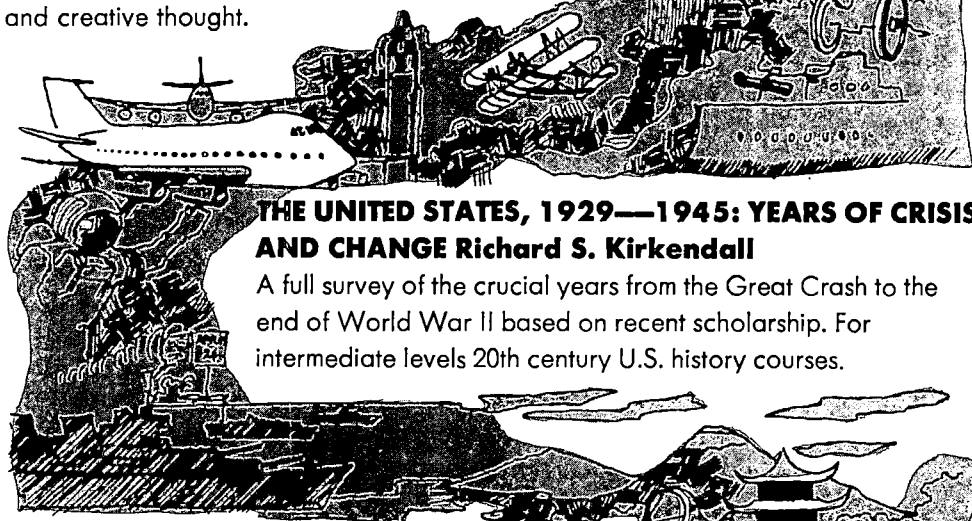
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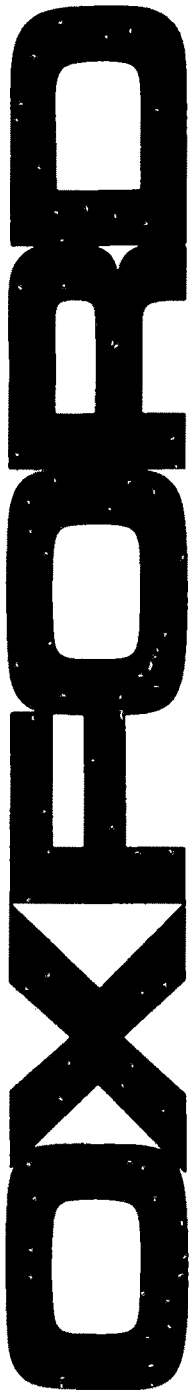
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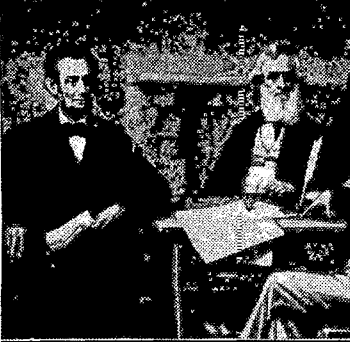
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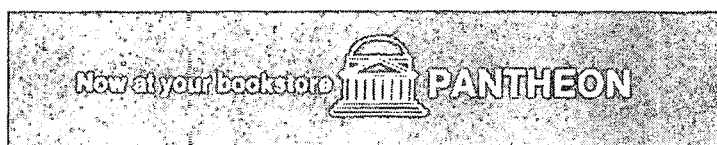
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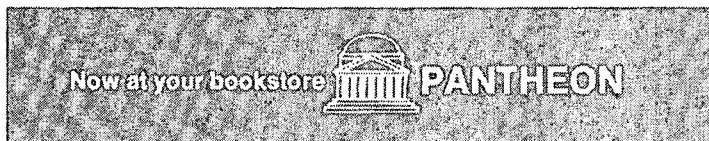
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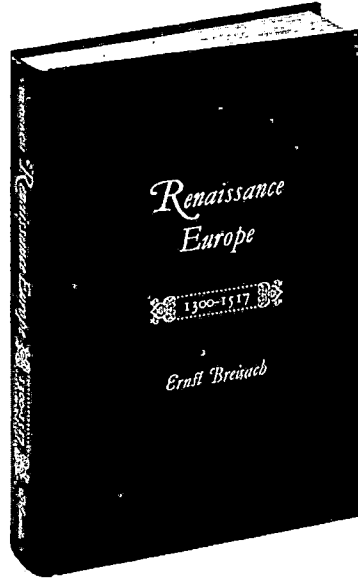
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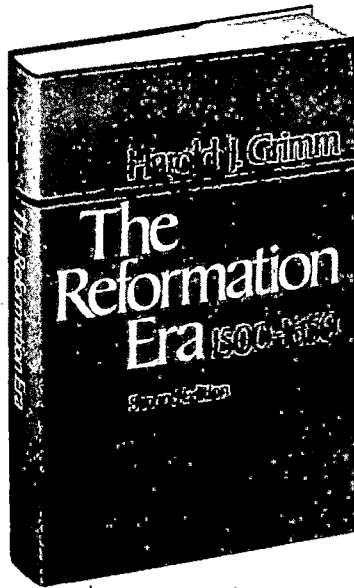
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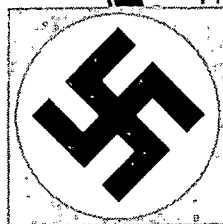
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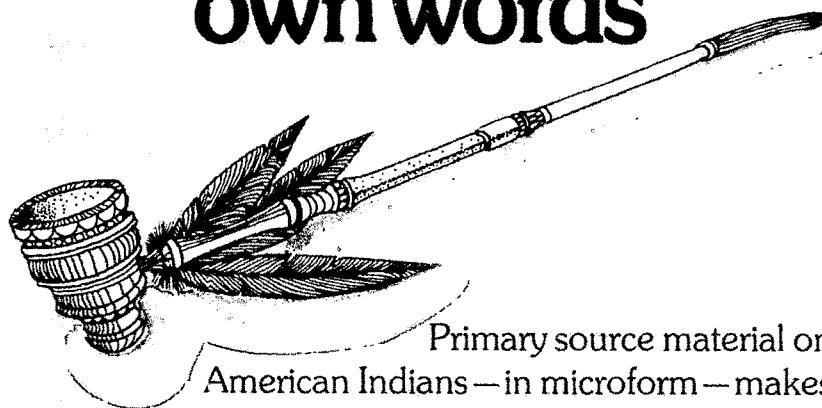
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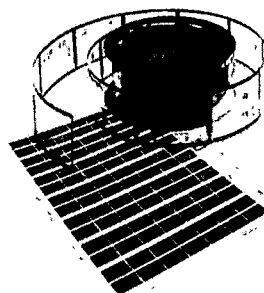


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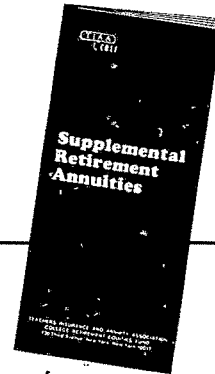
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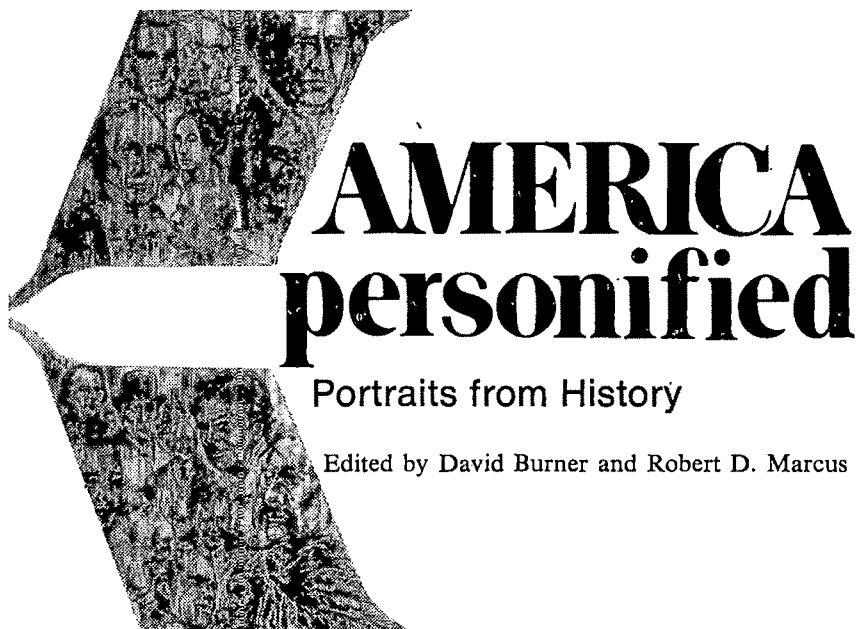
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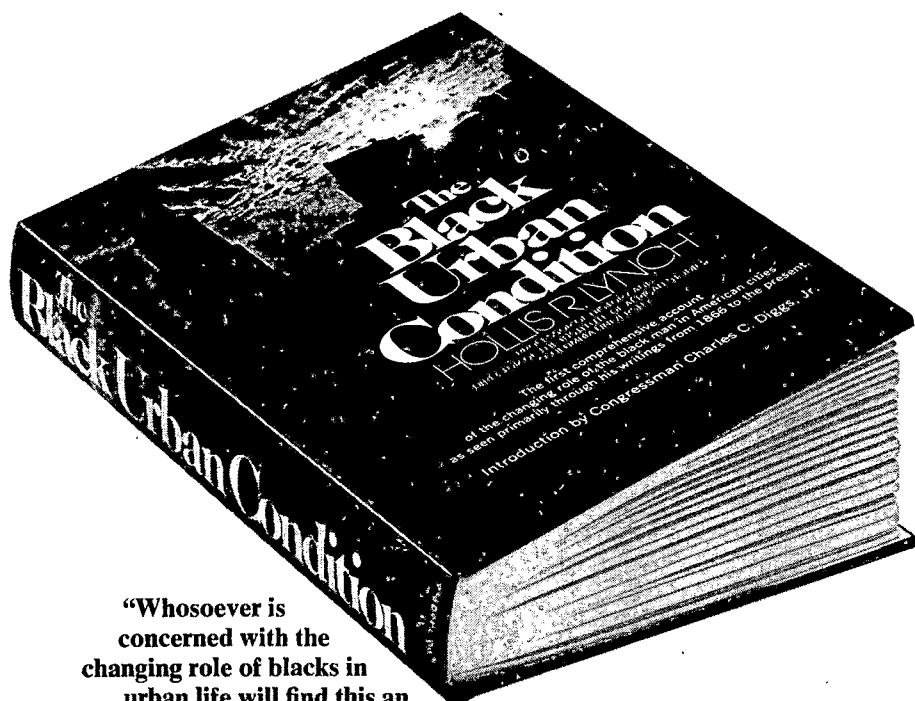
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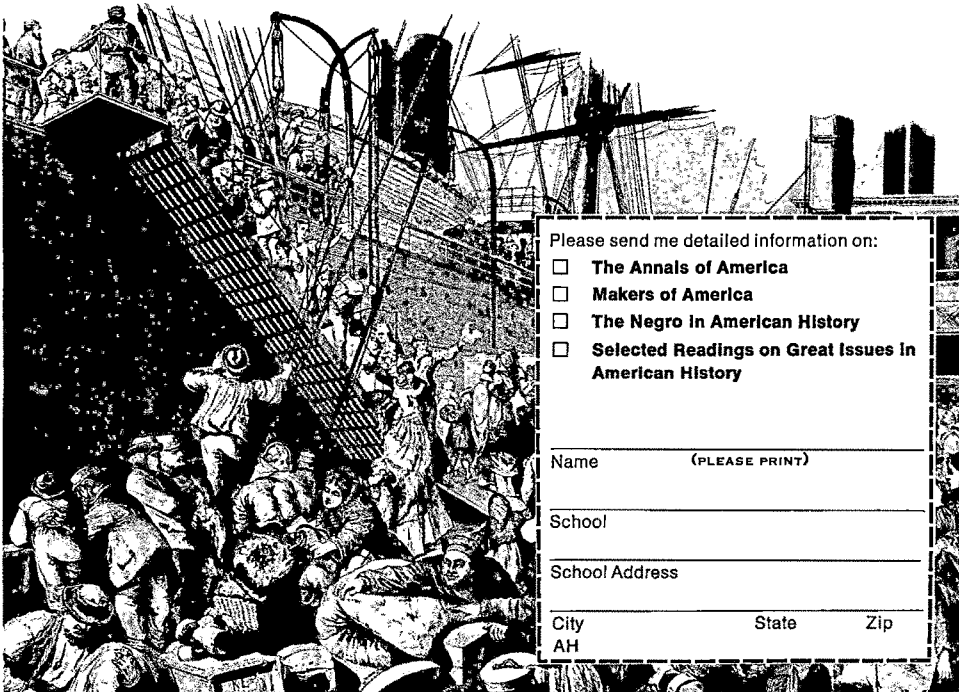
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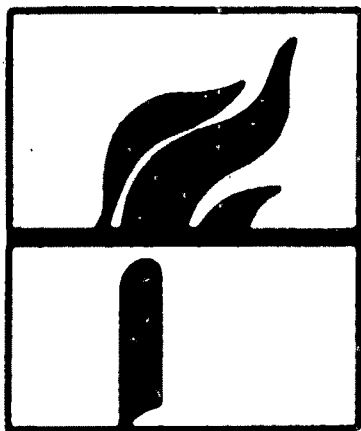
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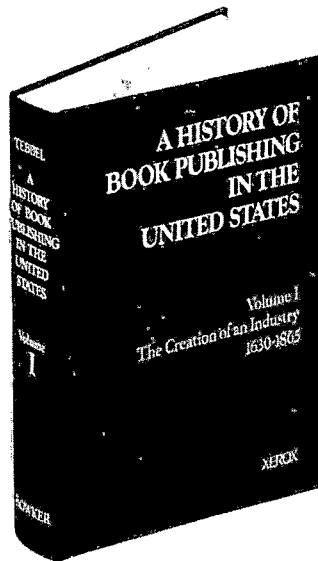
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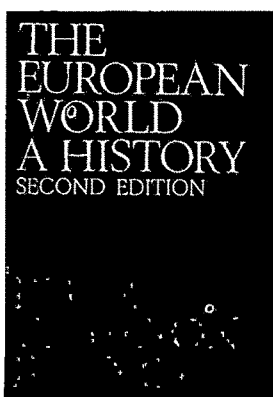
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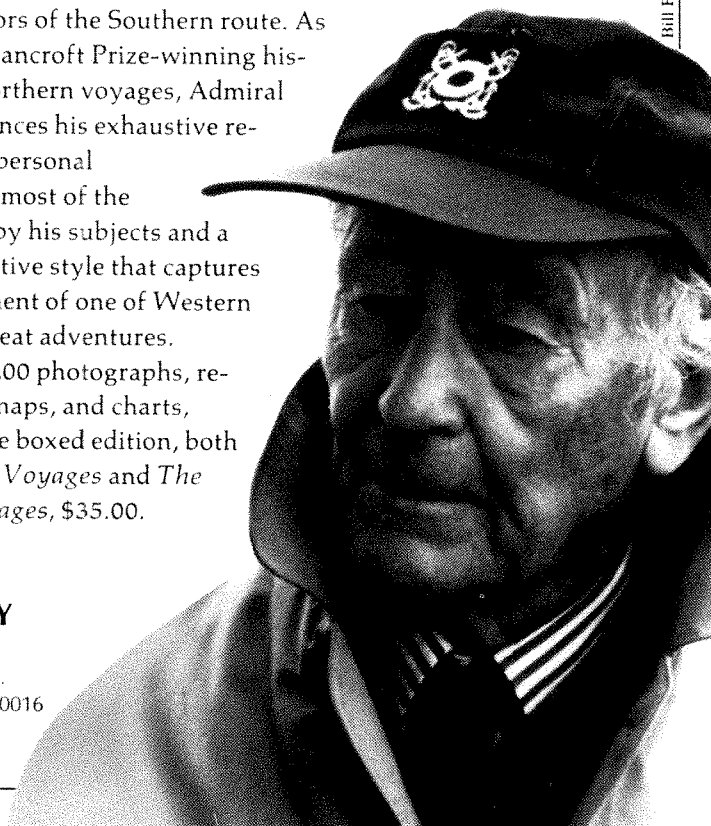
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EDWARD A. WHITCOMB completed his graduate work under Professors Alfred Cobban and Douglas Johnson at the University of London. His Ph.D. was on the French diplomatic service under Napoleon. His publications include the duties of Napoleon's diplomats and the prejudices revealed in the statistics used in describing Napoleon's invasion of Russia. In 1972–73 Professor Whitcomb was the recipient of a Canada Council Research Fellowship for one year of postdoctoral research in Paris on Caulaincourt, Napoleon's ambassador and last foreign minister.

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## The Tyranny of a Construct: Feudalism and Historians of Medieval Europe

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ELIZABETH A. R. BROWN

AT A RECENT CONFERENCE Thomas N. Bisson introduced his paper "Institutional Structures of the Medieval Peace" by cautioning his audience that in his discussion of peace movements, peace associations, and peace institutions in southern France and Spain he would not attempt to relate his findings to "feudalism."<sup>1</sup> His approach was descriptive—and thoroughly enlightening—and no further reference to any ism occurred until the question period. Then, bestowing the double-edged praise that is his hallmark, Professor John F. Benton asked how historians could have managed to overlook for so long such abundant evidence that would necessitate the revision of numerous lectures on medieval society. Responding to this remark, Professor Bisson again alluded to the eventual necessity of evaluating his conclusions with reference to the general topic of feudalism, but time prevented him from elaborating. It occurred to me as this interchange was taking place that the failure of historians to take account of the data used by Bisson may well have resulted from their concentration on feudalism—as model or Ideal Type—and their consequent tendency to disregard or dismiss documents not easily assimilable into that frame of reference.

Whatever their relevance to the subject of Professor Bisson's paper, feelings of uneasiness concerning the term "feudalism" are not uniquely mine. Historians have for years harbored doubts about the term "feudalism" and the phrase "feudal system," which has often been used as a synonym for it. One of the first, and certainly one of the wittiest and most eloquent,

An earlier version of this article was presented to a meeting of the Columbia University Seminar on Medieval Studies, May 8, 1973. I am grateful to the members of the seminar for their questions and suggestions. For their advice and counsel I would also like to express my thanks to Professor Fredric Cheyette of Amherst College, Professor John Bell Henneman of the University of Iowa, Professor Joshua Prawer of the Israel Academy of Sciences and Humanities, Professor Thomas N. Bisson of the University of California at Berkeley, Professor John F. Benton of the California Institute of Technology, Professors Edwin Burrows, Philip Dawson, Charlton Lewis, and Hyman Sardy of Brooklyn College of the City University of New York, Barbara W. Tuchman, and finally the members of the History Club and my students at Brooklyn College.

<sup>1</sup> Thomas N. Bisson, "Institutional Structures of the Medieval Peace," a paper presented to a colloquium held at Princeton University on March 31, 1973.



to comment on the problem was Frederic William Maitland. In lectures on English constitutional history prepared in 1887 and 1888 he wrote:

Now were an examiner to ask who introduced the feudal system into England? one very good answer, if properly explained, would be Henry Spelman, and if there followed the question, what was the feudal system? a good answer to that would be, an early essay in comparative jurisprudence. . . . If my examiner went on with his questions and asked me, when did the feudal system attain its most perfect development? I should answer, about the middle of the last century.<sup>2</sup>

Thanks to J. G. A. Pocock, it is now known that Henry Spelman, a learned English antiquarian of the seventeenth century, used neither the term "feudal system" nor the word "feudalism," but this does not detract from the validity or the importance of Maitland's observations. Following in the steps of the Scottish legal scholar Sir Thomas Craig, Spelman held that the social and political relationships of medieval England had been uniform and systematic enough to be described adequately as regulated by a "'feudal law' [which] was an hierarchical system imposed from above as a matter of state policy." The work of Craig and Spelman had its virtues, for they were the first British historians to attempt to relate British institutions to continental developments. Both, however, relied for their knowledge of continental institutions on Cujas's and Hotman's sixteenth-century editions of the twelfth-century Lombard *Libri Feudorum*, which gave, to paraphrase Pocock, a precise and detailed "definition of the *feudum* whereby it could be recognized in any part of Europe," or, as he says, "a systematic exposition of the principles of tenure, forfeiture and inheritance." These criteria Craig and Spelman employed to classify the evidence from Scottish and English sources, and their simplification and regimentation of phenomena notably offset the advantages to historical thought of their demonstration that the development of England and Scotland could be understood only in the context of the European experience.<sup>3</sup>

Given these beginnings, it is no wonder that eighteenth-century British writers began to accept the concept of a uniform feudal government and to concentrate on the system, the construct, instead of investigating the various social and political relationships found in medieval Europe. "They were," Pocock observes, "making an 'ism' of [feudalism]; they were reflecting on its essence and nature and endeavoring to fit it into a pattern of general ideas."<sup>4</sup> In so doing they resembled Boulainvilliers and Montesquieu, who

<sup>2</sup> Frederic William Maitland, *The Constitutional History of England*, ed. H. A. L. Fisher (Cambridge, 1908), 142. See also Fisher's introduction to this edition, p. v.

<sup>3</sup> J. G. A. Pocock, *The Ancient Constitution and the Feudal Law: English Historical Thought in the Seventeenth Century* (Cambridge, 1957), 70 n.2, 93-94, 249, 79-80, 97-99, 70-79, 72, 84, 99, 103, 102. Pocock perhaps exaggerates these advantages (p. 102) because of the strength of his admiration for the boldness and imagination with which Craig and Spelman challenged the distortedly insular approach taken by Coke and the common lawyers. It seems clear, furthermore, that Pocock himself does not question the validity or the usefulness of the term "feudalism."

<sup>4</sup> *Ibid.*, 249; see also Robert Boutruche, *Seigneurie et féodalité: Le premier âge des liens d'homme à homme* (Paris, 1959), 15 nn.16-17, 16 n.20.

wrote of *féodalité* and *lois féodales* as distinguishing a state of society, thus, incidentally, expanding the concept to include a far wider range of phenomena than it had for legal scholars.<sup>5</sup> The writers of the eighteenth century, like those of later times, assigned different meanings to the term *féodalité*, or, in English, "feodality." Some used it to designate a system of government, some to refer to conditions that developed as public power disappeared. By 1800 the construct had been launched and the expression "feudal system" devised; by the mid-nineteenth century the word "feudalism" was in use. The way was prepared for future scholars to study feudalism—whatever it was conceived to be—scientifically and for others to employ the ism to refer, abusively, to those selected elements of the past that were to be overthrown, abolished, or inexorably superseded.<sup>6</sup>

SINCE THE MIDDLE OF THE NINETEENTH CENTURY the concepts of feudalism and the feudal system have dominated the study of the medieval past. The appeal of these words, which provide a short, easy means of referring to the European social and political situation over an enormous stretch of time, has proved virtually impossible to resist, for they pander to the human desire to grasp—or to think one is grasping—a subject known or suspected to be complex by applying to it a simple label simplistically defined. The great authority of these terms has radically influenced the way in which the history of the Middle Ages has been conceptualized and investigated, encouraging concentration on oversimplified models that are applied as standards and stimulating investigation of similarities and differences, norms and deviations. As a result scholars have disregarded or paid insufficient attention to recalcitrant data that their models do not prepare them to expect.

But let us return to Maitland. Implicit in his assessment of Spelman and the feudal system is a clear objection to applying the label "feudal system" to medieval England, presumably because of a belief that England never underwent a systematization of social and political life—or, as Maitland puts it, never experienced "the development of what can properly be called a feudal system." Less evident, perhaps, is a hesitancy about the propriety of using the phrase "feudal system" at all. That Maitland questioned the wisdom of applying it to conditions of medieval society is hard to dispute, however, for in his lectures he remarks,

The phrase [feudal system] has thus become for us so large and vague that it is quite possible to maintain that of all countries England was the most, or for the

<sup>5</sup> Boutruche, *Seigneurie et féodalité*, 13–14; Marc Bloch, *La Société féodale* (Paris, 1949), 1: 1–3. The English edition, with a foreword by M. M. Postan, was translated by L. A. Manyon and is entitled *Feudal Society* (Chicago, 1961); the corresponding pages are xvi–xviii.

<sup>6</sup> Boutruche, *Seigneurie et féodalité*, 16, 18–23. See also the *Oxford English Dictionary*, s.v. "feudal," "feudalism," and "feudality."

matter of that the least, feudalized; that William the Conqueror introduced, or for the matter of that suppressed, the feudal system.<sup>7</sup>

Still, having bemoaned the terminological situation, Maitland proceeds to use the term "feudalism," equated by him with "feudal system."<sup>8</sup> He announces that "the feudalism of France differs radically from the feudalism of England, that the feudalism of the thirteenth is very different from that of the eleventh century." He then goes on to give his own definition of feudalism, emphasizing ties of vassalage, fiefs, service in arms owed the lord, and private administration of justice. Using this definition, he discusses the question of the progress toward such an organization that England had been making before the Norman Conquest, and he concludes, "Speaking generally then, that ideal feudalism of which we have spoken, an ideal which was pretty completely realized in France during the tenth, eleventh and twelfth centuries, was never realized in England." Here, he says, "the force of feudalism [was] limited and checked by other ideas."<sup>9</sup>

As these statements show, Maitland's tolerance for unresolved contradictions was high, and other historians have demonstrated a similarly striking capacity for living with inconsistency. Although they attack the term "feudalism," they are still unwilling and perhaps unable—whether from habit, inertia, or simple inattention—to jettison the word. Consider H. G. Richardson and G. O. Sayles. In a book published in 1963 they denounce "feudal" and "feudalism" as "the most regrettable coinages ever put into circulation to debase the language of historians." "We would, if we could," they declare, "avoid using them, for they have been given so many and such imprecise meanings." They confess, however—without apology or explanation—that they cannot "rid [themselves] of the words and must live with them" and therefore proclaim their determination to "endeavor, when [they use] them, to do so without ambiguity." They evidently have some sense of attachment, however grudging, to the terms, and their feelings are reflected in their insistence that "if the concept and the term are to be in the least useful"—thus implying that they can be—"there must be precise definition." Such definition they do not, unfortunately, offer. Nonetheless they doggedly persist in using the words, and they spend a large portion of their book dealing with their "thesis of the relative unimportance of any

<sup>7</sup> Maitland, *Constitutional History*, 161, 143. See also Sir Frederick Pollock and Frederic William Maitland, *The History of English Law before the Time of Edward I* (2d ed., introd. S. F. C. Milsom; Cambridge, 1968), 1: 66–67; and Frederic William Maitland, *Collected Papers*, ed. H. A. L. Fisher (Cambridge, 1911), 1: 489.

<sup>8</sup> Maitland does not subject the word "feudalism" to the same critical scrutiny he applies to the phrase "feudal system," and he is far less wary of using the former than the latter. At one point in his lectures he seems to be distinguishing between the two—"we do not hear of a feudal system until long after feudalism has ceased to exist"—but he also uses them as equivalents. In his conclusion he indicates that he considers "the development of . . . a feudal system" the same as the realization of "ideal feudalism." *Constitutional History*, 141–43, 161–63.

<sup>9</sup> *Ibid.*, 143–64.

element of 'feudalism' in post-Conquest England" and of "the essential continuity of English institutions."<sup>10</sup>

Such an approach logically requires isolating those elements that can properly be called feudal from those that cannot. Since Richardson and Sayles never explicitly objectify the enemy, however, their readers are left to deduce from their arguments just what phenomena they consider essential components of feudalism. Homage, " 'feudal' incidents," honors and honorial courts, knightly service connected with fiefs, and the use of military tenures for military purposes are all linked in one way or another with feudalism, although Richardson and Sayles clearly suggest that, unless found in their Franco-Norman forms, these elements should not be considered truly feudal. Thus the authors attempt to validate their hypothesis by showing either that these or similar institutions existed in England before 1066—and hence are to be classified as Old English and therefore not Norman feudal—or that they had no real importance after that date.<sup>11</sup> In the end, coming to grips with the problem of definition, they abruptly abandon their previous criteria. So that they can pronounce England safely nonfeudal and therefore non-French, they fall back on what they call "the classical theory of feudalism," described as the idea of lordship diminished by fragmentation or of "sovereignty . . . divided between the king and his feudataries," neither of which was ever found in England. They warn that feudalism should not be defined simply in terms of tenure, since if it is it will be found everywhere.<sup>12</sup>

As their lengthy discussion and conclusion make clear, Richardson and Sayles were never fully convinced, despite their initial volleys, that feudalism was in fact no "more than an arbitrary pattern imposed by modern writers upon men long dead and events long past." Although they end their analysis by remarking of the word "feudal" that "an adjective so ambiguous and so misleading is best avoided," their repeated use of the term belies their alleged distaste.<sup>13</sup>

IF NUMEROUS ARGUMENTS in defense of feudalism have been advanced, "utility" and "indispensability" are the chief rallying cries of the term's defenders. Let us turn first to the criterion of utility.

In the introduction to his classic study *Feudalism*, F. L. Ganshof states that he intends his book to facilitate the work of students of medieval society. In analyzing and describing feudal institutions he says he has "endeavored to bring out as clearly as possible their essential features, since, once these

<sup>10</sup> H. G. Richardson and G. O. Sayles, *The Governance of Mediaeval England from the Conquest to Magna Carta* (Edinburgh, 1963), 30, 92, 117-18, 30-31, 105, 116.

<sup>11</sup> *Ibid.*, 36-38, 77, 99, 105-12, 115; see also 85-91, 147, and the comments on p. 116: "The Normans were already familiar with much that they found in England, but we are not thereby warranted in terming those familiar things 'feudal' or in asserting that England was already 'feudal.'"

<sup>12</sup> *Ibid.*, 117-18.

<sup>13</sup> *Ibid.*, 92, 118.

are grasped, it is easy for the student to disentangle the elements that can properly be described as feudal in the institutions of the period or country with which he is primarily concerned."<sup>14</sup> Helping the scholar as well as the student to evaluate, analyze, and categorize the past is also important to Michael Postan, and in his foreword to the English edition of Marc Bloch's *Feudal Society* he argues that the usefulness of "generalized concepts" such as feudalism lies in their ability to "help us to distinguish one historical situation from another and to align similar situations in different countries and even in different periods." For Postan greater complexity apparently means greater utility, and he prefers Bloch's definition of feudalism, which embraces "most of the significant features of medieval society," to "constitutional and legal concepts of feudalism" centering on "military service" and "contractual principles." These latter concepts, he feels, may have some virtue as pedagogical devices, to promote "intellectual discipline," and to serve as "an antidote to the journalistic levities of modern historiography." Still, they cannot validly be considered "an intellectual tool, to be used in the study of society."<sup>15</sup>

If Postan draws a rather unsettling distinction between pedagogy on the one hand and research and sound intellectual endeavor on the other, it is clear that he is not alone in considering appropriate for the student what is decried for the scholar. This "track" approach to feudalism is widespread, even though those who espouse it may differ concerning what should be taught at different levels. Postan envisions progression from a partial to a more complex model, always retaining the term "feudalism" to denote the model. Others, expressing fundamental objections to the misleading impression of simplicity and system they believe inevitably associated with isms, still argue that authors of basic textbooks—as opposed to advanced studies—would be lost without the concept of feudalism. This rather inconsistent attitude apparently springs from two convictions: first, that beginning students are incapable of dealing with complex and diverse development and must for their own good be presented with an artificially regular schema; and second, that the term "feudalism" somehow helps these students by serving as a handy, familiar tag to which to attach consciously oversimplified generalizations. Later, as graduate students, they are presumably to be introduced to qualifications and complications, and finally, as scholars and initiates into the mysteries of the trade, they are to be encouraged to discard the offending ism for purposes of research, if not for purposes of teaching their own beginning students. Charles T. Wood, although not explicitly endorsing the use of the term "feudalism," writes that "the feudal pyramid . . . makes for clear diagrams, and schoolboys have to begin somewhere."

<sup>14</sup> F. L. Ganshof, *Feudalism*, foreword F. M. Stenton, tr. Philip Grierson (London, 1952), xviii; see also 151.

<sup>15</sup> Postan, foreword to Bloch, *Feudal Society*, xiv, xiii.

Still, he admits, "where they do begin is rather far removed from reality."<sup>16</sup>

Postan, and presumably Ganshof, feels that employing the construct has the virtue of enabling scholars to distinguish likenesses among different times and areas. Similarly John Le Patourel advocates formulating a definition of feudalism that could be used "as a measuring-rod,"<sup>17</sup> and such a standard could presumably be relied on not only, as he wants, to clarify "the old argument" over the introduction of feudalism into England but also, as Postan argues, to advance the work of those concerned with comparing developments in different countries.

If feudalism is praised as a teaching device and as a means of understanding societies, it is also said to be "indispensable," and that for a number of reasons. Marc Bloch maintains that scientists cannot function without abstractions and that since historians are scientists, they also require abstractions. The specific abstractions "feudal" and "feudalism" are defended on the grounds that, however awkward and inappropriate in terms of their original connotations these words and others like them may be, the historian is in this respect no worse off than the scientist, who must also make do with inconvenient and unsuitable terminology.<sup>18</sup> Michael Postan goes beyond Bloch to declare that "without generalized terms representing entire groups of phenomena not only history but all intelligent discourse would be impossible," and he maintains that no difference exists between such a word as "feudalism" and other general terms like "war" and "agriculture."<sup>19</sup> Equally positively, if less aggressively, Fredric Cheyette has insisted that the term "feudalism" cannot "simply be discarded—the verbal detours one would have to make to replace it would be strained as well as disingenuous."<sup>20</sup> Otto Hintze argues that the concept is indispensable not only for reasons of practicality and convenience but also because of the deficiencies of the processes of human thought, assumed to be incapable of comprehending the complexities of the real world. Hintze asserts that since "it is impossible to grasp the complicated circumstances of historical life, so laden with unique occurrences, in a few universal and unambiguous concepts—as is

<sup>16</sup> Wood's own description of medieval society deals with human beings rather than schemas, but he occasionally uses the terms "feudal" and "feudalism," which are not defined. *The Quest for Eternity: Medieval Manners and Morals* (New York, 1971), 28, 55–56, 177. Wood's index (p. 227) shows that he has not discarded the term "feudalism," which he seems to see as closely linked with vassalage.

<sup>17</sup> John Le Patourel, review of Richardson and Sayles, *Governance of Mediaeval England*, in *English Historical Review*, 80 (1965): 117 n.1; and see also Max Weber, *The Theory of Social and Economic Organization*, ed. Talcott Parsons, tr. A. M. Henderson and Talcott Parsons (New York, 1947), 329.

<sup>18</sup> Marc Bloch, *Apologie pour l'histoire ou Métier d'historien* (Paris, 1949), 86–87. The corresponding pages in the English edition—*The Historian's Craft*, ed. Lucien Febvre, tr. Peter Putnam (New York, 1953)—are 169–71.

<sup>19</sup> Postan, foreword to Bloch, *Feudal Society*, xiv.

<sup>20</sup> Fredric L. Cheyette, "Some Notations on Mr. Hollister's 'Irony,'" *Journal of British Studies*, 5 (1965): 4; see also Cheyette, ed., *Lordship and Community in Medieval Europe: Selected Readings* (New York, 1968), 2–3.

done in the natural sciences," historians must use "intuitive abstractions" and create "Ideal Types, and such types indeed underlie our scholarly terminology."<sup>21</sup>

Even its most eloquent advocates readily acknowledge the difficulties associated with the use of the term "feudalism." Marc Bloch, for one, states that "nearly every historian understands the word as he pleases," and "even if we do define, it is usually every man for himself." He admits that the word is charged with emotional overtones<sup>22</sup> and is in fact "very ill-chosen,"<sup>23</sup> and he acknowledges that, in general, abstractions which are "ill-chosen or too mechanically applied" should be avoided.<sup>24</sup> He goes so far as to declare that the word "capitalism" has lost its usefulness because it has become burdened with ambiguities and because it is "carelessly applied to the most diverse civilizations," so that, as a result, "it almost inevitably results in concealing their original features."<sup>25</sup> Even Postan, whose loyalty to Bloch exceeds Bloch's sense of commitment to his own ideas, grants that comprehensive terms like "feudalism" "over-simplify the reality they purport to epitomize," and he confesses that

in some contexts the practice of giving general names to whole epochs can even be dangerous, [luring] its practitioners into the worst pitfalls of the nominalist fallacy, and [encouraging] them to endow their terms with real existence, to derive features of an epoch from the etymology of the word used to describe it or to construct edifices of historical argument out of mere semantic conceits.<sup>26</sup>

THE VARIETY OF EXISTING DEFINITIONS of the term and the general unwillingness of any historian to accept any other historian's characterization of feudalism constitute a prime source of confusion. The best definition would doubtless be, as Cheyette suggests, one that helped "to make the body of evidence on medieval institutions coherent," but he himself has not found or formulated any definition to accomplish this purpose.<sup>27</sup> In the absence of consensus, the play with meanings has flourished and still continues.

<sup>21</sup> Otto Hintze, "Wesen und Verbreitung des Feudalismus" (1929), in Hintze, *Gesammelte Abhandlungen*, ed. Gerhard Oestreich, 1 (2d ed.; Göttingen, 1962): 85; for an English translation of the article, entitled "The Nature of Feudalism," see Cheyette, *Lordship and Community*, 22-31. See, too, the comments of Michael Lane and particularly the enlightening passage quoted from Max Weber, in which Weber describes how ideal types are formulated. *Introduction to Structuralism* (New York, 1970), 25-26.

<sup>22</sup> Bloch, *Apologie*, 89, 87 (*Historian's Craft*, 176, 171).

<sup>23</sup> "Un mot fort mal choisi." Bloch, *Société féodale*, 1: 3 (*Feudal Society*, xviii).

<sup>24</sup> Bloch, *Apologie*, 88 (*Historian's Craft*, 173). Bloch comments that the feudalisms which scholars have located in different parts of the world "bear scarcely any resemblance to each other." *Apologie*, 89 (*Historian's Craft*, 175-76).

<sup>25</sup> *Ibid.*, 88 (*Historian's Craft*, 174). For a fuller, if less extreme, analysis of the similar problems posed by using the terms capitalism and feudalism, see the review of J. Q. C. Mackrell, *The Attack on 'Feudalism' in 18th Century France* (London, 1973), in the *Times Literary Supplement*, Feb. 15, 1974, p. 160.

<sup>26</sup> Postan, foreword to Bloch, *Feudal Society*, xiv.

<sup>27</sup> Cheyette, "Some Notations on Mr. Hollister's 'Irony,'" 4, 12; see also 5-6, where he states that the usefulness of the term (he may in fact mean of the definition) "is determined by how it helps to order the evidence."

The sweeping perspective adopted by Marc Bloch produced a definition of European feudalism—equated by Bloch with feudal society and, in the translation of his book, with feudal system<sup>28</sup>—that in effect summarizes the topics treated in the central section of his *La Société féodale*. It encompasses a wide range of aspects of medieval life:

A subject peasantry; widespread use of the service tenement (i.e. the fief) instead of a salary, which was out of the question; the supremacy of a class of specialized warriors; ties of obedience and protection which bind man to man and, within the warrior class, assume the distinctive form called vassalage; fragmentation of authority—leading inevitably to disorder; and, in the midst of all this, the survival of other forms of association, family and State, of which the latter, during the second feudal age, was to acquire renewed strength.<sup>29</sup>

Some historians have accepted this inclusive list as a definition of feudalism, but others would prefer to link it only with feudal society, which they feel can and should be distinguished from a more narrowly conceived feudalism, in which the fief is accorded greater prominence than Bloch gives it.<sup>30</sup> Ganshof, for one, believes that in the Middle Ages “the fief, if not the cornerstone, was at least the most important element in the graded system of rights over land which this type of society involved.” The definition of feudalism he prefers—“the narrow, technical, legal sense of the word”—concentrates on service and maintenance and emphasizes the fief, while it excludes entirely the private exercise of public justice and jurisdiction. For Ganshof feudalism is envisaged as

a body of institutions creating and regulating the obligations of obedience and service—mainly military service—on the part of a free man (the vassal) towards another free man (the lord), and the obligations of protection and maintenance on the part of the lord with regard to his vassal. The obligation of maintenance had usually as one of its effects the grant by the lord to his vassal of a unit of real property known as a fief.

<sup>28</sup> Bloch, *Société féodale*, 2: 244–49 (*Feudal Society*, 443–45). In the translation (p. 443) “the feudal system” replaces Bloch’s “le régime féodal” (2: 245). Similarly, Bloch’s “les féodalités d’importation” (1: 289–92) become in translation “the imported feudal systems” (pp. 187–89). Both Ganshof and David Herlihy have indicated—misleadingly it seems to me—that Bloch perceived a fundamental difference between feudalism and feudal society. Ganshof, *Feudalism*, xvi; David Herlihy, ed., *The History of Feudalism* (New York, 1970), xix.

<sup>29</sup> Bloch, *Société féodale*, 249–50 (*Feudal Society*, 446).

<sup>30</sup> Herlihy, *History of Feudalism*, xix; Ganshof, *Feudalism*, xv. Ganshof’s description of feudalism as a form of society on the same page diverges at many points from Bloch’s: “a development pushed to extremes of the element of personal dependence in society, with a specialized military class occupying the higher levels in the social scale; an extreme subdivision of the rights of real property; a graded system of rights over land created by this subdivision and corresponding in broad outline to the grades of personal dependence just referred to; and a dispersal of political authority amongst a hierarchy of persons who exercise in their own interest powers normally attributed to the State and which are often, in fact, derived from its break-up.” Here there is no mention of peasantry or family; here the state is mentioned only by virtue of its dissolution (although see also pp. 141–51 for a lengthy discussion of feudalism and the state); here there is a stress on landed rights and property missing in Bloch’s definition.



Although Ganshof admits that "powers of jurisdiction [in particular what one normally calls feudal jurisdiction] were . . . very closely bound up with feudal relationships," he states firmly that "there was nothing in the relationships of feudalism . . . which required that a vassal receiving investiture of a fief should necessarily have the profits of jurisdiction within it, nor even that he should exercise such jurisdiction."<sup>31</sup>

Ganshof may have his followers, particularly among historians of the Normans and the English.<sup>32</sup> On the other hand, many scholars insist that the private exercise of public governmental authority—an element rejected by Ganshof—is the single essential component in any definition of feudalism. Several years ago Joseph R. Strayer adopted this position when he advocated a definition focusing on jurisdiction and omitting most of the other factors contained in the definitions just examined. "To obtain a usable concept of feudalism," Strayer argued, "we must eliminate extraneous factors and aspects which are common to many types of society." Having lopped off aristocracy, "the great estate worked by dependent or servile labor," "the relationship between lord and man," and "the system of dependent land tenures," he concluded that it is "only when rights of government (not mere political influence) are attached to lordship and fiefs that we can speak of fully developed feudalism in Western Europe."<sup>33</sup> Subsequently Strayer

<sup>31</sup> Ganshof, *Feudalism*, xvi–xvii, 143, 141.

<sup>32</sup> Similar to but narrower than Ganshof's is the definition of feudalism offered by D. C. Douglas. Since Douglas's works deal primarily with Normandy and the Norman conquests it is understandable that, like Ganshof, he should not consider the disintegration of central control a basic element. For Douglas two ideas are important: "the principle that the amount of service owed should be clearly determined before the grant of the fief" and "the notion of liege-homage." *The Norman Achievement, 1050–1100* (Berkeley, 1969), 177; see also 179. Douglas also emphasizes the idea of contractual military service, isolating this as the core of the "Norman feudal custom," which, he says, William the Conqueror interpreted "in a sense advantageous to himself" when he "[suddenly introduced] military feudalism into England." *William the Conqueror: The Norman Impact upon England* (Berkeley, 1964), 100, 101, 103, 283. See also Cheyette, who counsels historians to "consider feudalism a *technique*, rather than an *institution*, . . . a technique involving above all a relation of personal dependence and service normally sealed by the grant of a dependent tenure or some other form of material support, and confined to that group of professional warriors who in time become the nobility, the *miles* [*sic*], the *domini*—a technique used to achieve certain purposes in certain places at certain times." "Some Notations on Mr. Hollister's 'Irony,'" 12.

<sup>33</sup> Joseph R. Strayer, "Feudalism in Western Europe," in Rushton Coulborn, ed., *Feudalism in History* (Princeton, 1956), 16, reprinted in Cheyette, *Lordship and Community*, 13. A similar definition appears in a lecture presented by Strayer in 1963 and published four years later as "The Two Levels of Feudalism," in Robert S. Hoyt, ed., *Life and Thought in the Early Middle Ages* (Minneapolis, 1967), 52–53, reprinted in Joseph R. Strayer, *Medieval Statecraft and the Perspectives of History: Essays by Joseph R. Strayer*, ed. John F. Benton and Thomas N. Bisson (Princeton, 1971), 63–65. In this essay Strayer maintains that a broader definition, referring to economic and social conditions, "in fact defined nothing," and he asserts that "the narrow, military definition of feudalism" ("a way of raising an army of heavy-armed cavalymen by uniting the two institutions of vassalage and the fief"), while laudably precise, is "too limited" to be useful, since, if defined in this way, feudalism "would have little historical significance." In Hoyt, *Life and Thought*, 52–53 (in Strayer, *Medieval Statecraft*, 64–65). See also Strayer's comments in *Feudalism* (Princeton, 1965), 13–14. This point of view was again expressed, in modified form, in an essay Strayer published in 1968: "The Tokugawa Period and Japanese Feudalism," in John W. Hall and Marius B. Jansen, eds., *Studies in the Institutional History of Modern Japan* (Princeton, 1968), 3, reprinted in Strayer, *Medieval Statecraft*, 90. In this

decided that this definition was defective,<sup>34</sup> and in 1965 he advanced one that included a military as well as a political element. Then he presented as "the basic characteristics of feudalism in Western Europe . . . a fragmentation of political authority, public power in private hands, and a military system in which an essential part of the armed forces is secured through private contracts." Thus feudalism was seen not only as "a method of government" but also as "a way of securing the forces necessary to preserve that method of government." It seems clear, however, that Strayer still considered the jurisdictional element fundamental, for in concluding his discussion he wrote that "a drive for political power by the aristocracy led to the rise of feudalism."<sup>35</sup>

Other approaches to the problem of defining feudalism have been taken. In 1953 Georges Duby stated a bit hesitantly that "what one refers to as feudalism" (*ce qu'on appelle la féodalité*) should be understood to have two aspects, the political—involving the dissolution of sovereignty—and the economic—the constitution of a coherent network of dependencies embracing all lands and through them their holders.<sup>36</sup> Thus he created a bridge

essay Strayer states that "in political terms, feudalism is marked by a fragmentation of political authority, private possession of public rights, and a ruling class composed (at least originally) of military leaders and their followers." Note that this definition, explicitly couched "in political terms," does not exclude the possibility of formulating other definitions phrased in different terms.

<sup>34</sup> This modification resulted from a reorientation of approach that occurred in 1962 and 1963, when Strayer established his concept of two levels of feudalism. In reviewing Marie Fauroux's *Recueil des actes des ducs de Normandie, 911-1066* (Caen, 1961), Strayer commented that "many scholars have failed to see that there were really two feudalisms—the feudalism of the armed retainer or knight, and the feudalism of the counts and other great lords who were practically independent rulers of their districts. The two feudalisms began at different times and under different circumstances, and it was a long time before they were fully meshed together." In *Speculum*, 37 (1962): 608. Although Strayer did not explicitly define feudalism, his discussion revealed that "Norman feudalism of the classic type" required the holding of "land in return for a definite quota of military service." "Knights and other vassals" were important not only "for military purposes" but also as "part of the governing group," whose aid and counsel the duke needed to rule effectively, and who possessed local administrative authority (pp. 608-09). It is hard to reconcile this analysis with a definition of feudalism that emphasizes the disintegration of central authority and the consequent distribution of political power among numerous members of a ruling group, and in 1963 Strayer acknowledged that in Normandy political fragmentation—an essential element of the political definition of feudalism he described in the same essay as the original and "best" definition—was tardy and incomplete. "Two Levels of Feudalism," in Hoyt, *Life and Thought*, 51-52, see also 63-65 (in Strayer, *Medieval Statecraft*, 63, see also 74-75). In addition see Strayer, *Feudalism*, 39. Even outside Normandy it was not until the eleventh century—and then not consistently and regularly—that the lower as well as the higher social and military orders distinguished by Strayer can be said to have exercised independent political power. With the inadequacy of the political definition of feudalism exposed, it must have become evident that some additional element or elements would have to be added to produce a satisfactory definition of the term.

<sup>35</sup> Strayer, *Feudalism*, 13, 74. Note, too, that in "The Tokugawa Period," published in 1968, Strayer still laid heavy emphasis on the political aspect of feudalism.

<sup>36</sup> Georges Duby, *La société aux XI<sup>e</sup> et XII<sup>e</sup> siècles dans la région mâconnaise* (Paris, 1953), 643, the corresponding page in the reprint (Paris, 1971) is 481. Duby's evasive approach to the word *féodalité* reappears in his book *Guerriers et paysans, VII<sup>e</sup>-XII<sup>e</sup> siècle: Premier essor de l'économie européenne* (Paris, 1973). Here he uses terms reminiscent of those he employed in 1953 as he refers to "ce que les historiens ont coutume d'appeler la féodalité" (p. 179). Calling it "un mouvement de très grande amplitude," he does not define it precisely and explicitly, although he says that it was characterized by "la décomposition de l'autorité

of sorts, reconciling the definitions of Strayer and Ganshof. Later, however, Duby turned from government and land to mentalities, and in 1958 he suggested that feudalism might best be considered

a psychological complex formed in the small world of warriors who little by little became nobles. A consciousness of the superiority of a status characterized by military specialization, one that presupposes respect for certain moral precepts, the practice of certain virtues; the associated idea that social relations are organized as a function of companionship in combat; notions of homage, of personal dependence, now in the foreground, replacing all previous forms of political association.<sup>37</sup>

Definitions of feudalism abound, and student and scholar have available to them broad ones that lump together numerous facets of medieval society and narrow ones that center on carefully chosen aspects of that society—tenurial, political, military, and psychical. The possibilities for bewilderment and dispute are dizzying, particularly since a single author's interpretation of the term can undergo marked shifts.

Another difficulty posed by feudalism and its system is the fact that those employing the terms, in whatever sense they use them, are constantly found qualifying and limiting the extent to which they believe them applicable to any particular time and locality in medieval Europe. Marc Bloch writes,

In the area of Western civilization the map of feudalism reveals some large blank spaces—the Scandinavian peninsula, Frisia, Ireland. Perhaps it is more important still to note that feudal Europe was not all feudalized in the same degree or according to the same rhythm and, above all, that it was nowhere feudalized completely.

Nostalgically, and with regret only a confirmed Platonist could harbor, he concludes, "No doubt it is the fate of every system of human institutions never to be more than imperfectly realized."<sup>38</sup>

While Robert S. Hoyt could write of the growth and development of feudalism and could state that by the mid-eleventh century "an essentially feudal society had emerged throughout western continental Europe," he felt obliged, first, to deny that there was a "'feudal system' common to all Europe," and second, to assert that "there were endless diversity and variety."<sup>39</sup> In the introduction to *Feudalism* Ganshof notes that he proposes

monarchique" and coincided with the development of a new sort of warfare and the establishment of a new conception of peace; he discusses "un système économique que l'on peut, en simplifiant, appeler féodal"; he concludes that "au plan de l'économie, la féodalité n'est pas seulement la hiérarchie des conditions sociales qu'entend représenter le schéma des trois ordres [elsewhere described as le clergé, les spécialistes de la guerre, et les travailleurs]", c'est aussi—et d'abord sans doute—l'institution seigneuriale" (pp. 179, 184, 185, 187, 191). Thus, on the economic plane, Duby substitutes the development of the lordship for the coherent network of dependencies that he stressed in 1953.

<sup>37</sup> Georges Duby, "La Féodalité? Une mentalité médiévale," *Annales: Économies, Sociétés, Civilisations*, 13 (1958): 766. See also the comments of J. M. Wallace-Hadrill in a review of Bloch's *Feudal Society*, in *English Historical Review*, 78 (1963): 117.

<sup>38</sup> Bloch, *Société féodale*, 2: 248, 249 (*Feudal Society*, 445).

<sup>39</sup> Robert S. Hoyt, *Europe in the Middle Ages* (2d ed.; New York, 1966), 190–96, 185.

to study feudalism mainly as it existed in France, in the kingdom of Burgundy-Arles and in Germany, since in these countries its characteristics were essentially the same, and to concentrate on the regions lying between the Loire and the Rhine, which were the heart of the Carolingian state and the original home of feudalism. Further afield, in the south of France and in Germany beyond the Rhine, the institutions that grew up are often far from typical of feudalism as a whole.<sup>40</sup>

In his foreword to the book, F. M. Stenton praises Ganshof's self-imposed limitations and suggests that they result from a realization "that social arrangements, arising from the instinctive search for a tolerable life, vary indefinitely with varieties of time and circumstance." While it is easy to agree with Stenton that students should be disabused of the idea that "an ideal type of social order" dominated Western Europe, it comes as something of a shock to find him readily accepting the doctrine that in the huge area on which Ganshof focuses a single "classical feudalism" was to be found.<sup>41</sup> The expectation of infinite variety in social arrangements seemingly ends for Stenton at the Loire and the Rhine, a good safe distance from the Thames.

The variety of definitions of feudalism and the limitations imposed on their relevance are confusing. Equally disconcerting is the pervasive tendency on the part of those who use the word to personify, reify, and to coin two words, occasionally "bacterialize," and even "lunarize" the abstraction. How often does one read that feudalism, like a virus, spread from one area to another, or that, later on, it slowly waned. In a single study feudalism is assigned a dazzling array of roles. It is found giving birth, being extremely virile, having vitality, being strong, knowing a long tradition, being successfully transplanted, surviving, being replaced, teetering, being routed, declining and falling, and finally dead and in its grave. Another author sees it destroying the Frankish Empire and making a clean sweep of outmoded institutions. For another it makes onslaughts on the power of the kings of France and England; "les forces féodaux" end the confusion of spiritual and temporal authorities. Still another work reassuringly attributes a home to feudalism, which is said to have exercised, rather adventurously, "paralyzing action" over "many forms of royal activity," and, more decorously, to have been "introduced into England in its French form" by the duke of Normandy.<sup>42</sup> In concluding *Seigneurie et féodalité* Boutruche in fact triumphantly proclaims it madness to consider feudalism an abstraction. "In

<sup>40</sup> Ganshof, *Feudalism*, xvii.

<sup>41</sup> Stenton, foreword to *ibid.*, vii-viii.

<sup>42</sup> See Bryce D. Lyon, *From Fief to Indenture: The Transition from Feudal to Non-Feudal Contract in Western Europe* (Cambridge, Mass., 1957), 272-73; Georges Duby, *Adolescence de la Chrétienté occidentale, 980-1140* (Geneva, 1967), 61, 83. The corresponding pages in the English edition—translated by Stuart Gilbert and entitled *The Making of the Christian West, 980-1140* (Geneva, 1967)—are 61, 83. See also Bloch, *Feudal Society*, 59, 142, 443, where the statements found in *Société féodale*, 1: 95, 221 and 2: 245, are sometimes given a rather free interpretation. Finally, see Ganshof, *Feudalism*, xvii, 54, 59, 61.

actuality, it is a person. . . . Feudalism is medieval. . . . It is the daughter of the West."<sup>43</sup>

Another problem is the inclination to employ the idea of fully developed, classical, or perfectly formed feudalism as a standard by which to rank and measure areas or societies. Territories are regularly divided into categories: some highly or thoroughly feudalized; others never, gradually, or only partly feudalized.<sup>44</sup> Non-European countries are evaluated in this manner, and the standard has often been applied to Japanese modes of social and political organization.<sup>45</sup> Such assessments can also be made of institutions. The Church in Norman Italy, for instance, has been judged "never feudalized to the same extent as . . . the Church in Norman England."<sup>46</sup>

These examples all involve inanimate phenomena, geographical or institutional, but it is also possible to attribute to an individual or a group the aim of achieving complete feudalization or of introducing an articulated feudal system and then judge the person or group a success or failure in achieving this hypothesized objective. The precise nature of the goal would naturally depend on how the historian making the attribution defined feudalism or feudal system, but such assessments immediately imply that the person or group in question consciously planned and then attempted to implement a system based primarily on the granting of fiefs but also involving the establishment of a graded hierarchy of status and command and the delegation of sovereign power. D. C. Douglas transposes feudalism from the realm of the abstract into a concretely human framework when he says that in England William the Conqueror "was concerned to establish a completed feudal organization by means of administrative acts" and when he indicates that the conquest of England enabled William to realize the "feudal organization in Normandy." Before 1066, Douglas says, the Normans were "as yet unorganized in any rigid feudal scheme," the feudal structure "had not yet been fully formed," "the structure of Norman society had [not] as yet been made to conform to an ordered feudal plan."<sup>47</sup> A

<sup>43</sup> "La féodalité est présentée parfois comme une abstraction. Folie! En vérité, c'est une personne. . . . La féodalité est médiévale. . . . Elle est fille de l'Occident." Boutruche, *Seigneurie et féodalité*, 297.

<sup>44</sup> Lyon, *From Fief to Indenture*, 23-24; Joseph R. Strayer, "The Development of Feudal Institutions," in Marshall Clagett, Gaines Post, and Robert Reynolds, eds., *Twelfth-Century Europe and the Foundations of Modern Society* (Madison, 1961), 79, reprinted in Strayer, *Medieval Statecraft*, 78-79.

<sup>45</sup> Bloch, *Société féodale*, 2: 250-52 (*Feudal Society*, 446-47); Strayer, "The Tokugawa Period." For hesitations expressed by Ganshof and by Bloch himself concerning the validity of this approach, see Ganshof, *Feudalism*, xv-xvi; Bloch, *Société féodale*, 2: 242 (*Feudal Society*, 441), and *Apologie*, 89 (*Historian's Craft*, 175-76).

<sup>46</sup> Douglas, *Norman Achievement*, 176.

<sup>47</sup> Douglas, *William the Conqueror*, 281, 98, 283, 96, 104; see also note 32 above. See the more convincing analysis presented by Strayer in his review of Fauroux's *Recueil des actes*, 609-10. Like Douglas, however, Strayer concludes that although "Norman feudalism of the classic type was not fully developed until the second half of the eleventh century . . . it was William the Conqueror, more than any other ruler, who gave it definitive form." Note the warnings given by Richardson and Sayles against assuming that William had any "grand designs or well devised plans." *Governance of Mediaeval England*, 71. For a clearly integrated account

similar transformation of abstract model into consciously held goal occurs as Christopher Brooke asserts that "only in the Norman and the crusading states, colonized in great measure from the homeland of French feudalism, did one find any attempt to live up to a conception of feudalism as coherent as that of northern France."<sup>48</sup>

APPRAISING IN TERMS OF AN IDEAL STANDARD need not involve making value judgments, but such assessments are ordinarily expressed in value-loaded terms. To say that a person or a group is attempting to live up to or realize a standard certainly suggests virtuous dedication on the part of the people in question. To declare that a country which is not feudalized is lagging behind is to indicate that the area is in some sense backward. Even more evidently evaluative are such expressions as decayed, decadent, and bastard feudalism, all of them implying a society's failure or inability to maintain pure principles that were once upheld.<sup>49</sup> One is occasionally struck by a rather sentimental regret that the societies, individuals, and groups which might have been encouraged by high marks to persevere or shamed by low ones into exerting an additional push are unable to benefit from them. Even if formulated in value-free terms, analyses of societies on the basis of their conformity to or deviation from a norm offer little insight into the societies themselves, however much the process of comparison may stimulate and challenge the ingenuity of historians. To produce helpful insights, comparative history must involve the examination of the widest possible range of elements, not those idiosyncratically dubbed essential by the historians devising the standard to be applied.

Asserting that individual rulers actively and consciously aimed at establishing feudalism and judging them in terms of this aim is, at another level, equally misconceived and misleading. That William the Conqueror, the Normans, and the Crusaders wanted to establish control within the areas they conquered as effectively as circumstances permitted is, I think, unquestionable; that they used and molded the institutional forms and arrangements with which they were familiar and which were available to them is equally undeniable. To suggest, however, that they operated on the basis of a definite, preconceived scheme focused primarily on the fief, and to measure their accomplishments by such a standard, is to give a distorted, simplistic picture of their actions and policies, projecting into the minds of people who dealt creatively and flexibly with numerous options and who

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of William's accomplishments—which only once mentions the adjective "feudal"—see D. C. Douglas, "William the Conqueror: Duke and King," in Dorothy Whitelock, D. C. Douglas, C. H. Lemmon, and Frank Barlow, *The Norman Conquest: Its Setting and Impact* (London, 1966), 45–76; see p. 65 for "feudal."

<sup>48</sup> Christopher Brooke, *Europe in the Central Middle Ages, 962–1154* (New York [1963]), 100.

<sup>49</sup> See the comments of K. B. McFarlane, "'Bastard Feudalism,'" *Bulletin of the Institute of Historical Research*, 20 (1943–45): 161–62.

manipulated a variety of institutional devices to achieve their purposes a degree of calculation, narrowness of vision, and rigidity that the surviving evidence does not suggest characterized them and in which even a contemporary management specialist might have difficulty believing.

What of the other virtues attributed to feudalism as a means of comprehending medieval social and political life? As far as pedagogy is concerned, students should certainly be spared an approach that inevitably gives an unwarranted impression of unity and systematization and unduly emphasizes, owing to the etymology of the word, the significance of the fief. Even if historians agreed to define feudalism as feudal society and included within its scope all facets of social and political development, the practical problem would remain. There are other, more basic, disadvantages. To advocate teaching what is acknowledged to be deceptive and what must later be untaught reflects an unsettling attitude of condescension toward younger students. Furthermore, not only does such a procedure waste the time of teacher and student, but its supporters apparently disregard the difficulty of, as a student of mine puts it, " 'erasing' an erroneous concept or fact from the mind of a child who has been taught it, mistakenly or intentionally, at a lower school level." This student, Marie Heinbach, who teaches social studies in a New York junior high school, goes on to point out that "the difficulty becomes almost insurmountable when the amazing retentive powers of a young and impressionable child are considered. In addition, as the amount of time between the learning and unlearning of a concept increases, it becomes nearly impossible totally to correct the misconceptions that a student may have."<sup>50</sup> Experts who knowingly mislead their students appear to be unsure of their own ability to present a simplified account of the conclusions concerning medieval society that historians have now reached. Those of their students who do not progress beyond the introductory stage are denied the knowledge that most medieval historians study the actions and interrelationships of human beings rather than concentrating on the formulation and refinement of definitions of abstractions. Such students are never exposed to the problems of social and family structure and their corresponding etiquettes or to the problems of territorial loyalties and group attachments that historians are now examining. Presented with an abstract model and sternly cautioned against assuming its general relevance and applicability, only the staunchest will be motivated to pursue the individuals and groups lurking behind and beyond the ism.

For scholars the approach has equally little use. Applying an artificially fabricated standard in which certain components are divorced from the context in which they existed is essentially sterile. And those who investigate the workings of medieval society run the risk of having their vision narrowed, their perspective anachronistically skewed, and their receptivity

<sup>50</sup> This statement was made in an examination submitted on March 27, 1974.

to divergent data consequently blunted unless they firmly divorce themselves from the preconceptions and sets associated with the oversimplified models and abstractions with which they have been indoctrinated and which they themselves pass on to their students.

What of the indispensability of feudalism? Here a distinction must be made. While the creation of intuitive abstractions and simple Ideal Types can indeed be explained by invoking the infinite and confusing variety of human experience, it is quite another matter to suggest that the procedure is obligatory, necessary, or laudable. Alternative modes of classifying and describing exist and can be used. Again, attempting to justify the formulation and use of such models and abstractions by maintaining that scholarly and scientific terminology and common usage assume their existence is patently circular, avoiding as this argument does the obvious fact that scholarly terminology can be revised and common usage clarified. Far more appropriate to express regret and to apologize for measures attributable to the weaknesses and defects of human modes of expression and perception. Historians and social scientists can, like natural scientists, devise multifactor, heuristic models that encompass and account for the available evidence, are reformulated to include newly discovered data, and are not misleadingly labeled so as to suggest either system and conscious organization where none existed or the predominant importance of one element in a situation in which many elements are known to have been significant.<sup>51</sup> Such multifactor models and descriptive, narrative accounts, which emphasize complexity and the unique, can convincingly be said to encourage fuller, less distorted, and hence more acceptable understanding of the past than any "one-sided accentuation of one or more points of view."<sup>52</sup>

The contention that such general terms as "feudalism" are essential for intelligent discourse is also debatable, and those who advance this defense reveal their own discomfiture when they invoke other commonly used abstractions, such as "war" and "agriculture," to serve as buttressing middle elements. Intelligent discourse devoid of general abstract terms is, the argument runs, inconceivable. All abstractions—feudalism, war, agriculture—are similar in nature. Therefore the isms are indispensable if intelligent discourse is to occur. This chain of reasoning is, however, flawed in its second step, for there is an evident difference between, on the one hand, those collective descriptive abstractions arrived at by isolating common features of different phenomena similar enough to permit the use and assure the acceptance of single words to denote them, and, on the other

<sup>51</sup> See, for the natural sciences, N. R. Hanson, *Observation and Explanation: A Guide to Philosophy of Science* (New York, 1971), 77–84; T. S. Kuhn, *The Structure of Scientific Revolutions*, vol. 2, no. 2 of the *International Encyclopedia of Unified Science* (2d ed.; Chicago, 1970), 100–02; George Gamow, *Thirty Years That Shook Physics: The Story of Quantum Theory* (Garden City, 1966), 155; and James D. Watson, *The Double Helix* (New York, 1969), 18, 38, 47, 49, 61, 83, 123.

<sup>52</sup> Max Weber, *The Methodology of the Social Sciences* (Glencoe, 1949), 90, quoted and discussed in Lane, *Introduction to Structuralism*, 25.



hand, those abstract analytic constructs formulated and defined as a short-hand means of designating the characteristics that the observers consider essential to various time periods, modes of organization, movements, and doctrines. To a degree to which the first type is not, the second sort of general term is inevitably and often intentionally affected by the theories and assumptions of the formulators and users. Disagreements over the exact meaning of "war" or "agriculture" do occur, but they can ordinarily be resolved by introducing greater precision and clarity into the definitions of the terms, whose core signification is not generally contested. In distinction, infinite disagreement about the meanings of the isms is possible and perhaps inevitable, since the terms were not devised to designate the basic elements of fundamentally similar classes of phenomena but rather to refer to selected elements of complex phenomena, the choice of which inevitably involves the idiosyncratic value judgments of the terms' inventors and employers. Thus, however easy it is to say what the words "fief," "capital," and "merchant" mean, it is another thing entirely to seek consensus on the definitions of "feudalism," "capitalism," and "mercantilism," precisely because of the subjective nature of the definitions of these words. To raise the level of discourse and make it truly intelligent, there should be general agreement to consider the isms no more than the artificialities they are.

DIRECT EXPRESSIONS OF DISCONTENT with the term "feudalism" have increased in number and strength over the past two decades. From time to time there has seemed reason to hope that, with a resounding whoop, historians would join together, following the example of the National Assembly, to annihilate the feudal regime and, with the good members of the Legion of Honor, agree "to combat . . . any enterprise tending to reestablish it."<sup>53</sup> At least partly responsible for the mounting volume of protest is the reorientation of perspective that took place in 1953 with the publication of two remarkable books, one French and one English, both dealing with the political and social life of Western Europe in the tenth through the twelfth centuries, both concerned with individuals rather than abstractions, and both avoiding the medieval isms.

Of these books the purest—in that it does not, as far as I can tell, contain the word "feudalism"—is Richard W. Southern's study, *The Making of the Middle Ages*. In a section devoted to "The Bonds of Society" Southern presents an illuminating introduction to the political life of the eleventh and twelfth centuries by concentrating on a single, "unusually instructive" example of "what happened where the control exercised by the past was least effective, and where the disturbing elements of trade, large

<sup>53</sup> Bloch, *Société féodale*, 1: 2-3 (*Feudal Society*, xvii); see also Boutruche, *Seigneurie et féodalité*, 20-21.

towns and active commercial oligarchies were not conspicuous." Discussing the emergence of the county of Anjou, Southern uses such abstract terms as "the disintegration of authority" and "the shaping of a new political order." He writes, generally, of "an age of serious, expansive wars waged by well-organized and strongly fortified territorial lords." The term "feudal" is sometimes used in a general sense, in contexts in which it clearly implies more than connection or involvement with the fief. When the term is given this broader meaning, however, it seems to be so used out of force of habit rather than from any conscious conviction that it is the most appropriate and meaningful word to be found. "The art of feudal government" and "the early feudal age," neither phrase explicitly defined by Southern, are reminiscent of Bloch's *La Société féodale*, a book Southern recommends, and they strike a jarring note of vagueness and imprecision in a discussion otherwise notable for its concreteness. On the few other occasions when Southern employs the term "feudal" in this general way, alternative expressions that he devises to describe the phenomena in question are strikingly more informative. "Knightly" is one of these alternative terms, and, on a more extended scale, "the straightforward feudal-contract view of society" is far less subtle and suggestive than his evocative description of an "imagination . . . circumscribed by the ties of lordship and vassalage, by the recollection of fiefs and honours and well-known shrines, by the sacred bond of comradeship."<sup>54</sup>

Only a small portion of Southern's book is devoted to social and political ties and the exercise of governmental power, but Georges Duby, in his study of the Mâconnais in the eleventh and twelfth centuries, dedicates an entire volume to these subjects. Hence it is all the more noteworthy that in his index, as in Southern's, there is no reference to *féodalité*, although the index does list the indisputably acceptable terms *feudataire*, *fidèle*, *fidélité*, and *fief*, which are derived from and accurately reflect the terminology and usage of the eleventh and twelfth centuries.<sup>55</sup> Duby's avoidance of the term *féodalité* is consistent with his avowed purpose in writing his book. In his preface he announces that he is studying a small province in order to approach human beings directly, without isolating them from their milieu.<sup>56</sup> This he does, describing first the state of society in the Mâconnais at the end of the tenth century, then the period of independent castellánies from

<sup>54</sup> Richard W. Southern, *The Making of the Middle Ages* (New Haven, 1953), 90-91, 80-81, 87, 86, 262, 55, 241. When Southern mentions "the straightforward feudal-contract view of society," he associates the term "feudatory" with the "holding [of] land in return for military service" (p. 55); see also p. 56 for a reference to "the formula of feudal government" and p. 242 for "feudal custom" and "feudal etiquette"; for "knightly ideal" see p. 241; see also pp. 55, 243.

<sup>55</sup> Duby, *Société aux XI<sup>e</sup> et XII<sup>e</sup> siècles*, 666 (reprint, 501).

<sup>56</sup> "J'ai volontairement conduit mes recherches dans le cadre étroit d'une petite province. La méthode des monographies régionales permet en effet d'approcher directement les hommes sans les isoler de leur milieu." *Ibid.*, ix (reprint, 7). In his conclusion, Duby again describes his approach: "Pour approcher de plus près les hommes, nous avons concentré notre attention sur une toute petite région" (p. 644 [reprint, 482]).

980 to 1160, and finally the movement between 1160 and 1240 from castellany to principality. His conclusions are significant, first because of the wealth of data on which they are founded but even more because the Mâconnais lies within—if at the southern extreme of—the area between the Loire and the Rhine where countless scholars have seen “classical feudalism” emerging, and also because its history does not exemplify the characteristics associated with this development.

Stressing the survival of comital power and superiority until the end of the tenth century, Duby shows that among the higher ranks of society the ties of fidelity linking those agreeing to some sort of mutual support were vague and imprecise, like family ties, and can best be described as confirming a relationship of *amicitia*. As the count's power declined and as that of the castellans increased, bonds of dependence among the higher classes became more important, and grants of land were used to solidify the ties until by 1075 land outweighed loyalty as their determinant. Obligations were still indefinite, however, and military service was not a significant component. Between men of unequal status, dependent relationships were closer, but the strength and meaning of these ties were limited by the small value of the fiefs that lords gave their followers, who generally possessed large allodial holdings, and by the multiplicity of the ties. According to Duby, “feudal institutions”—by which he apparently means not only fiefs but also homage and vassalage—had only superficial importance. They constituted a sort of superstructure that formalized without affecting pre-existing relationships.

Feudal institutions were adapted to the previous structure of the higher class without significantly modifying it. Between great lords or knights, homage is a simple guarantee, an agreement not to harm; between a small noble and a powerful one, it is a true dedication, an agreement to serve. Vassalage and the fief, customary practices born in private usage, organized the relations that unequal division of wealth and power had already determined; they created no additional ones. In eleventh-century Mâconnais, there was no pyramid of vassals, there was no feudal system.<sup>57</sup>

Duby concludes that for the higher classes “feudalism was a step toward anarchy,” but by this he evidently means not that any ill-conceived and abortive attempt had been made to create harmony by introducing homage, vassalage, and the fief, but that the links ordered by these institutions were not strong or meaningful enough to serve as effective restraints. These were instead provided by the teachings and intervention of the Church, by family bonds, and by a variety of oaths. Thus, “although violent and disturbed, the world of lords was not anarchic.”<sup>58</sup> In this period the nobility exercised

<sup>57</sup> *Ibid.*, 94–116, 140–41, 172, 177–85, 194–95, see also 185–93, 291 (reprint, 93–108, 124–25, 149, 153–58, 164–65, see also 158–64, 235–36).

<sup>58</sup> *Ibid.*, 204 (reprint, 170). For a full discussion of these restraints see pp. 196–204 (reprint, 165–70).

for their own benefit governmental powers over the lower classes, but their actual control over land did not increase.<sup>59</sup>

In the late twelfth and early thirteenth century the economy of the Mâconnais was transformed, and the king of France, long absent from the area, reappeared there. Economic pressures and royal policy produced a proliferation of ties of dependence and a marked decrease in allodial holdings; concomitantly, services may have become more definite and heavier. As far as justice was concerned, "the peace of the prince replaced the peace of God," and judicial procedures developed in the eleventh century were regularized and made more effective.<sup>60</sup>

Duby occasionally uses the word *féodalité*, but the term has no central significance in his book, thanks to his determination to focus on individuals and their actions. In his general conclusion he relates his findings to his own definition of feudalism, which, as has been seen, involves the disintegration of central authority and the development of an inclusive web of dependencies. In the Mâconnais, he reminds his readers, these two characteristics appeared successively rather than simultaneously, since in the eleventh and twelfth centuries, when most lands were freely held, jurisdictional powers were in the hands of private lords, and in the thirteenth century, when most lands were involved with dependent relationships, sovereignty reappeared in the persons of kings and princes.<sup>61</sup> Duby refuses to comment on the districts outside the Mâconnais, and he calls for additional local studies. Nonetheless he notes that "the society of the Mâconnais did not evolve in isolation." Pointing out that the Mâconnais was "a province of feudalism with marked individual characteristics,"<sup>62</sup> he implicitly suggests that other areas lying within the fabled heartland of feudalism were equally distinctive.

Duby does not openly attack the use of the concept of feudalism, nor does he denounce the idea that institutions in the Loire-Rhine region were similar enough to be described as a single phenomenon. Still, his conclusions demonstrate the futility of generalizations that are not based on the study of successive generations of human beings inhabiting a restricted area. They also suggest the inappropriateness of descriptive terms that fail to convey a sense of the variety of experience and development to be found throughout Western Europe between the tenth and the late twelfth centuries. When I once asked Monsieur Duby what difference there was between his book on the Mâconnais and Ganshof's study of feudalism, he

<sup>59</sup> *Ibid.*, 329-30 (reprint, 261-62). For the close connection Duby now posits between the development of the ideology of the peace of God and "les premiers phases de la féodalisation," see note 36 above and Duby, *Guerriers et paysans*, 185.

<sup>60</sup> Duby, *Société aux XI<sup>e</sup> et XII<sup>e</sup> siècles*, 473-569, 571 (reprint, 361-427, 429).

<sup>61</sup> Using Bloch's periodization, Duby concludes that only in this sense could there be said to have been "two feudal ages." The second age—a time of fiefs, censives, and feudal principalities—contrasted with the earlier age of independent castellanies, and Duby believes that it began no earlier than 1160 and that it ended in 1240. *Ibid.*, 642-43 (reprint, 481-82):

<sup>62</sup> *Ibid.*, 644 (reprint, 482).

replied with a modest shrug of the shoulders, "Toute la différence du monde, Madame." His own book is a testimony to his conviction that understanding the workings of medieval society necessarily involves exploring the intricate complexities of life rather than elaborating definitions and formulas designed to minimize, simplify, and, in the last analysis, obscure these complexities.<sup>63</sup>

SOUTHERN AND DUBY had their predecessors—historians who probed beyond or disregarded the construct feudalism and who concentrated on analyzing and describing the many different ties and modes of dependency binding human beings to one another. Unquestionably, the work of Duby and Southern has acted as an additional, powerful stimulus, prompting more scholars to study the actual functioning of society in different areas. In general, however, and certainly in works directed at a popular rather than a scholarly audience, the situation remains much the same as it has been, and there is virtually universal resistance and opposition to abandoning the term "feudalism" and to confining the word "feudal" to its narrow sense—"relating to fiefs." The reservations regarding the use of the generalized constructs implicit in the books of Southern and Duby have not yet had the widespread effect that might have been hoped.

Exceptions do, of course, exist. In the books he has published since 1953 Southern has consistently employed his brilliant descriptive techniques and has assiduously avoided the term "feudalism."<sup>64</sup> R. H. C. Davis is now following a similar path, having apparently undergone something of a conversion. In the history of medieval Europe that he wrote in 1957 the word "feudalism" occasionally appears. England after William's conquest is called "the best and simplest example of a feudal monarchy." The index refers readers wishing to learn about "fully-developed feudalism" to pages Davis evidently considers relevant to this subject. How refreshing, then, to turn to an article

<sup>63</sup> See, however, his more restrained comments in "La Féodalité?" 765-66. Duby recommended Ganshof's study of feudalism as a guide and reference work but suggested that the very clarity, simplicity, and Cartesian rigor which are among its chief virtues may give the reader a false impression of order and regularity.

<sup>64</sup> See his book *Western Society and the Church in the Middle Ages* (Harmondsworth, 1970). Under the circumstances it is not difficult to forgive him for translating the word *homo*, which literally means no more than "man," as "vassal" in his edition of the *Vita Anselmi: The Life of St Anselm, Archbishop of Canterbury, by Eadmer* (Edinburgh, 1962), 111. Southern recently told me that he thinks "deplorable" not only the term "feudalism" but also the words "humanism" and "scholasticism." He said that he had never knowingly used the word "feudalism" to refer to actual conditions in the Middle Ages. He offered, however, a tentative and qualified defense of the word in *Medieval Humanism and Other Studies* (Oxford, 1970), 29. Southern's work suggests that he thinks the words "humanism" and "scholasticism" may have some practical value, however defective he may judge them on a theoretical plane. Medieval humanism is the central subject of his collected essays, and in a lecture, "The Origins of Universities in the Middle Ages," given at Philadelphia on April 8, 1974, Southern emphasized the importance of "scholasticism" and "scholastic" thought, calling the universities "the power house of scholasticism." In his conclusion, however, he warned that "European scholastic development" should be envisioned not as a single whole but as marked by diversity and variety.

on the Norman Conquest written ten years later and to find there a convincing analysis of William's accomplishments that contains no reference to the ism or its associated forms.<sup>65</sup>

Southern and Davis are unfortunately in a minority. Far more numerous are the scholars who, while attacking the concept feudalism, still use the term and even encourage its propagation by suggesting new and better definitions. The contradictions in the work of Richardson and Sayles have already been discussed. Fully as puzzling is the case of Duby himself. Having implicitly questioned the aptness of the term in his study of the Mâconnais, he proceeded in 1958 not only to employ it but also, as has been seen, to advance an alternative definition, unusual and idiosyncratic, which he appears subsequently to have rejected. In a still later work, directed at a less scholarly audience, Duby employs the term *féodal* which, while undefined, clearly refers to something more general than the fief. It is found modifying such nouns as *éparpillement*, *forces*, *cours*, *princes*, and *seigneur*; a section of the book is entitled "Les féodaux," and the construct feudalism is several times personified.<sup>66</sup> A popular work published in 1973 shows that Duby's dedication to and reliance on the term have, in recent years, simply increased. He repeatedly refers to *féodalité* and uses the adjective *féodal* in a vague, indefinite way, and he goes so far as to designate the period from the mid-eleventh to the late twelfth century "les temps féodaux."<sup>67</sup> Saying that feudalism was characterized by the disintegration of monarchical authority and associating it with the institutions of the *seigneurie*, Duby presents feudalism as being implanted and established; he refers to feudalization, a feudal epoch, feudal society, feudal Europe, feudal peace, feudal structures, and a feudal economy and economic system.<sup>68</sup>

Striking inconsistencies appear in Christopher Brooke's five-page discus-

<sup>65</sup> R. H. C. Davis, *A History of Medieval Europe from Constantine to Saint Louis* (London, 1957), s.v. "feudalism" in the index, and see also p. 127, where he enclosed the term "feudalism" in quotation marks; and see pp. 295, 414. Davis, "The Norman Conquest," *History*, 51 (1966): 279-86, reprinted in C. W. Hollister, ed., *The Impact of the Norman Conquest* (New York, 1969), 123-33.

<sup>66</sup> Duby, *Adolescence de la Chrétienté occidentale*, 60-61, 84. The translation of this book exaggerates these tendencies: see the corresponding pages, *Making of the Christian West*, 61-62, 83-84, and note that the section Duby entitled "Les féodaux" is called "Feudalism" in the translation. In *L'économie rurale et la vie des campagnes dans l'Occident médiéval* (Paris, 1962), Duby may refer to "la seigneurie des temps féodaux" (p. 379), but he generally avoids the term, and *féodalité* is not listed in the index. Note, however, that in the translation of the book published in 1968, "temps féodaux" becomes "the feudal period," and "rente seigneuriale" is transformed into "feudal rent." *Rural Economy and Country Life in the Medieval West*, tr. Cynthia Postan (London, 1968), 171, 232-59.

<sup>67</sup> Duby, *Guerriers et paysans*, 179-204, and see note 36 above.

<sup>68</sup> See also *ibid.*, 194 ("l'implantation de la féodalité"), 262 ("l'établissement de la féodalité"), 184 ("l'établissement des structures féodales"), 185 ("la féodalisation"), 278 ("l'époque féodale"), 192 ("la société féodale"), 201 ("l'Europe féodale"), 300 ("la paix féodale"), 184 ("les structures féodales"), 189 ("l'économie féodale"), 187 ("un système économique que l'on peut, en simplifiant, appeler féodal"). It is heartening to note that the review of Duby's *Guerriers et paysans* in the *Times Literary Supplement* does not contain the words "feudal" or "feudalism." Aug. 17, 1973, pp. 941-42.

sion of barons and knights in a book he published in 1963. Having begun by declaring that "few historical labels are more ambiguous than 'feudal' " and by proclaiming that he would therefore "use it as little as possible," having then warned that "it is doubtful whether [strict feudalism] ever existed outside the imaginations of historians," he proceeds, without defining the term "feudal," to use it, imprecisely and ambiguously, in writing of "the feudal bond," "feudal conceptions," "the feudal contract," "the feudal oath," and "feudal and quasi-feudal institutions." He also refers to "highly developed" feudalism, "classical feudalism," "French feudalism," and "strict feudalism." Finally he both reifies feudalism and uses the phrase "coherent feudalism" to designate a consciously formulated and adopted set of goals and principles.<sup>69</sup>

THE HESITANCIES, CONTRADICTIONS, AND INCONSISTENCIES that have been reviewed—and that are wholly typical of statements found in the books on medieval society published in the past twenty years—clearly demonstrate how necessary it is to reassess the value of the words "feudal" and "feudalism." It must be admitted that there is little possibility of ridding the historical vocabulary of them, adopted as they have been by the scholarly community in general and by the economists in particular. The terms exist. They have been and probably will be used for many years. As words students know if they know nothing else about the Middle Ages, they cannot be avoided. But confrontation need not mean capitulation, for it is perfectly possible to instruct students at all levels to use "feudal" only with specific reference to fiefs and to teach them what feudalism is, always has been, and always will be—a construct devised in the seventeenth century and then and subsequently used by lawyers, scholars, teachers, and polemicists to refer to phenomena, generally associated more or less closely with the Middle Ages, but always and inevitably phenomena selected by the person employing the term and reflecting that particular viewer's biases, values, and orientations. Illustrations of the many meanings attached to "feudal" and "feudalism" can be given, and students with a flair for historiography can be encouraged to explore the eccentricities of usage associated with the terms.

Other students will be directed to the study of medieval society and politics, and they and their instructors will be faced with the necessity and challenge of finding an adequate means of describing the elements historians have investigated and should explore and the positive conclusions that have been reached.<sup>70</sup> Throughout, the terminology and word usage of those who

<sup>69</sup> Brooke, *Europe in the Central Middle Ages*, 95–96, 99–100. Brooke writes that "in its origin feudalism provided for the recruitment of vitally needed cavalry troops" (p. 100). See also pp. 1076–77 above. Note, too, that having just questioned the validity of the idea of "strict feudalism," Brooke scrupulously encloses "feudal" in quotation marks when he refers to "'feudal' means" of raising troops (p. 100).

<sup>70</sup> See Joseph R. Strayer, "The Future of Medieval History," *Medievalia et Humanistica*, n.s. 2 (1971): 182. Only since the appearance in 1968 of Fredric Cheyette's invaluable collec-

lived in the Middle Ages must be emphasized, and attention must be paid to the shifting meanings of key words, as well as to the gulf between actual practice and the formal, stylized records that have survived. Some elements will be pointed to as constants of general importance: the slowness and difficulty of communication, the general insecurity, the sluggish rate of technological change, and the reverence for tradition. The varying effects and significance of terrain, warfare, and violence must be emphasized. Stress must also be given to the resultant regional and diachronic variations in forms of government, modes of military organization, social and family structure, social mobility, the relationship between social class and function, styles of agricultural exploitation and commercial activity, and urban growth. Attention must be called to the different social and political relationships in which human beings were involved, to the ceremonies through which these relationships were fixed and manifested, and to the varying sorts of ties that superficially similar ceremonies could be used to create: bonds of obligation, fidelity, and support between sovereigns and their subjects, created and confirmed by oaths, pledges, and services; ties of loyalty, solidarity, and mutual assistance among people of similar and different social classes, formalized in communes, confraternities, guilds, leagues, and alliances, constituted through mutual undertakings that were sometimes left vague and sometimes clearly defined, solidified through privileges granted to and demanded by these groups; religious ties binding members of local congregations, regional churches, and similar faiths; ties of dependence forged between individuals or inherited from the past, sometimes involving friendship, sometimes service, sometimes protection, reinforced by gestures and oaths, resulting in benefits—material, monetary, territorial, social—to one or both parties; family bonds, revealed and consolidated in testamentary provisions, marriages, special festivities, and feuds and vendettas. The written and unwritten rules governing these ties and relationships must be considered, as must the ways in which and the different degrees to which these principles were systematized and enforced.

But to be properly understood, these elements must be observed as they developed, interacted, and changed, and thus the importance of presenting searching and detailed descriptions of areas characterized by different forms of governmental and social structure and organization and by different modes of development. Regions where strong monarchies developed and survived must be given as extensive consideration as areas where they disappeared, so that any given region—the Empire, England, Italy, Normandy, the Ile-de-France, the Mâconnais—will be considered neither abnormal nor typical but will be viewed as an instance of the varying ways human beings responded to similar and dissimilar circumstances, whose

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tion of translated essays, *Lordship and Community*, has it been practically possible to direct beginning students to the recent literature in which this perspective on medieval society is reflected.



impact was conditioned by the total pasts of the people they affected. Those who are introduced to the study of medieval social and political life in this way will be far less likely than those presented with definitions and monistically oriented models to be misled about the conditions of existence in the Middle Ages. They will find it difficult to contrive and parrot simplistic and inaccurate generalizations about medieval Europe, and they may be challenged to inquire into subjects and areas as yet uninvestigated and to seek solutions to problems as yet unanswered.

The unhappiness of historians with the terms "feudal" and "feudalism" is, thus, understandable. Far less comprehensible is their willingness to tolerate for so long a situation often deplored. Countless different, and sometimes contradictory, definitions of the terms exist, and any and all of these definitions are hedged around with qualifications. Using the terms seems to lead almost inevitably to treating the *ism* or its system as a sentient, autonomous agent, to assuming that medieval people—or at least the most perspicacious of them—knew what feudalism was and struggled to achieve it, and to evaluating and ranking societies, areas, and institutions in terms of their approximation to or deviation from an oversimplified Ideal Type.

Despite the examples set by Southern and Duby some twenty years ago and followed in the interim by some scholars, historians have been generally loath to restrict the term "feudal" and discard the term "feudalism," particularly in dealing with general rather than specialized audiences. Feudalism's reign has continued virtually unchallenged, with ambivalence characterizing the attitudes of most historians toward the subject. The situation, however, can and should change. The arguments advanced to defend using the terms as they have been used in the past are weak, based as they are on vaguely articulated assumptions concerning the concept's utility as a verbal and intellectual tool, as a teaching device, or as a mode of evaluation—none of which is convincingly established. Similarly unsatisfactory are justifications founded on hypothesized requirements: the historian's need, as scientist, for abstractions like feudalism; the basic demands of discourse; or necessities created by the fundamental and seemingly insurmountable limitations of the human mind. Preferable alternative perspectives and terms exist, and there seems no reason to delay channeling all available energies to the study of human beings who lived in the past, thus putting an end to the elaboration of arid definitions and the construction of simplistic models. The tyrant feudalism must be declared once and for all deposed and its influence over students of the Middle Ages finally ended. Perhaps in its downfall it will carry with it those other obdurate *isms*—manorial, scholastic, and human—that have dominated for far too long the investigation of medieval life and thought.

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## Napoleon's Prefects

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EDWARD A. WHITCOMB

IN ALL THE GREAT BODY of Napoleonic literature there exists not a single scholarly study of Napoleon's prefects. The creation of the prefectural corps was unquestionably one of Napoleon's greatest administrative achievements and certainly one of the most lasting. Napoleon's prefects governed France for fifteen years; they liquidated the Revolution; they established the basis of local administration in modern France; and they administered the largest and most successful European empire since Charlemagne. The texts of French, European, and administrative history devote a few lines or paragraphs to the prefects; the more specific studies of the period may contain a page or two on them. Yet, if one looks to the bibliography or the footnotes for a satisfactory reference on the prefects one is quickly disappointed. The accounts by J. Regnier and Jean Savant are often cited, but neither is scholarly, well researched, accurate, or documented.<sup>1</sup> A number of specific studies exist, biographies of a few of the prefects or of the administration in certain areas. These are useful in themselves but dangerous as a basis for generalization, the biographies because they concentrate on a few totally unrepresentative prefects, the regional studies because they are concentrated on the border areas of France, which are least representative of France as a whole. Nor are there sufficient local studies to permit generalization.<sup>2</sup>

The texts suggest a number of generalizations about the prefects—they were supposedly young; from a variety of backgrounds, especially the Revolutionary assemblies; from a place other than the department they administered; increasingly of noble birth; and were good administrators. These

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<sup>1</sup> J. Régner, *Les Préfets du Consulat et de l'Empire* (Paris, 1907); and J. Savant, *Les Préfets de Napoléon* (Paris, 1958).

<sup>2</sup> Sixty years ago Aulard concluded his short study of Napoleon's administration with an appeal for more regional studies. In 1965 Jacques Godechot concluded his introduction to one such study with a similar plea: "We hope that this book will be the first in a long series of similar studies." See A. Aulard, *Études et leçons sur la Révolution française, septième série* (Paris, 1913); and J. F. Soulet, *Les Premiers Préfets des Hautes-Pyrénées 1800-1814* (Paris, 1965), preface by Jacques Godechot.

popular generalizations are partly true, partly false, have never been proven, and tell us little anyway. It would be valuable to know exactly who these prefects were: their social, political, functional, and geographical backgrounds; whether they were young or old; whether French or foreign; what happened to them after their employment as prefects; and how all these factors evolved during the Napoleonic period. But most valuable of all would be a clear and authoritative answer to the significant and complicated question of the quality of the prefectural corps as an administration. The general opinion is that the prefects provided excellent administration but that the quality of the corps deteriorated, especially after 1808 or 1810. This raises immediately the question of how quality is evaluated, a matter that Napoleonic historians have hardly considered, in spite of their repeated generalizations about the subject. This study will suggest a number of criteria of administrative quality and will argue that by these criteria the Napoleonic prefectural corps gradually and consistently improved. Such a conclusion is diametrically contrary to the prevailing opinion of the vast majority of historians.

Sixty years ago Aulard, the great historian of the French Revolution, said that Napoleon recruited his prefects from the moderates of the Revolution. He went on to identify thirteen conventionnels, fifteen constituants (three of whom were also conventionnels), eight members of the Legislative Assembly, and six from the Directory (one of whom was also in the Legislative Assembly). Yet, strange to say, Aulard's arithmetic seems to be faulty, for his evidence shows that only 41 of the first 97 prefects were Revolutionary politicians. This means that a majority (58 per cent) were not Revolutionary politicians, which is a flat contradiction of his statement. Later on Aulard suggests that there was no change in recruitment under the Empire, leaving the impression that a majority of the entire corps consisted of moderates from the Revolution.<sup>3</sup> It is a significant commentary on Napoleonic historiography that these statements have gone unchallenged for half a century and have been repeated by the most notable historians of the period. Thus the best biography of Napoleon states that "they were nearly all moderate men of the Revolution."<sup>4</sup> While Aulard exaggerated the conclusion but not the evidence, some historians have vastly increased the number of Revolutionaries. Aulard found only 41 politicians, but J. Siwek-Pouydesseau claims there were 71 former politicians among the first appointees.<sup>5</sup> There is only one way this figure could be obtained, and that is by counting some prefects two or three times because they sat in more than one assembly.

Even if the evidence presented by Aulard and others were accurate,

<sup>3</sup> Aulard, *Études*, 128-30.

<sup>4</sup> F. M. H. Markham, *Napoleon* (New York, 1963), 81.

<sup>5</sup> J. Siwek-Pouydesseau, *Le Corps Préfectoral sous la troisième et quatrième Républiques* (Paris, 1969), 12. Similarly, Godechot says that there were 76 Revolutionaries among the first appointees, which would be almost 80 per cent. Jacques Godechot, *Les Institutions de la France sous la Révolution et l'Empire* (2d ed.; Paris, 1968), 387.

which it is not, it would be irrelevant, since it only applies to the first 97 prefects and not to the entire Napoleonic corps of 281; the first prefects were the most unstable, one-third of them leaving the corps within the first three years. These Revolutionary prefects have taken on a special significance since Aulard's time, partly because some historians have come to regard them as the best administrators in the corps, partly because of the exaggeration in their numbers.<sup>6</sup>

Clearly, a significant number of prefects had been Revolutionary politicians, and they have attracted a considerable amount of attention from historians. Unfortunately, a discussion of them involves some problems of definition. A careful reading of the job applications throughout the period reveals that around 1794 the applicants were ardent supporters of the Revolution. By 1799 the same people had become moderate supporters. By 1810 they claimed to have accepted the ideas of the Revolution but never its excesses; and by 1814 they had become counterrevolutionaries. Between Robespierre and the Comte d'Artois there were a million shades of difference, and the identification of Revolutionaries can become purely subjective. And if the definition of a Revolutionary has its problems, then the identification of moderates is altogether impossible. It is best to avoid the word moderate, and define a Revolutionary politician as anyone who belonged to a national assembly between 1789 and 1799.

Eighty-three of Napoleon's prefects (30 per cent) had belonged to a Revolutionary assembly.<sup>7</sup> They included 16 members of the National Assembly, 13 from the Legislative Assembly, and 19 members of the Council of Five Hundred or of Ancients. Twenty-four sat in the Convention, most of these in one or more other assemblies. In addition, five members of the National Assembly and six of the Legislative Assembly also sat in the assemblies of the Directory. If one ends the Revolution at Thermidor, as many historians do, then only 64 (22 per cent) of the prefects were Revolutionaries. We shall assume, however, that the Revolution lasted until 1799 and thus exclude only the Napoleonic assemblies from the count. Forty of these 83 politicians entered the corps in 1800, while most of the remaining ones were appointed in the early years. Naturally their numbers gradually

<sup>6</sup> See, for example, the works of such historians as George Lefebvre, Geoffroy Bruun, and Alfred Cobban.

<sup>7</sup> My evidence is based on a study of about 90-95 per cent of Napoleon's prefects and includes their place and date of birth; their social, political, and functional background; their administrative career; and their future. The remaining five per cent were the least important and served for only short periods. The evidence comes from the personnel dossiers of the prefects, series F1b1 150-178 (232 cartons) in the Archives Nationales (Paris), plus the following biographical dictionaries: *Biographie Nationale de Belgique* (Brussels, 1866); *Biographie Universelle ancienne et moderne* (2d ed.; Paris, 1843); *Dictionnaire Historique et Biographique de la Révolution et de l'Empire* (Paris, 1899); *Dictionnaire des Parlementaires français de 1789 à 1889* (Paris, 1889-90); *Galerie administrative ou biographie des préfets depuis l'organisation des préfectures jusqu'à ce jour* (Aurillac, 1839); and G. Six, *Dictionnaire biographique des généraux et amiraux français de la Révolution et de l'Empire* (Paris, 1934). Information readily available in biographical dictionaries will not be documented in the text.

TABLE 1. POLITICAL BACKGROUND: NUMBER AND PROPORTION OF REVOLUTIONARY POLITICIANS<sup>a</sup>

<i>Years</i>	<i>Number</i>	<i>Proportion</i>
1800	40	40%
1801	46	47%
1802	50	46%
1804	47	44%
1805	47	43%
1806	45	42%
1807	42	38%
1808	41	37%
1809	37	33%
1810	35	30%
1811	28	22%
1812	28	22%
1813	29	23%
1814	24	19%

<sup>a</sup> The percentages in all the following tables must be explained. The size of the prefectural corps and the number of departments were not necessarily the same, because a department was unoccupied at some times and, at other times, a new prefect would be appointed before his predecessor retired. The tables, unless otherwise indicated, are based on the number of prefects or the number about whom information is available.

decreased owing to the effects of death, retirement, promotion, transfer, or dismissal. Their proportion decreased even faster because of the expansion of the corps from 97 to 131. Table 1 gives their absolute number and proportion as of January 1 of every year, or April 1800 when the first appointments were made. Although the Revolutionary politicians constituted a significant but diminishing part of the prefectural corps, they were never in a majority.

When discussing the political origins of the prefects, writers often identify emigrés or royalists as being at the opposite end of the political spectrum from the Revolutionary politicians. Alfred Cobban tells us that Napoleon appointed more and more "secret royalists," and other historians mention royalists and emigrés in the corps.<sup>8</sup> Some of these can readily be identified. Joseph Arbaud-Joucques, the son of a *président à mortier* of the parlement of Aix, emigrated and served in Condé's army. Alexandre de Larochefoucauld and Frédéric LaTour du Pin were among the most famous of the emigrés. Benôit Capelle hastily surrendered Geneva to the Allies in 1813, organized resistance to Napoleon during the One Hundred Days, and served in the First and Second Restorations. Vincent Viennot de Vaublanc was retained by the First Restoration, defended the Bourbons in 1815, and accompanied Louis XVIII to Ghent in 1815.

It would be most useful to identify the total number, proportion, and changing strength of the emigrés and royalists. Unfortunately this is impossible. Donald Greer has pointed out that the emigrés were people of

<sup>8</sup> Alfred Cobban, *A History of France 1799-1871* (2d ed.; Harmondsworth, 1965), 2: 26.

all classes and occupations, mostly from the border areas of France, who were forced to leave for a variety of reasons.<sup>9</sup> Some of them were active counter-revolutionaries, but these political emigrés cannot be separated from the rest, and a few of them had been Revolutionaries. Some of the political emigrés did not remain loyal to the king: Jean Grégoire du Colombier was a *fédéralist*, an emigré, and a counterrevolutionary, but he supported Napoleon in 1814–15.<sup>10</sup> Also it is impossible to identify the number of royalists within the corps. In 1814 most of the prefects rallied to the Bourbons—Cobban's reason for declaring that previously they had been secret royalists—but often they changed sides only after the arrival of the Allied armies or the abdication of Napoleon. In 1815 some of them returned to Napoleon—were they secret royalists under Napoleon and secret Bonapartists under Louis XVIII? The few who in 1814 rallied to Louis before the collapse of the Napoleonic regime and subsequently followed Louis to Ghent were certainly royalists. Most prefects, however, made their decisions in 1814–15 on the basis of political calculation or loyalty to the administration and France, and not loyalty to Bourbon or Bonaparte. One example will illustrate how quickly loyalties could change and how meaningless some of these loyalties were. On June 25, 1814, a political *commissaire du roi* reported that the Napoleonic prefect Duval should be retained because he was an excellent administrator and "M. Duval is loyal; he is devoted to the royal government and he will remain faithful to it."<sup>11</sup> A royalist undoubtedly, but he went back to Napoleon in 1815. Many of the royalists of 1814 could best be classed as *ex post facto* royalists—in applying to Louis XVIII for jobs they claimed to have been royalists all along, but if Bonaparte had survived they would have been equally emphatic about their loyalty to the Empire. For these reasons it has proven totally impossible to identify accurately the political emigrés or the royalists within the prefectural corps. They must remain what Cobban said they were—secret royalists.

"THE REVOLUTION IS OVER," Napoleon declared in 1800. Then, to make sure that this was so, he set out to fuse the men of the Revolution, who were essentially bourgeois, with the men of the *ancien régime*, who were essentially noble. One method of breaking the class barriers of both Revolution and *ancien régime* was to appoint bourgeois and noble to the same institutions and make them work together. In social origins the prefectural corps was part noble, part bourgeois. This, of course, has always been known. But the important question has remained unexamined: what proportion was recruited from each class and how did these proportions change over

<sup>9</sup> Donald Greer, *The Incidence of the Emigration during the French Revolution* (Cambridge, Mass., 1951).

<sup>10</sup> F1b1 158–31.

<sup>11</sup> F1b1 158–41, fol. 21.

time. A number of historians have asserted that the proportion of nobles increased.<sup>12</sup> Some of them date this change from around 1801 or refer to an aristocratic reaction associated with the year of the Austrian marriage, 1810.<sup>13</sup> The proportion did, in fact, increase, but the assertion is highly misleading, and the extent of the change has never been estimated.

Any investigation of social origins leads to the great debate as to what constituted the nobility and what was the bourgeoisie.<sup>14</sup> Suffice it to say that there were no clear answers to these questions at the time and there are no clear answers now. Each historian is therefore obliged to define his own terms and to adopt his own methods of research. My sources show that 110 of the prefects were born into families that had been ennobled during the *ancien régime*.<sup>15</sup> A further five years of exhaustive research might add a few more names but could hardly change the conclusions. Apparently none of the prefects was recruited from the lower class—that is, the peasants or the workers. Thus the remaining 171 were middle class (neither noble nor lower class), and the middle class may be defined for working purposes as the bourgeoisie. Some of these were clearly from the haute bourgeoisie, others from the petite bourgeoisie. It is impossible, however, to divide accurately all 171 prefects into two such loose categories, so they shall remain simply the bourgeois prefects.

Thus defined, an absolute majority of Napoleon's prefects, 171 or 61 per cent, were bourgeois in origin, a significant minority, 110 or 39 per cent, were noble. The great nobles of the *ancien régime* began to rally to Napoleon immediately after the coup d'état. In the first years of the Consulate Napoleon appointed as prefects a Larochehoucauld, a Chauvelin, and Claude Brugière de Barante. Such names as Brunateau de Sainte Suzanne, Ville-neuve-Bargemont, and Barral de Rochinaud appeared around 1805. In the middle period one sees a Houdetot and a Latour du Pin entering the corps. After 1811 the families de Rambuteau and de Sainte Aulaire gain their representation, and how could Napoleon ignore someone with a name like Scipion Cyprien Jules Louis Martin Marie Elizabeth, Marquis de Nicolai? The increase in the proportion of nobles and the compensating decline in the proportion of bourgeois was a gradual one, as table 2 indicates (January 1 of every year and March 1800).

The number of bourgeois was constant; they were in a majority in the

<sup>12</sup> For example, N. Hampson, *The First European Revolution* (London, 1969); and Godechot, *Institutions*, 588.

<sup>13</sup> Cobban, *France*, 26; and Savant, *Préfets*, 170.

<sup>14</sup> N. Richardson, *The French Prefectoral Corps 1814-1830* (Cambridge, 1966), 179-244; C. B. E. Behrens, *The Ancien Régime* (London, 1967); and Alfred Cobban, *The Social Interpretation of the French Revolution* (Cambridge, 1964).

<sup>15</sup> The principal genealogical dictionaries consulted to establish a list of nobles (in addition to other dictionaries and sources) include Chais d'Est Ange, *Dictionnaire des familles françaises anciennes ou notables à la fin du XIX<sup>e</sup> siècle* (Evreux, 1903-29); Henri Jouglu de Morenas, *Grand Armorial de France* (Paris, 1934-52); A. Révérend, *Armorial du premier Empire* (Paris, 1894-97); and A. Révérend, *Les familles titrées et anoblies au XIX<sup>e</sup> siècle* (Paris, 1901-06).

corps from first to last. The proportion of nobles increased because they received the positions created by the expansion of the Empire and not because they replaced bourgeois prefects. The real influx of nobles occurred in the first seven years of the regime. From 1807 to 1814 the proportion of nobles increased by only five per cent, an insignificant change that makes nonsense of the assertion that nobles were flocking to the corps in 1808 or 1810 or 1812. The aristocratic reaction, if ever there was one, occurred under the Consulate. What is true is that the nobles came to occupy a disproportionate number of the more important departments. In 1801 a larger proportion of bourgeois than of nobles were in the first-, second-, and third-class depart-

TABLE 2. SOCIAL BACKGROUND: NUMBER AND PROPORTION OF BOURGEOIS AND NOBLE PREFECTS

<i>Years</i>	<i>Number of Bourgeois</i>	<i>Proportion of Bourgeois</i>	<i>Number of Nobles</i>	<i>Proportion of Nobles</i>
1800	77	77%	23	23%
1801	74	75%	25	25%
1802	78	72%	30	28%
1803	74	69%	33	31%
1804	74	68%	35	32%
1805	76	70%	32	30%
1806	71	66%	37	34%
1807	69	63%	41	37%
1808	67	61%	43	39%
1809	73	64%	41	36%
1810	67	58%	48	42%
1811	74	59%	52	41%
1812	73	57%	54	43%
1813	74	58%	52	41%
1814	71	57%	53	43%

ments; by 1814 over half of the nobles occupied prefectures of the first three ranks. And, in 1812, the Seine or Paris passed from bourgeois to noble hands.

This proportion and concentration of noble prefects are sufficient to raise another question about the Napoleonic regime. Eugene Tarlé, the Soviet-Marxist historian, flatly states that the Napoleonic regime functioned in the interests of the bourgeoisie,<sup>16</sup> an idea accepted by various historians, including George Lefebvre. A definitive conclusion would require exhaustive study of the policies followed by the individual prefects, but it would be curious indeed to discover that the 110 nobles administered their departments exclusively or even mainly in the interests of the bourgeoisie. What is more logical, and what conforms to the evidence, is that the attempt to impose a class-conflict theory on the Napoleonic regime, or at least on the administration of that regime, is fallacious, misleading, and pointless. What is more likely

<sup>16</sup> Eugene Tarlé, *Napoléon* (3d ed.; Moscow, 1937), 6-7, 9, 110-11, 224, 319-22.



is that the Revolution was indeed over and that Napoleon's attempt to fuse the bourgeoisie of the Revolution with the nobility of the *ancien régime* was largely successful. Within the prefectoral corps one finds noble and bourgeois working together and working with ministers, subprefects, advisory councils, and the people of all classes. One finds a bourgeois prefect such as Antoine Thibadeau recommending a noble subprefect such as Arbaud Joucques for promotion.<sup>17</sup> There is scarcely any evidence of class conflict or of real class interest. Bourgeois and noble prefects worked together in relative harmony. They appear to have administered France in the interests of all Frenchmen and, of course, in the interests of Napoleon Bonaparte. The proportion of nobles increased, but this occurred mainly in the first years of the regime. The bourgeois remained in a majority in the corps until the Restoration when the real aristocratic reaction began.

The prefects, it is often said, came from a wide variety of backgrounds. We are told that they were recruited from the Revolutionary assemblies, the diplomatic service, the Court, the army, the lower administration, and even from the Church. These generalizations suffer from four major weaknesses. They usually refer to the first hundred prefects and not the entire corps of 281. They do not tell us how many came from each background, which is, after all, the important question. They often confuse political, social, and functional backgrounds, as does Pierre Henry when he identifies the Revolutionaries (political background); six old aristocrats (social background); and three ambassadors, seven generals, and two administrators (functional background).<sup>18</sup> Finally, the information itself is often erroneous. A brief acquaintance with the corps reveals that the prefects were drawn from four main occupations—the administration, the army, the legal profession, and politics. Those who worked in a single profession before becoming prefects present no problems of classification. Unfortunately most prefects had flirted with several occupations so that it becomes difficult to categorize their functional background. Charles d'Arberg, for example, was an auditor in the administration, a major in the army, a chamberlain in the Imperial Court, and had undertaken various diplomatic missions.<sup>19</sup> Charles Delacroix had been an *avocat*, administrator of a department, politician, ambassador, and minister.

Given these problems, it becomes necessary to define exactly what is meant by functional background. For the purposes of this article, the term signifies the chief occupation, business, profession, or activity before becoming a prefect, or any occupation in which the prefect was engaged for more than five years. Someone who had been a lawyer for a dozen years and then entered politics for a few years and the army for a few more will be defined as a lawyer. On the other hand, an *ancien régime* army officer who sat in the

<sup>17</sup> G. St. Yves and J. Fournier, *Le Département des Bouches-du-Rhône* (Paris, 1899), 60-61.

<sup>18</sup> Pierre Henry, *Histoire des Préfets* (Paris, 1950), 22.

<sup>19</sup> *F1b1* 155-6.

Convention and the Council of Ancients and was six years a subprefect will be classified as having military, political, and administrative backgrounds.<sup>20</sup> Sometimes the definition appears arbitrary: Joseph Mounier achieved his fame in two years in the National Assembly, but that short political interlude does not make politics his functional background. There is a very clear difference, after all, between political and functional background. Politics is only defined as a functional background if the prefect had spent more than five years in the assemblies or if most of his time had been spent in politics. By this standard the vast majority of the Revolutionary politicians are properly classified as having functional backgrounds other than politics.

The largest group within the prefectural corps—99 or 32 per cent—was recruited from the administration itself, and not from politics as most historians suggest. These men were the subprefects and secretaries-general, the intendants of the conquered provinces (there were no intendants from *ancien régime* France), the bureaucrats of local or national government, or the members of the diplomatic or consular services. The second most important functional background was law, from which 61 (20 per cent) of the prefects were recruited. There were a variety of titles for lawyers, including *avocat*, *parlementaire*, *notaire*, *procureur*, *juge*, *commissaire*, or simply *homme de loi*. Almost all of these men were lawyers before the Revolution, and most fulfilled legal functions during the Revolution. Many of them were elected to the assemblies, some were involved in local administration, but their chief occupation was law. Politics forms the third functional background. Forty-one (14 per cent) of the prefects had spent over five years in the assemblies or had spent sufficient time in politics for it to count as their chief former activity. The armed forces provided 36 (12 per cent) of the prefects. Most of these *militaires* were officers from the *ancien régime*, some of whom were elected to the assemblies. We are left with 63 prefects (22 per cent) of miscellaneous background. Their backgrounds are usually known, but their numbers are too few to consider separately. They include, for example, six chamberlains from the Imperial Court, four members of the clergy, perhaps a dozen businessmen, six academics, and a few landowners. Most of this group spent a few years in a variety of functions so that it is more accurate to classify them as miscellaneous than by any of the major categories. Finally, this category includes perhaps a dozen about whom nothing is known.

In so identifying the prefects we have been considering the corps as a whole, but when did these groups serve in the fifteen-year period? One in five was a lawyer, but when were the lawyers employed, and did their influence within the corps grow or diminish? Table 3 indicates the changing proportion of prefects in terms of background. The proportion of lawyers

<sup>20</sup> One prefect could therefore have as many as three functional backgrounds, so the following statistics are based on 309 identified backgrounds and not 281 prefects.

TABLE 3. FUNCTIONAL BACKGROUND

Years	Administration		Politics		Army		Law		Miscellaneous	
	Number	Per cent	Number	Per cent	Number	Per cent	Number	Per cent	Number	Per cent
1800	19	17	26	23	8	7	35	30	27	24
1801	20	17	25	21	10	9	35	30	28	24
1802	19	15	24	20	13	11	37	30	30	24
1803	20	16	22	18	17	14	41	33	25	20
1804	21	17	22	18	19	15	38	30	25	20
1805	22	18	21	17	17	14	38	31	25	20
1806	24	19	22	18	17	14	38	31	23	19
1807	29	23	21	16	19	15	38	30	21	16
1808	30	23	22	17	19	15	38	29	21	16
1809	34	26	18	14	19	14	35	27	26	17
1810	39	28	19	14	17	12	39	28	25	18
1811	59	41	18	13	12	8	30	20	24	17
1812	59	41	18	12	14	10	29	20	25	17
1813	57	41	17	12	15	11	26	19	24	17
1814	65	47	15	11	12	9	22	16	23	17

declined steadily from one-third to one-sixth, the proportion of politicians from one-fifth to one-tenth. The number of *militaires* remained constant at around 10 per cent; but the number who were recruited from the administration rises from a relatively insignificant 15 per cent to almost half by 1814. What occurred was a gradual professionalization of the corps. In 1800 a majority were former lawyers and politicians; by 1814 nearly a majority had been administrators. The greatest change occurred in 1810 when the proportion of administrators jumped from 28 to 41 per cent. Curiously enough, this is the date some historians identify as the beginning of the decline in the quality of the administration.

IT WAS THE GOVERNMENT'S POLICY that prefects, unlike subprefects and secretaries-general, would never be appointed to the departments of their birth. This has long been known and, since it was official policy, has been assumed to be true.<sup>21</sup> In fact, ten prefects were appointed to the departments of their birth; but the important questions remain: from which departments were they recruited and to which departments were they appointed? To determine geographical origins I have somewhat arbitrarily divided France into six regions, plus Paris. The 11 departments of the West (north of the Loire and west of 0° longitude) provided 26 prefects (2.5 per department). The 18 departments of the South (from the Isère to the Cantal to the Pyrénées Orientales) provided the same number per department, or 42. The area of least representation was the South-West (Charente Inférieure to Creuse to Ariège), where 27 prefects were born in the 16 departments.

<sup>21</sup> Aulard, *Études*, 123.

Twenty-six prefects came from the eight departments of the North (Ardennes to Seine Inférieure) and 18 from the seven departments of the North-East (Meuse to Doubs), these areas sending more than three prefects per department. The rest of France, the 25 departments in the center, sent 58 prefects, and a further 37 were recruited in Paris. Thus Paris and the North proved the best recruiting ground for prefects.

No fewer than 31 prefects (12 per cent) came from outside the borders of France of 1789.<sup>22</sup> They included thirteen Italians, nine Belgians, six Dutchmen, one German, and two Swiss. The number of foreigners was around seven from 1800 to 1810, then jumped to around sixteen (13 per cent) after that date. This is a considerable group of foreigners, but by 1811 the Empire contained 130 departments, one-third of which were non-French. The presence of this substantial group of foreigners forces a modification of another widely held idea about the Empire: by expanding so rapidly the French administration became overextended.<sup>23</sup> Such a generalization is true up to a point but must be qualified. In all the new areas Napoleon recruited promising people for the imperial administration, including even the Council of State. In 1812, for example, one in four ministers in the diplomatic service was non-French. By themselves the French were administering an area much larger than pre-Revolutionary France, but they were assisted by a large and growing number of people from these newly acquired territories.

While we know that prefects were rarely appointed to the departments of their birth, it remains to be seen whether there was a connection between their place of birth and their appointments. In fact, 22 per cent of the appointments were to departments in the neighborhood of their birthplace (within 100 kilometers); and half of these were to departments bordering on the departments where they were born. A further 17 per cent were to departments in the same area (within 200 kilometers), and 14 per cent were to departments within the same region (within 300 kilometers). Together with those appointed to the department of their birth, these appointments within the region of birth constitute an absolute majority of the known appointments, and four appointments in ten were to departments within 200 kilometers. These trends are even more pronounced for the non-French prefects. One-third of them served within 100 kilometers of the place of their birth, 14 per cent within 200 kilometers, and another 14 per cent within 300 kilometers, almost two-thirds serving within the area of their birth. In other words, the prefects in the Italian departments were often Italians, those in Belgium were Belgians, and most prefects served in the regions of their birth.

It has long been known that the prefects were young, but the assertion

<sup>22</sup> Yet Godechot claims that "none came from the newly-acquired territories." Godechot, *Institutions*, 588.

<sup>23</sup> Geoffroy Bruun, *Europe and the French Imperium* (New York, 1938), 66-67.

has hardly been proven, and the exact meaning of "young" has never been identified. Pierre Henry, for example, states that 25 prefects were appointed in their twenties, which leaves us in ignorance of the age of appointment of the remaining 90 per cent of the corps and of the age of all the prefects while they held office.<sup>24</sup> The corps was indeed relatively young, especially in terms of the bureaucracies of the nineteenth and twentieth centuries. Twenty-three prefects (8.5 per cent) were appointed in their twenties, and the largest group—97 or 35.8 per cent—entered in their thirties. Ninety-six (35.4 per cent) were between 40 and 50. Only 55 (20 per cent) were over 50 at the time of appointment, with 11 of these in their sixties. As the regime went on, more and more young men were recruited from the lower levels of the administration, and the age of the prefects at the time of appointment

TABLE 4. AVERAGE AGE OF THE PREFECTS

<i>Years</i>	<i>Average Age</i>
1800	41.6
1801	43.0
1802	43.3
1803	43.8
1804	44.3
1805	45.4
1806	46.6
1807	46.8
1808	48.2
1809	48.1
1810	48.0
1811	45.5
1812	46.2
1813	47.0
1814	45.9

decreased. These figures reveal that the prefects were young at the time of appointment but tell us nothing about their age while in office. Table 4 gives the average age of the prefects as of January 1 of each year.

There was, as could be expected, a gradual aging of the corps caused by the retention of early appointees and the establishment of some degree of stability. The periods of instability or turnover, the *mouvements*, are readily identified—1800 when many of the first appointees were replaced by older men (shown on January 1, 1801), 1810, and 1813 when there occurred massive retirements of older prefects and their replacement by younger men. These two changes reflect the international rather than the internal situation. In 1810 Napoleon began drifting into war with Russia and had to reinvigorate his administration. It is commonly argued that a chief factor in

<sup>24</sup> Henry, *Préfets*, 44.

Napoleon's eventual defeat was the war weariness of the generals and the nation at large. Obviously Napoleon was perfectly aware of the problem. In 1813 there were major retirements of old and tired prefects or of any thought to be less than zealous in their administration in general or in the recruitment of troops in particular. They were invariably replaced by auditors from the Council of State or by subprefects, men who were young, ambitious, aggressive, had served no other regime, and were already experienced in administration. As a whole, the corps only aged 4.3 years during the fifteen-year period of its existence, which proves that there was a steady turnover of administrators.

A brief examination of the futures of the prefects will cast some light on their role as prefects. In particular, it will reveal whether the prefectural corps was a career in itself, the climax to a career, or a step to further promotion. One hundred and thirty prefects were still in office when the regime collapsed. What happened to them after the Allied invasion or after Napoleon's abdication is a problem of Restoration politics, which has been exhaustively studied by Nicolas Richardson.<sup>25</sup> Of the remainder, 19 prefects were promoted to the Council of State, the supreme legislative body in France. They included Jean Pierre Bachasson de Montalivet who soon became minister of the interior in charge of his former colleagues. These 19 men received a considerable proportion of the vacancies in the Council of State, which shows that Napoleon was using the prefectural corps as an apprenticeship for the highest positions in government. The prefects still in office in 1814 and those promoted to the Council of State constituted more than half of those employed by Napoleon. Another 21 died in office, and 18 have left little or no trace of their subsequent careers.

The remaining prefects are more difficult to classify. They were retired, transferred, or dismissed, but it is often difficult to distinguish between these three types of removal. Nineteen went to the Napoleonic assemblies, ten of these to the Senate. Twelve more retired without such honors, and two resigned. Twenty-nine were transferred to other administrative functions, and three went back to the army. Several became intendants of the conquered territories. A number entered the government in Paris, receiving such positions as *directeur général des mines*, *inspecteur général de l'instruction publique*, or *secrétaire général du Ministère de Police*. Laroche-foucauld entered the diplomatic service, three others became consuls. Many also went back to legal positions. On the whole it is probable that less than half of these transfers were to positions of equal or superior importance.

Twenty-eight prefects were dismissed, including those *destitué de ses fonctions* and those *appelé aux autres fonctions* but never reappointed. The motives, however diverse, were not often known to the prefects and are not always known today. Joseph Lagarde was suspended and ordered

<sup>25</sup> Richardson, *Prefectural Corps*, chs. 3 and 4.

to leave his department immediately, but he never found out why.<sup>26</sup> Felix Desportes, on hearing of his dismissal, wrote to the minister begging for an explanation and for an interview with Napoleon: "I am innocent of all the wrongs of which I am accused." But an interview would have accomplished little, as his successor reported that his department was in a state of complete confusion.<sup>27</sup> Claude Brugière de Barante was recalled in 1810 for his kindnesses towards Mme. de Staël and other former exiles, a piece of evidence that fits poorly into the general theory of an aristocratic reaction in that year.<sup>28</sup> Another noble, George de Belloc, was summarily dismissed in 1804 without ever receiving an explanation. Later on he discovered the cause. His wife, the daughter of the Duc d'Enghien's equerry, had expressed her horror at the execution of the duc. Her comments were in a letter to a friend, but the minister of police, Fouché, had all the friends of the Duc d'Enghien under surveillance. The letter was found on a suspect, and de Belloc found himself in premature retirement with his somewhat indiscreet wife.<sup>29</sup> Another royalist was cashiered in 1801 on the basis of a massive petition that accused him of being *chef de parti*, of juggling electoral lists, and of persecuting former revolutionaries.<sup>30</sup> Other prefects were dismissed for failing in various administrative tasks, failing to respond to crises, or failing to provide firm administration. Many were dismissed on the basis of petitions, reports from generals, the minister of war or of police, or the director of conscription. In the major turnovers of 1810 and 1813 the charge was usually failure to fulfill conscription quotas, the most serious administrative failing in any country facing imminent invasion.

Among the prefects there is one group whose future is a subject of intense interest to political historians of the period. These are the Revolutionary politicians. Lefebvre talks of the way Napoleon replaced his best administrators with second-rate men. In the Côte d'Or, he writes, the department passed through the hands of a Revolutionary, a tribune, a noble, and finally a duke. "The remaining survivors of the Revolution were few and far between."<sup>31</sup> Lefebvre clearly means to say that Napoleon deliberately replaced the excellent Revolutionary politicians with inferior people. The idea that the Revolutionary politicians were independent-minded advisers capable of standing up to Napoleon pervades the historiography of the subject. Equally pervasive is the idea that Napoleon became irritated by such men and deliberately removed them from his government. Let us reserve these allegations for examination later in the article in order to turn to the question of what happened to the 83 Revolutionary politicians Napoleon employed as prefects.

<sup>26</sup> F1b1 166-5.

<sup>27</sup> Felix Desportes to the minister of the interior, Dec. 13, 1813, F1b1 176-11 and fol. 52.

<sup>28</sup> F1b1 150.

<sup>29</sup> F1b1 156-15.

<sup>30</sup> F1b1 167-1, dossier Magnytôt.

<sup>31</sup> George Lefebvre, *Napoleon* (London, 1969), 2: 161-62.

Twenty-four Revolutionary politicians (29 per cent) were still in office in 1814. Sixteen (19 per cent), including Jeanbon St. André and Charles Delacroix, died in office. A further nine (10 per cent) were promoted, eight of these to the Council of State. More might have entered the Council of State except that the cream of the Revolutionaries entered the Council in 1800, the Council was relatively small, and its personnel was exceptionally stable. The promotion of these nine prefects suggests that Revolutionary politicians continued to move upward in the administrative pyramid even though there were few opportunities for such promotion. A number of politicians were retired, Nicolas Harmand at the age of sixty-seven, Jean Marquis, Joseph Martin, and Louis Milet at the age of sixty. Six more retired in their fifties, two in their forties, many of these going to the Senate.

More difficult to explain are those prefects transferred to other functions. Benoît Najac and Jean Jollivet simply returned to the Council of State after organizing their departments; Beytz and Charles Dugua received equivalent administrative positions in Holland and San Domingo. Jacques Guinebaud, at sixty-three, entered the consular service. A further eight, mostly in their late forties, received appointments as *juges*, members of the *conseil des prises*, *secrétaire du conseil des arts et commerce*, *maître des comptes*, or *receveur particulier des finances*, and François de Saint-Horent was reduced to the rank of *conseiller général* at the age of forty-two. Most of these transfers were normal administrative changes or semiretirements of older administrators. A few of them might be classed as demotions, but none of them were outright dismissals.

All but seven of the 83 Revolutionary politicians have been accounted for, and the reasons for the dismissal of J. Harmand and François Delattre are unknown. Nicolas Frochot was the most important prefect (Seine-Paris) in France from 1800 until 1812, when he accepted the unproven rumor that Napoleon had been killed in Russia. This piece of political gullibility immediately cost him his job, especially as an example had to be made. Alexandre Guérin de Chateauneuf-Randon, prefect of the Alpes Maritimes until 1802, had a checkered career. He entered the Court of the *ancien régime* as a page to the king and later to the Comte d'Artois, then became an officer in Artois's regiment. Betraying this royalist past, Guérin became a Jacobin Terrorist. He voted for the death of the king he had once served, sat on the Committee of Public Safety, and was sent on many missions during the Terror. He ended his career by spending the years 1812 to 1817 in debtor's jail. Antoine Rabusson-Lamothe was dismissed from the Haute Loire in 1810. Later, in an application for a job under the First Restoration, he claimed that he had been dismissed "by the perfidious insinuations of Lacuée, then minister of conscription." To have failed Napoleon in conscription was an advantage under the Restoration—Rabusson admitted he had tried to modify the harshness of conscription and, anyway, had always



been a royalist!<sup>32</sup> Jean Sabatier was dismissed "on the denunciation of several officials in the Department."<sup>33</sup> Finally Edme Bailly was one of the many prefects removed in 1813 for failing to recruit sufficient troops. This event occurred two years after the minister of war had officially reported to Napoleon that Bailly was somewhat feeble and too willing to conciliate. His dismissal followed repeated complaints from the minister, although Bailly claimed that he had done everything possible.<sup>34</sup>

One wonders what is left of Lefebvre's allegation that Napoleon deliberately removed the excellent Revolutionary prefects from the corps? The answer is—very little indeed. In 1814 almost one-third were still in office, and the Revolutionaries accounted for one prefect in five. The largest group who left the corps were those who died in office. Thirteen retired in their fifties or sixties, well over the average age of the prefects. Fourteen were transferred, but few of these could be regarded as demotions. Only six were dismissed outright, several perhaps unjustly, several with good cause. But against this must be set the nine who were promoted to higher office. It is true that the number of Revolutionary politicians fell from 50 to 24, but this attrition was caused by death, retirement, promotion, normal administrative transfers, and the mathematical inevitability of aging. In fifteen years in power Napoleon dismissed, demoted, and disgraced thousands of people. They included Revolutionary politicians as well as every other group who served him—noble and bourgeois, Jacobin and emigré, even his own friends and family. It cannot be said that he deliberately weeded the Revolutionary politicians from the prefectural corps.

FRENCH, ENGLISH, AND AMERICAN HISTORIANS have argued that Napoleon's government gradually deteriorated, and some of them have mentioned specifically the deterioration of the prefectural corps. The prefects governed France; therefore the quality of their administration is one of the most important historical problems of the period. The deterioration of the prefectural corps is such a widespread assumption that the historiography of it and the evidence for it must be reviewed at some length.

George Lefebvre has written the best French account of Napoleon. He talks of the ministry itself and argues that gradually "the great administrators" were replaced with "second-rate men" and nobles, "many of [whom] were lacking in ability." "The recruitment of prefects underwent a much more perceptible change," which is identified as the replacement of Revolutionary prefects by nobles with the result that "there can thus be no illusion

<sup>32</sup> F1b1 172-1.

<sup>33</sup> F1b1 173-1.

<sup>34</sup> F1b1 156-2.

about the imperfections of local government."<sup>35</sup> Lefebvre's evidence is the replacement of a few Revolutionary politicians with a few nobles. In full agreement with Lefebvre, on this issue at least, is the late Professor Cobban. Cobban repeats the general statement about "the ablest men in France" being replaced by "flunkies." In the prefectural corps Napoleon appointed more and more nobles and emigrés who "introduced more easy-going methods . . . and leave the real work to the secretaries-general. Many were now secret royalists." The American historian Geoffroy Bruun would agree: "Bonaparte's first generation of officials, trained in the pragmatic school of the Revolution, formed a group unique in character and experience. His second generation . . . was by his own confession less capable and less enterprising." Again the administration deteriorated because fewer Revolutionaries were used. Jacques Godechot agrees with Cobban that the number of nobles increased and that they would not hesitate to betray Napoleon, though no betrayals are actually mentioned. Paul Gagnon refers to the loss of "many able men" and adds that "to face mounting problems, Napoleon relied on lesser men." Jean Savant argues the same: the appointment of a noble was like replacing a colonel with a lieutenant.<sup>36</sup>

None of these general accounts has given any real evidence to prove that this group of 281 administrators gradually deteriorated. Perhaps this is understandable in general works; perhaps the evidence is found in the more specialized studies. A local study that draws conclusions about the general quality of the corps is that of L. Benaerts, who argues that the prefects after 1804 were not as good as those of the Consulate.<sup>37</sup> Benaerts's work, however, provides little basis for his argument; his narrative only extends to 1804, he has no comparison of administrators before and after 1804, and his only evidence of inferior administration is a quotation from a biased subprefect. And, even if his evidence were sound, which it is not, no one should pretend to generalize from the exceptional situation of Brittany to the whole of the French Empire. In a similar study of the Bouches-du-Rhône (1800 to 1810), St. Yves and Fournier came to similar conclusions.<sup>38</sup> According to them, the administrators before 1805 were largely Revolutionaries, while those after 1805 were largely nobles who were enemies of the Revolution, critics of its institutions, and royalists in 1814. Their evidence does not include the prefects who, from 1800 on, were Revolutionaries; the dividing line of 1805 is largely arbitrary; the conclusions about 1814 are unsupported because the narrative only extends to 1810; and the only noble subprefect from the Bouches-du-Rhône who

<sup>35</sup> Lefebvre, *Napoleon*, 2: 161-62. It should be noted that most historians who write surveys of Napoleon are experts on the French Revolution and not on Napoleon. Naturally, they often see the Napoleonic regime in relation to the Revolution and not as a separate historical entity.

<sup>36</sup> See Cobban, *History of France*, 25-26, 55; Bruun, *French Imperium*, 66; Godechot, *Institutions*, 588; Paul Gagnon, *France Since 1789* (New York, 1964), 77; and Savant, *Préfets*, 170-73.

<sup>37</sup> L. Benaerts, *Le Régime consulaire en Bretagne* (Paris, 1914), 357-59.

<sup>38</sup> St. Yves and Fournier, *Bouches-du-Rhône*, 398-99.

became a prefect was not then a royalist. Furthermore, conclusions about the Bouches-du-Rhône are inapplicable to the rest of France, because the area was a notable center of royalism, religious strife, local patriotism, and opposition to the centralization of the French Revolution.

A more recent study is that of J. P. Soulet on the Hautes Pyrénées under Napoleon. This department had three prefects. The first, Bernard Lannes, was dismissed in 1802 for incompetence.<sup>39</sup> The second, the Revolutionary politician Jean Chazal, provided excellent administration from 1802 until 1813 when, as a non-noble, he was caught up in the social transformation of that year.<sup>40</sup> Much later Soulet explains that Chazal had been transferred to another department, not dismissed, and that the reason was not his social origin but his rheumatism, which made it impossible for him to work and led to his complete retirement in early 1814.<sup>41</sup> Chazal was replaced by a nobleman, Arbaud-Joucques, who had been a subprefect for six years. By Soulet's admission Arbaud was far better prepared for the position than the Revolutionary politician he replaced, and, by Soulet's evidence, he was every bit as good an administrator as Chazal.<sup>42</sup> Arbaud did not go over to the Bourbons until two weeks after Napoleon's abdication, at which time he made no apologies for having loyally served Napoleon.<sup>43</sup> The evidence, then, is of a useless administrator being replaced by a competent Revolutionary politician replaced in turn by a better qualified and equally competent nobleman. However, so strong is the opinion that the administration deteriorated that Soulet, in spite of his own evidence, asserts that Arbaud was just another of those docile émigrés.<sup>44</sup>

The evidence for decline is in some ways denied by Godechot in his preface to Soulet's book. He accepts that Lannes was "without doubt unsuited for administration" (which is the important issue in a book on administration) and stresses the things the three prefects supposedly had in common. They were, he says, intelligent, honest, conscientious, loyal, and shrewd; they understood the interests of the state and the problems of the department, and all applied the orders of the government. The information in the book indicates that Lannes did not rank with Chazal and Arbaud in these matters. Then, by going outside the book's evidence, Godechot subtly discredits the noble Arbaud by saying that he was "representative" of the *ancien régime* and of the Restoration and of the landowners who, unlike the sans culottes of 1792-93, failed to rally to the defense of France. (Chazal

<sup>39</sup> Soulet, *Hautes-Pyrénées*, 14, 16, 39, 45, 233.

<sup>40</sup> *Ibid.*, 193.

<sup>41</sup> *Ibid.*, 234-35.

<sup>42</sup> *Ibid.*, 195-96.

<sup>43</sup> *Ibid.*, 225.

<sup>44</sup> *Ibid.*, 195. The adjective "docile" is constantly used to discredit nobles, yet the nobles of the *ancien régime* were anything but docile. On the other hand, some of the Revolutionary politicians were and had been docile. However, in describing them, historians prefer the adjective "moderate," which is always seen in a favorable light. This is a case of heads the Revolutionaries win, tails the nobles lose.

was not a *sans culotte* and Arbaud did defend France in 1814.) This one modern and thorough study of a department indicates clearly that Napoleon's first prefect was incompetent and that there was little to choose between the next two except that by 1813 Chazal was old and sick and Arbaud was better trained for the position. Why, we may wonder, does Godechot find it necessary to promote Lannes by equating him with the other two and to discredit Arbaud on the basis of the actions of other people of whom he was not at all representative?

Other regional and individual studies prove little about the changing quality of the prefects. Charles Poitevin-Maissemy, the object of a study of the Pas-de-Calais, was still in the corps in 1814, so that any conclusion about his ability would apply to the entire period. The same is true of Jean de Bry, prefect until the Restoration. An exhaustive thesis on Jeanbon St. André, cited in several bibliographies of the prefects, covers his activities as politician but not as prefect, and, like de Bry, he was in office until the Allied invasion. A study of the Rhine departments is excellent administrative history, but it tells us little about the administrators. The biographer of Jacques Beugeot argues that Beugeot was an excellent prefect, an opinion obviously shared by Napoleon, who promoted him to the Council of State and made him a chief minister in the satellite kingdom of Westphalia. In another regional study, that of the Meurthe under the Consulate, Thiry tells us that the first prefect was an excellent administrator. This was the *conventionnel*, Jean Marquis, but around 1808 he applied for his retirement because of illness. The study says nothing of his replacement. In short, these regional and biographical studies tend to agree with the theory of deterioration, but in no way do they prove it, and much of the information provided actually contradicts it.<sup>45</sup>

The overwhelming opinion of all of these historians is that the prefectural corps deteriorated. But have they really proven their opinion? Some historians mention one or two excellent prefects who left the administration around 1810, and perhaps a few nobles who were appointed. In a corps of 281 prefects can one or two such changes prove a general deterioration? The arguments put forward can be summarized as follows: the number of Revolutionary politicians declined, therefore the administration deteriorated because Revolutionary politicians always made good administrators. At the same time the number of nobles increased, therefore the administration deteriorated because nobles always made poor administrators, or were royalists, or were "ready" to be disloyal. Only a few examples are produced to back up these arguments. Furthermore, the assumption about the quality

<sup>45</sup> See J. Chavanon and G. St. Yves, *Le Pas-de-Calais de 1800 à 1810* (Paris, 1907); Léonce Pingaud, *Jean de Bry* (Paris, 1909); L. Levy-Schneider, *Le Conventionnel Jeanbon Saint André 1749-1813* (Thèse, Paris, 1901); P. Sagnac, *Le Rhin français pendant la Révolution et l'Empire* (Paris, 1917); E. Dejean, *Un Préfet du Consulat, Jacques-Claude Beugeot* (Paris, 1907); and J. C. Thiry, *Le Département de la Meurthe sous le Consulat* (Thèse, Nancy, 1957), 29-30.

of Revolutionary politicians is entirely political, and tells us nothing about their quality as administrators; the assumption about the weakness of the nobles is a social one and so does not bear upon administrative matters. The arguments are not based on evidence and turn out to be little more than prejudices.

What might prove more fruitful would be to identify a number of accepted characteristics of quality in bureaucracy and then determine the degree to which these criteria were met by the prefectural corps. For this purpose several assumptions are necessary: an experienced administrator, by and large, is likely to be better at his job than an inexperienced one; a trained administrator is probably better than an untrained one; an administrator who rises through the ranks by promotion is likely to be better than one who does not; and a stable administration is preferable to an unstable one. In short, we may argue that a stable group of professionals who know their business are probably better at that business than a group of novices, no matter how successful those novices were in some other activity. This is, indeed, the gist of what Max Weber tells us: experience, training, hierarchy, promotion, and stability are the distinguishing features of bureaucracy; they are the essential bases for the quality of modern bureaucracy and for its superiority over nonbureaucratic forms of organization.<sup>46</sup> The details have never been agreed on, but only an anarchist could deny the general importance of these characteristics. We may usefully assume that, in evaluating the quality of an administration, these criteria, though perhaps not final, have more historical and universal validity than the assumption that any Revolutionary politician will be a better administrator than any person of noble birth.

Probably the most important attribute of the successful bureaucrat is experience. In general, the experienced administrator can be distinguished from the inexperienced one in that the former knows the routine and the rules; he is thoroughly familiar with the functioning of the administrative machine; he has dealt with many of the problems or observed his colleagues dealing with them; he has learned some lessons from his own mistakes and perhaps from those of others; he has learned how to interpret or apply national laws to local circumstances. One might observe that occasionally a totally inexperienced man is far superior to a civil servant with years in office or that, from time to time, a group of novices has worked miracles in administration. But such exceptions do not seriously contradict the rule, even if they attract the historian's attention. If this can be accepted, then one can generalize to the bureaucracy as a whole and assume that an experi-

<sup>46</sup> For administrative studies commenting on the characteristics of bureaucracy see C. Friedrich, "Some Observations on Weber's Analysis of Bureaucracy," in Robert K. Merton *et al.*, *Reader in Bureaucracy* (Glencoe, Ill., 1952), 29; W. A. Robson, ed., *The Civil Service in Britain and France* (London, 1956), 1; F. M. Marx, *The Administrative State* (Chicago, 1957), 22-51; F. M. Marx, ed., *Elements of Public Administration* (2d ed.; Englewood Cliffs, 1959), 34-37; and Max Weber, *Essays in Sociology*, ed. H. H. Gerth and C. Wright Mills (New York, 1946), 196-203.

enced bureaucracy is likely to be superior to an inexperienced one, and that the more experience that bureaucracy has, the better the government it is likely to provide. Experience is defined here as years spent in administrative work, including work in local or national government, in the diplomatic and consular service, or in the auditoriat of the Council of State. It has already been established that around one-third of Napoleon's prefects were recruited from other branches of the administration, thereby ensuring that many prefects were experienced bureaucrats when they arrived in the departments. Given the stability of the corps, a general rise in average experience was inevitable; but the details of that increase provide some interest.

The overall increase is indeed impressive—the prefects of 1814 had four to five times more experience in administration than those of 1800. The rise is almost perfectly gradual, with only slight declines in 1811 and 1814. By January 1, 1801, the average had climbed by more than one year, indicating that in 1800 Napoleon had dismissed a number of the first prefects and replaced them with more experienced men. This is a reflection of the haste with which the original list of prefects was prepared. The same occurred in 1804. In the eight years before 1810 the average rose by almost six years, reflecting the high degree of stability of that period.

The effects of the renewals of 1810 and 1813 are of particular interest. In 1810, seventeen per cent of the prefects were dismissed, and the compensating appointments caused the average age of the corps to drop by  $2\frac{1}{2}$  years. In 1813 nearly one prefect in five was dismissed, the average age of the prefects declining by more than one year. In both cases the average amount of experience of the corps declined only slightly. How could average age

TABLE 5. AVERAGE YEARS OF ADMINISTRATIVE  
EXPERIENCE OF THE PREFECTS

<i>Years</i>	<i>Average Years of Administrative Experience</i>
1800	2.4
1801	3.5
1802	3.9
1803	4.2
1804	5.4
1805	6.5
1806	7.0
1807	7.8
1808	8.7
1809	8.9
1810	9.8
1811	9.5
1812	10.0
1813	11.5
1814	11.3

decline by  $2\frac{1}{2}$  years and average experience by only four months? The answer is simple. In both years a large number of older administrators were replaced by much younger men who had been in the administration almost as long as the men they replaced. This fact casts an entirely new light on the famous "movements" of 1810 and 1813. They have often been seen as political purges or part of an "aristocratic reaction" in which able Revolutionary politicians were replaced by lazy *ancien régime* nobles. In 1810 the number of Revolutionary politicians did indeed decline, but only seven of the nineteen dismissed were Revolutionaries. On the other hand the number of nobles increased by only four, but their proportion remained the same owing to the expansion of the Empire. Obviously, what occurred in 1810 and in 1813 was neither a purge of Revolutionaries nor an aristocratic reaction: it was a significant administrative change in which a number of aging prefects were replaced by younger men fully qualified for promotion by way of apprenticeship. For this reason the average amount of experience declined only slightly during these two periods of massive renewal. If experience be the most important qualification for the administrator, then in this respect Napoleon's prefectural corps improved steadily from the beginning to the end, with the prefects of 1814 having four times as much experience as their colleagues of 1800.

STABILITY OF PERSONNEL may be readily identified as one of the main characteristics of a developed and professional bureaucracy. If half the bureaucrats are being replaced each year then few people know their jobs, few have experience, the better ones will try to find more stable employment, and the administration will be chaotic. On the other hand, if the bureaucrat can hold his job for a sufficient length of time then he can acquire wisdom in the execution of his tasks, accumulate the necessary information and knowledge, learn of other positions so that he may competently respond to a promotion or transfer, and fulfill his functions with some confidence and objectivity because he knows he has tenure. Of course, stability may lead to stagnation, with administrators who are too old, have occupied the same positions for too long, whose horizons have grown too narrow, who resist new ideas, and who cannot respond to new challenges. But that possibility will not blind the serious student of administration to the general truth that a degree of stability is highly desirable.

In 1913 Aulard stated that Napoleon's prefectural corps was stable, his evidence being that two prefects out of 281 served for more than fourteen years.<sup>47</sup> His generalization was true, though his evidence hardly proved it. The prefects remained in the corps for an average of  $5\frac{1}{2}$  years and in each department for an average of 4.3 years, each prefect holding an average of

<sup>47</sup> Aulard, *Études*, 130.

1.3 appointments. Averages tell us little about individual records. Ninety-two of the prefects served less than three years, with 47 of these being appointed and dismissed within the first three years, clearly marking this period as one of the most unstable of the regime. Another 38 were appointed during the last three years, their careers under Napoleon being cut short by the fall of the regime.<sup>48</sup> Only seven of the 92 who served less than three years were appointed between 1803 and 1811. The second largest group, in terms of length of service, were the 73 prefects who occupied their positions for periods of three to six years. Forty-five served for periods of six to nine years; thirty-nine for nine to twelve. Above these, the real stability of the corps centered on the 33 prefects who served more than twelve years, many of them for the entire period. If one ignores the confused years of

TABLE 6. STABILITY: NUMBER AND PROPORTION OF DISMISSALS PER YEAR

<i>Years</i>	<i>Number</i>	<i>Proportion</i>
1800	9	9%
1801	14	14%
1802	18	17%
1803	7	7%
1804	7	6%
1805	9	8%
1806	8	7%
1807	4	4%
1808	9	8%
1809	10	9%
1810	19	17%
1811	5	4%
1812	3	3%
1813	23	18%

the early Consulate when the administration was being established and the last year when it was disintegrating, one is left with a remarkably stable and permanent administration.

But the important question is whether the administration grew more or less stable. The best indicator of relative stability is the turnover per year, which can be established by comparing the number of dismissals per year to the total number of departments (see table 6). Clearly the period of greatest instability was from 1800 to 1802, with one-third of the prefects dismissed. This massive shuffle underlines the weakness of all those accounts based on the original appointments to the corps. From 1802 until 1809 the turnover was from six to eight per cent, which would be normal. The exces-

<sup>48</sup> We are concerned here with the stability of the corps under Napoleon and not with the individual careers. Many prefects still in office in January 1814 had long subsequent careers. A study of their entire careers would reveal a vast increase in the average length of service.



sive turnover of 1810 was followed by the two greatest years of stability of the period, so that the average turnover between 1810 and 1812 was seven per cent or normal. The attempt to head off impending disaster made 1813 the year of greatest change. Then, 1814-15 were years of massive instability in the prefectural corps.

Was this stability so marked as to make the corps stagnant? The answer is no. For one thing the Napoleonic period was really too short for administrators to have become stagnant. Bourgeois de Jessaint was a prefect from 1800 to 1838. By the 1830s he might have become somewhat set in his ways, but he certainly was not in 1814. Second, there was considerable transfer between departments. Third, the table of average age shows that the administration was turning over fairly well: it was becoming increasingly stable but remaining relatively young. So, after three years of uncertainty and confusion, the Napoleonic prefectural corps became remarkably stable and experienced only one major renewal before 1813. In terms of administration, this meant that after 1803 the prefects had the time, the opportunity, and the confidence to learn their jobs, familiarize themselves with their departments, and provide intelligent and stable administration.

One of the cardinal features of bureaucracy is a hierarchy or pyramid of positions running from least to most important. From the beginning the prefectural corps was organized in just such a fashion. At the top the Seine or Paris was in a class by itself, the prefect holding near-ministerial rank. The rest of the departments were divided into four ranks. In the first category were the six departments that contained the largest cities in the Empire, such as the Rhône with Lyons. In the second rank were the next 12 departments, such as the Nord with Lille. The third category contained 32 departments, the fourth, 79. The sixth rank consisted of the prefect's chief subordinate, the 130 secretaries-general, and beneath them were the 500 sub-prefects. In 1803 another level in the hierarchy was created. This was the auditoriat of the Council of State, some of whose members trained to be subprefects. From the auditor to the prefect of the Seine there were eight separate ranks or echelons in the prefectural hierarchy.

If hierarchy be the characteristic of modern bureaucracy, then promotion through that hierarchy is characteristic of the personnel. Promotion, in theory at least, is based on merit—the bureaucracy providing an opportunity for a career open to talent. Thus the vast majority of upper-level administrators, be they in government, the army, or business, have only arrived at the top after proving their ability during apprenticeship and earning promotion through the junior ranks. The reason for this is simply that it has long been recognized that the best administrators are those who work their way from the bottom or the middle to the top, learning about each level as they pass through it, being promoted to the higher offices only after the successful completion of more minor responsibilities, and being promoted on the recommendation of superior officials who identify the reasons for the promo-

tion. It follows that a measure of the quality of an administration is the degree to which the superior bureaucrats are recruited from the lower echelons of the administration. Again, one or two exceptions will not destroy the general validity of this rule.

An official report sent to Napoleon by Champagny, the minister of the interior, clearly shows that promotion through the ranks was indeed the policy of the Napoleonic government.<sup>49</sup> Champagny's list of candidates for important vacancies in the corps was composed of men who were already prefects: "It appeared to me that [Napoleon's] intention was that the honour of occupying the most prestigious positions in the career would be the reward for the zeal and talent which had been displayed in the less important positions." Of the eight prefects recommended five received their promotions within two years. There were many cases of promotion through the ranks. Alexandre Lameth, one of those recommended by Champagny, began in a fourth-class department, moved to a third-class one in 1806, then to one of the second-class in 1809. The career of Amable Brugière de Barante provides an almost perfect example of promotion through the hierarchy. After spending two years in local administration in Carcassonne, he entered the ministry of the interior as a student of administration. Later, as auditor of the Council of State, he fulfilled the duties of intendant of Danzig and by 1807 was subprefect. Two years later he was prefect of the fourth-class Vendée and by 1813 had been promoted to the second-class Loire-Inferieur. He was still only thirty-one, but it was his sixth promotion in thirteen years of administration.

The first method of determining the degree of promotion is to see what proportion of the total number of appointments in any year were, in fact, promotions. Table 7 indicates that in the first five years approximately 30 per cent of the appointments constituted promotions within the administration. Between 1804 and 1805 the proportion jumped from 23 per cent to 57 per cent and from then on averaged around two-thirds. So, during the Consulate one prefect in three arrived at that position as a result of promotion from lesser offices; after the establishment of the Empire twice as many prefects were promoted to office. A more accurate method of determining the degree of promotion is to establish how many prefects in each year had arrived at their positions as a result of promotion from inferior ranks. Table 8 shows that during the Consulate approximately 40 per cent of the prefects had been promoted, in the next five years around 50 per cent, and after 1810 over 60 per cent had arrived at their positions after apprenticeship at lower levels of the administration. The trend toward professional appointments from the junior levels was steady and consistent. If a good administration necessitates promotion through the ranks and apprenticeship at the junior levels before promotion to the senior, then the prefectural corps

<sup>49</sup> Champagny to Napoleon, Nov. 20, 1805, F1b1 150-152.

TABLE 7. NUMBER OF PREFECTS PROMOTED AND PERCENTAGE OF THE TOTAL NUMBER OF APPOINTMENTS THAT WERE PROMOTIONS

<i>Years</i>	<i>Number</i>	<i>Percentage</i>
1800	41	36%
1801	9	38%
1802	6	22%
1803	4	29%
1804	3	23%
1805	13	57%
1806	13	72%
1807	3	60%
1808	7	50%
1809	11	79%
1810	29	66%
1811	4	36%
1812	4	57%
1813	17	44%
1814	10	91%

was a relatively good administration that gradually and consistently improved throughout the period.

PROMOTION THROUGH THE RANKS is one method of recruiting able executives for the higher positions in a bureaucracy. Another method is specific training and education, the trainee entering the hierarchy at the junior officer or junior executive level and then being promoted upward toward the top. The two methods are complementary rather than contradictory. They ensure that the top of the hierarchy is staffed by a combination of two groups, one of which has worked its way from bottom to top and is rich in experience, the other of which has entered at the executive level and is rich in education and formal training.

This second method, that of formal education and training, has become increasingly popular due to the sophistication of modern society. I think it will be readily agreed that it is desirable for a man to be trained before entering high-level administrative functions. It would follow that a group of trained administrators could be expected to perform their tasks better than a group of untrained ones and that the more training an administration has, the better it probably is, providing that the recruitment of these trainees does not destroy the career open to talent for the other junior administrators. The system of training developed by Napoleon was the auditoriat of the Council of State, described by Markham as "one of the most interesting and original creations of the Napoleonic regime."<sup>50</sup>

The auditoriat was devised as a common training program for all branches

<sup>50</sup> Markham, *Napoleon*, 98.

TABLE 8. NUMBER OF PREFECTS WHO ONCE HELD INFERIOR ADMINISTRATIVE POSITIONS AND THEIR PROPORTION OF THE WHOLE CORPS

<i>Years</i>	<i>Number</i>	<i>Percentage</i>
1800	41	36%
1801	49	40%
1802	51	40%
1803	47	40%
1804	46	40%
1805	52	45%
1806	59	50%
1807	59	51%
1808	69	57%
1809	72	58%
1810	90	64%
1811	82	62%
1812	81	61%
1813	92	63%
1814	88	63%

of the French administration. It began in 1803 when sixteen young men were attached to the five sections of the Council of State and the six ministries represented on the council. At the council the auditors listened to the debates, read reports, assisted the councillors, and undertook various administrative and educational missions. After several years of training the auditors were examined by three councillors of state upon whose recommendations they would be permanently attached to one branch of the administration or retained at the council for further training. It is impossible to deny the quality of the training these young men were receiving. To enter the auditoriat they had to have a university degree in public law. The opportunity to listen to debates in the Council of State, prepare reports for the councillors, go on fact-finding missions to all parts of France and of Europe, work directly with a minister, report directly to the council, and be interviewed by Napoleon himself would make today's administrative trainees envious indeed. Many of those who passed the examinations became sub-prefects, as one-quarter of the subprefectoral positions were reserved for auditors. Then, if successful, they could be promoted to the rank of prefect. There could not possibly be better preparation for a prefecture than the duties of subprefect or intendant of a conquered province.

Often the program was not followed. Young men were sometimes promoted before they were ready and some members of the administration were appointed auditors without receiving the training. But several careers will show clearly the type of training that some of these young men received before becoming prefects. Rouen des Mallets entered the auditoriat in 1809 and spent two years as intendant of Ragusa and Carniole before becoming

prefect in late 1813.<sup>51</sup> After formal education in the École polytechnique and the École des ponts et chaussées, Camille Basset de Chateaubourg trained for two years in the auditoriat before becoming subprefect in 1806. Seven years later he was promoted to the prefecture of the Vendée.<sup>52</sup> G. J. A. Stassart trained in the auditoriat for two years, spent 1806–07 as intendant of the Tyrol, Elbing, Königsberg, East Prussia, and then Berlin, and was subprefect for three years before becoming prefect of the Vaucluse.<sup>53</sup> The real influx of auditors began with the appointment of twelve men in 1810, eight more in 1813, and a further six in 1814. By 1813, 32 of the 130 prefects were auditors. In all, 42 auditors were promoted to prefect, 18 of whom had been subprefects. However, these 42 were just an anticipation of what would have come. With one-quarter of the subprefectures reserved for the auditors, Napoleon obviously intended eventually to recruit almost all prefects from the auditoriat by way of the subprefectures.

The prefects appointed in 1810–14 were the best-trained men ever to enter the corps under Napoleon—in fact they were possibly the best-trained prefects to enter the corps in the nineteenth century. Alas, the whole system was abolished in 1814. The reason, sad to say, was the rise of representative democracy. In France regular elections began under the Restoration. Almost the first consequence was that the prefects were told to deliver the vote to the government candidates. A change of government, therefore, meant a change of prefects. In this way the chief connection of the prefectural corps came to be with the government and the Assembly and not with the subordinate levels of the administration. Siwek-Pouydesseau reports that from 1876 to the Vichy regime only a small fraction of subprefects was recruited from the administration. “Before 1928, no test of ability was demanded for entrance into the sub prefectural corps. Political patronage was the only means of obtaining these positions.”<sup>54</sup> Since the chief activity of the prefects was political, a system of administrative training was pointless. It was only in the 1920s and 1930s that the French administration began developing a system of formal training and promotion for the prefects reminiscent of the auditoriat.<sup>55</sup> The British civil service did not adopt similar standards until the mid-nineteenth century.<sup>56</sup> Napoleon’s system, although faulty in its application and subject to abuses, was already well developed by 1810 and was providing an increasing proportion of prefects by 1812. Markham claims that with the auditoriat “Napoleon was at least fifty years ahead of his time

<sup>51</sup> F1b1 177–17.

<sup>52</sup> F1b1 156–8.

<sup>53</sup> F1b1 173–20.

<sup>54</sup> Siwek-Pouydesseau, *Le Corps Préfectoral*, 35.

<sup>55</sup> For the reforms in the French prefectural corps and administration in the 1930s see W. A. Sharp, *The Government of the French Republic* (London, 1938), 156–57; and Siwek-Pouydesseau, *Le Corps Préfectoral*, 46 ff.

<sup>56</sup> Edward Bridges, “The Reforms of 1854 in Retrospect,” in Robson, *The Civil Service*.

in his conception of a trained higher civil service."<sup>57</sup> In terms of the prefectoral corps, he appears to have been closer to a century ahead of his time.

The quality of Napoleon's prefectoral corps is reflected in one professional source of information that is especially interesting because it has not been seriously considered by historians. From time to time the minister of the interior prepared for Napoleon a resumé of his opinions on all the prefects. These assessments contained his opinions on each prefect's administrative ability, whether he was too old or too young, his relations with his *administrés* or with various groups in the department, whether he was respected, whether he was having difficulties, whether he should be promoted, transferred, retired, or dismissed. In one sense these opinions are subjective, the opinions of only one man toward his fellow administrators. On the other hand these assessments are highly professional, the reports of a minister on his immediate subordinates submitted to a ruler who knew many of the prefects personally, who had read many of their reports, and who would not likely be swayed by the minister's personal bias. It is on these professional reports that many prefects were promoted or dismissed, so they provide a useful commentary on the government's opinion of their quality. Unfortunately, they were not submitted regularly. And, naturally, different ministers held different opinions. Several have been published, especially those for 1800, 1812, 1813, and for the time of individual appointments.<sup>58</sup>

The reports are far too extensive to analyze here in detail. But after reading through them it is apparent that most prefects were well qualified in the eyes of the administration. There is no evidence in these reports that the bureaucracy was deteriorating. Some poor prefects were singled out in the early period. Jean Huguot, for example, was "surrounded by enemies of the government," and Pierre Montaut-Desilles "had been accused of feebleness." In the latter period other prefects were found wanting. The auditor Jean Duval was "a little light." Joseph Frain had been in the corps since 1800, but by 1813 it was noted that "his administration is lifeless. . . . He is not a bad man, he is just a nobody."<sup>59</sup> Prefects thus described usually disappeared quickly from the administration. In the latter years the criticisms tended to be levied against the older prefects, precisely those people who are held out by some historians as being the best administrators. In previous reports they had proven satisfactory, but they were growing tired. The reports do not suggest any deterioration and, on the contrary, suggest that a number of inadequate appointees of the early years were eliminated and that the prefects who grew feeble were progressively retired. This evidence suggests an administration that was constantly being examined, constantly renewed, and gradually improving or remaining constant through the elimination of ineffective administrators.

<sup>57</sup> Markham, *Napoleon*, 99.

<sup>58</sup> Savant, *Préfets*, 218-321.

<sup>59</sup> *Ibid.*, 220, 271, 224, 225.

To sum up, the founding of the prefectural corps was one of the most lasting of Napoleon's achievements. Napoléon began with 97 men who were mainly bourgeois in social origin; who were usually in their thirties or forties; who were lawyers, politicians, and, to a lesser extent, administrators and soldiers; and who were usually appointed to a department in the region of their birth. As time went on the nature of the corps changed perceptibly. The bourgeois remained in the majority, but the corps came to be more evenly balanced after the early influx of the nobles. It became a successful example of the fusion of noble and bourgeois, of Revolution and *ancien régime*. Many of the lawyers and politicians retired, transferred, were promoted, or died. Their places were often taken by trained and experienced administrators. After several years of confusion and turnover the prefectural corps developed marked characteristics often identified as the features of modern bureaucracy, and indeed as the chief advantages of modern bureaucracy. The corps became stable, more professional, and more experienced; it became subject to fairly regular evaluation, with the advancement of careers based on promotion through the ranks; a formal system of training was acquired that was not re-established until the mid-twentieth century. It has long been argued that the prefectural corps, in common with the whole Napoleonic administration, deteriorated after the early years of excellence. This argument has been everywhere asserted and repeated but nowhere proven or documented. Much of the evidence adduced for it is fragmentary or contradictory. The basic argument for deterioration has been that "docile" nobles replaced "excellent" bourgeois Revolutionaries. This is to turn the *ancien régime* upside down with the bourgeois now entitled and qualified to rule by nature of their birth! It is an argument that appears to be founded on nothing more substantial than a prejudice. The application of more acceptable criteria for administrative quality reveals a Napoleonic prefectural corps that gradually and steadily improved throughout its fifteen-year existence.

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## The Cold War Warmed Over

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A Review Article by WARREN F. KIMBALL

JOHN LEWIS GADDIS. *The United States and the Origins of the Cold War, 1941-1947*. (Contemporary American History Series.) New York: Columbia University Press. 1972. Pp. ix, 396. Cloth \$12.50, paper \$3.95.

JOYCE and GABRIEL KOLKO. *The Limits of Power: The World and United States Foreign Policy, 1945-1954*. New York: Harper and Row. 1972. Pp. xii, 820. Cloth \$15.00, paper \$6.25.

ROBERT JAMES MADDOX. *The New Left and the Origins of the Cold War*. Princeton: Princeton University Press. 1973. Pp. ix, 169. \$7.95.

IN GENERAL, cold war historiography has followed a pattern familiar to diplomatic historians. First come the "white paper" style histories, often written during a particular crisis and reflecting the view of government policy makers. Close on the heels of official and semiofficial apologies come the initial attempts at revision. Limited by the lack of evidence, these studies frequently spring from partisan politics and/or personal opposition to administration policy, and they are often characterized by visions of conspiracy. At this point defenses of official policy appear. Based on greater amounts of documentary evidence they often accept a few of the revisionist arguments while rejecting the revisionist's overall conclusions. These scholarly defenses bring on scholarly revisionism—though usually far more restrained in tone than the earlier critiques. Finally, time plus the desire to say something new results in an eclectic synthesis that, though based upon an examination of documentary materials, seems to approach the golden mean by extracting arguments and conclusions from all sides. Thereafter the last three steps are repeated again and again, though often rhetoric overpowers scholarship.

This general progression is not, of course, rigidly adhered to. Graduate

I wish to express my thanks to faculty members of the Rutgers-Newark history department seminar for their help. I am particularly grateful to my colleagues, Elliot Rosen and Herbert Meritt, as well as Professor Charles C. Alexander of Ohio University for having read and critiqued the entire manuscript.



professors all too frequently train rather than educate their students, and this results in the creation of long-lived schools. More importantly, external events often shape intellectual attitudes. Just as World War II affected the perspective from which most historians looked at World War I, so the Vietnam War brought forth from the left a strong and intense critique of American policy as it related to the origins of the cold war.

The three books under review, however, do fit the pattern fairly well. Joyce and Gabriel Kolko's *The Limits of Power*, which is a continuation of Gabriel Kolko's *The Politics of War*,<sup>1</sup> represents a left revisionist response to the scholarly defenses of official policy that dominated American diplomatic histories prior to the Vietnam War. Robert J. Maddox's attack on the work "of the most prominent New Left" historians fits in with the old orthodox defenses of American policy, even though Maddox is sometimes erroneously labelled a "younger scholar."<sup>2</sup> John L. Gaddis's *The United States and the Origins of the Cold War, 1941-1947* epitomizes the eclecticism of a new generation of scholars just beginning to make its mark.<sup>3</sup>

Although review articles are often intended as state-of-the-literature pieces, some of the routine of normal book reviewing is required, particularly in these three cases. Joyce and Gabriel Kolko posit a simplistic theme reminiscent of Arthur Schlesinger, jr.'s unsophisticated definition of liberalism as "ordinarily the movement on the part of the other sections of society to restrain the power of the business community."<sup>4</sup> The Kolkos merely include the liberals as part and parcel of the business community and hence the villains of the piece: "As we shall see repeatedly, it is the expansive interests of American capitalism as an economy with specific structural needs that guide the definition of foreign economic policy and the United States' larger global role and needs."<sup>5</sup> Since the goals of American policy are dictated by institutional necessity, they specifically reject the notion that American society can change its direction merely by electing good men. At the core of all American programs and policies during and

<sup>1</sup> Gabriel Kolko, *The Politics of War: The World and United States Foreign Policy, 1943-1945* (New York, 1968).

<sup>2</sup> Maddox was born in 1931 and attended the University of Wisconsin (Madison) and Rutgers University (New Brunswick) at the same time as many of the revisionists he critiques. The "younger historian" tag is attached by John L. Gaddis, "Checking History's Footnotes: *The New Left and the Origins of the Cold War*," *Book World*, May 6, 1973.

<sup>3</sup> Other examples of eclectic scholarship are George C. Herring, Jr., *Aid to Russia, 1941-1946: Strategy, Diplomacy, and the Origins of the Cold War* (New York, 1973); Thomas M. Campbell, *Masquerade Peace: America's UN Policy, 1944-1945* (Tallahassee, 1973); both accept many of the left revisionist arguments while denying the ideological underpinnings. Herbert Feis's *From Trust to Terror: The Onset of the Cold War, 1945-1950* (New York, 1970), though admittedly written as a refutation of the left revisionist interpretation, is a classic if occasionally amusing example of creeping eclecticism. In spite of stridently antirevisionist conclusions and footnotes, Feis repeatedly agrees with many of the most important left revisionist arguments regarding economic diplomacy. Considering Feis's earlier historical efforts, such as *The Diplomacy of the Dollar, 1919-1932* (Baltimore, 1950), it is not surprising that such data seemed important—even if his conclusions often disregarded it.

<sup>4</sup> Arthur Schlesinger, jr., *The Age of Jackson* (Boston, 1945), 505.

<sup>5</sup> Kolko and Kolko, *The Limits of Power*, 8.

after World War II, the Kolkos feel, was a simple, carefully formulated objective—"to restructure the world so that American business could trade, operate, and profit without restrictions everywhere." Unfortunately American leaders failed to comprehend fully the political requirements of such a goal. The result was much more than a struggle with the Soviet Union for power—in fact the Kolkos find that battle only a convenience for American leaders—rather, it was a conscious attempt by the United States to crush the "Left" (the term is always capitalized by the Kolkos). The Kolkos find World War II merely a prelude to the "irreversible" changes in society that are presently occurring. Clearly this view reflects no "tragedy of American diplomacy"—with the implication of lost opportunities for change—but is instead the old Hegelian "world force," moving events irresistably, inevitably, and mechanistically toward their predetermined end.<sup>6</sup> So much so, in fact, that the Kolkos' arguments begin to seem counter-revolutionary and to militate against the development of any radical consensus. Why construct a radical position when the triumph of the left is absolutely inevitable? A tragedy exists only after a person or society has made a ruinous choice—but the Kolkos merely chronicle the pre-ordained decline of the American nation.

In spite of such clear philosophical views the entire book suffers from the lack of coherent, consistent, usable definitions. Although the "Left" is defined by the Kolkos as the rising working class in Europe, it is also identified with anticolonialism in Asia and elsewhere. Combining as complex a group as the European working class with the Asian, African, and the Latin American peasantry is intellectually indigestible. Whether such confused concepts stem from intellectual or linguistic problems is difficult to determine, for one of the most frustrating features of the book is its turgid, repetitive, imprecise prose.<sup>7</sup> For example, in deprecating the "cold war" as an unhelpful term, the authors assert that the "more significant context for understanding postwar history is the entire globe and the revolution, the counterrevolution, and the great, often violent, interaction between the United States, its European allies, and the vast social and economic transformation in the Third World *that is the defining fact of our world.*"<sup>8</sup> I think I know what they are trying to say, but who can say for certain without precise instructions just what is modified by the italicized phrase. When "the defining fact of our world" is identified, we should have no doubt as to exactly what it is.

<sup>6</sup> The above discussion is all taken from the introduction to *ibid.*, 1–8. Although I deal with *The Limits of Power* primarily as it relates to the debate over the origins of the cold war the book also deals with American foreign policy during the period of the institutionalization of the cold war—from 1947 through 1954.

<sup>7</sup> Every student, without exception, in my recent seminar on cold war historiography at Rutgers University has agreed with the assessment of the book as unreadable. That includes a number of students who enthusiastically endorsed the Kolko's interpretations. I am indebted to those students for their stimulating analyses of the literature on the cold war. I am particularly grateful to Nancy Beck who will recognize many of the points she made in her seminar paper. The definitions are in Kolko and Kolko, *The Limits of Power*, 4.

<sup>8</sup> *Ibid.*, 12 (italics added).

Similar examples abound. The role of public opinion in the late 1940s is briefly analyzed and dismissed because "over three-quarters of the nation simply paid no attention to foreign affairs and was apathetic or ignorant toward it." Yet a few sentences later American leaders are pictured as concerned with achieving their goals "over the indifference or opposition of the majority of the people."<sup>9</sup> Do or do not the people care? This is not manipulation or distortion; it is simply poor writing. A good editor—one who would catch such contradictions and drastically cut the length of the book—would have been a blessing.

The rigidity with which the Kolkos apply their ideological framework results in some curious interpretations and undocumented conclusions. The chapter dealing with the outbreak of the Korean War is a good example. The sum of their arguments is that the Korean War was a logical and understandable response to the military build-up in South Korea; that American and South Korean intelligence experts knew of the North Korean build-up, but apparently said nothing; that an easy series of victories by North Korea served the interests of the South Korean president, Syngman Rhee, since they would eventually deeply and irrevocably involve American forces in the struggle; and that the whole crisis was part of some vague conspiracy designed to make American fears of a worldwide Communist plot a self-fulfilling prophecy. This entire presentation is based primarily on the basic philosophy underlying the entire book and the fact that military reports during the first days of the crisis were obscure and contradictory.<sup>10</sup> If contradictory military reports during a surprise attack are proof of conspiracy then historians have much rewriting to do.

In one sense this is a most honest presentation, for the Kolkos lay out their ideological preconceptions for all to see. If you accept their notion that mankind cannot really affect its fate, and that one can therefore select any individual government official and present his or her views as those of "Washington," then their book is persuasive. What makes American readers so very uncomfortable is their method of isolating selfish, usually economic, motives behind the more hallowed episodes and aspects of American foreign policy. Thus the Marshall Plan, which was originally painted as sheer altruism and which later historians saw as part of an overall economic recovery plan for both Europe and America, becomes a quasi-conspiratorial scheme for the penetration of Europe by American capital. Intervention in Korea is no longer a defense against aggression, but instead a means of propping up a pro-American, right-wing tyrant. Anticolonialism, once thought a bulwark of traditional United States foreign policy, becomes merely a façade designed to hide the real American goal—the displacing of British colonialism by American imperialism. All this occurs because the Kolkos impose their own ideological orthodoxy upon their historical sub-

<sup>9</sup> *Ibid.*, 333.

<sup>10</sup> *Ibid.*, 574–85.

jects. In accepting an imperative determinist framework they apparently fail to understand that humans regularly act from a complex set of often contradictory motives; thus the Kolkos suffer from the same limited horizons as the liberal historians they argue against.

Even with these faults the book adds much to our knowledge. Their investigation of those motives they find important is thorough, and their evidence forces us to integrate the economic and social drives that underlay much of our foreign policy with the more traditional treatments of power and politics. As a bibliographical guide alone the book provides a definitive compilation of the primary sources available for studying American foreign policy from 1945 through 1954.

John L. Gaddis's *The United States and the Origins of the Cold War* has been hailed as "a successful response to the current revisionist fad."<sup>11</sup> Yet time and again Gaddis accepts many of the most significant left revisionist arguments. Granted, he puts them in a broader context, re-emphasizing politics, power, and domestic opinion, but throughout it all economics plays a primary role. Had Gaddis, six or seven years ago, presented the idea that "Washington chose to withhold the one instrument which might have influenced Soviet economic behavior—a postwar reconstruction loan—in hopes of extracting political concessions,"<sup>12</sup> his work would today be interpreted as an example of New Left history. The applause from some quarters for Gaddis's work as an answer to the left revisionists stems primarily from a tendency to lump such diverse historians as Gar Alperovitz, Lloyd Gardner, and Gabriel Kolko under one label, instead of evaluating each as an individual.

Gaddis's contribution is primarily his eclectic combining of the best of recent scholarship. He has used the documents extensively, but his interpretations show the cumulative effect of twenty-five years of historical writings on the cold war. He finds that the basic American goals during World War II began, logically enough, with a commitment to win the war and conquer the enemy. The next goal, that of achieving a permanent peace, fostered three specific policies: the promotion of self-determination, cooperation with the United Nations, and the prevention of future economic depressions—all worldwide problems. The last major objective, the creation of a cooperative relationship between the major powers, stemmed from the realization that such cooperation was necessary for the attainment of the other goals.<sup>13</sup>

Gaddis reconciles his orthodox and revisionist interpretations by placing Roosevelt's priorities in descending order of importance: military victory overshadowed all else, coalition diplomacy as it related to the wartime

<sup>11</sup> John W. Spanier in a review in *Journal of American History*, 59 (1972): 768-69.

<sup>12</sup> Gaddis, *Origins of the Cold War*, 23.

<sup>13</sup> *Ibid.*, 2.

alliance came next, and postwar diplomacy came last.<sup>14</sup> Like some revisionists, he finds great consistency between Roosevelt's and Truman's policies, though he seems to contradict himself when he agrees that Truman was strongly influenced by Harriman and Forrestal to move toward a hard-line approach.<sup>15</sup> He makes sense out of the knotty problem of American policy in Eastern Europe by identifying two fundamentally contradictory goals: self-determination and the unity of the Grand Alliance. Since Gaddis earlier identified open channels of trade as a way Americans sought to promote peace, self-determination can be seen as a means of guaranteeing the existence of such open channels in Eastern Europe. Given the fact that by late 1943 Roosevelt had tacitly agreed to Soviet domination of Eastern Europe, any attempt to push for self-determination was bound to weaken or destroy Soviet-American trust.<sup>16</sup>

If there is one basic reason for the development of the cold war, Gaddis finds it in Roosevelt's refusal to correlate his private diplomacy with his public policies. Gaddis can accept the normal tensions that go with a world of nation states, but he finds the special nature of the cold war in America to be a product of Roosevelt's consistent pie-in-the-sky rhetoric about the future of international relations. Maybe the real difference between FDR and his successor was that Truman operated from the assumption that Roosevelt meant what he had said about demanding Soviet cooperation, while Roosevelt seems to have been engaged in but another campaign to educate public opinion.

Although Gaddis refrains from any further evaluation of Roosevelt's tactics in foreign policy, the mere recounting of the facts adds much to our knowledge of what Arthur Schlesinger, jr. recently called the "imperial presidency." To follow the twists and turns of something like American policy regarding postwar Poland—implying one thing to the Russians, another to the British, and still another to the American public—is to come to believe that the only thing that saved Roosevelt from the same loss of credibility as Lyndon Johnson is that FDR won his war.

The books by Gaddis and the Kolkos are both important, thought-provoking studies that offer positive theses and *ad rem* argumentation. While they often disagree totally on the interpretation given the same set of facts, each can be read and studied with great profit. The other book under review is essentially a historiographical critique and thus requires somewhat different treatment.

Robert J. Maddox, in *The New Left and the Origins of the Cold War*, raises a set of disturbing questions for our profession. Maddox examines portions of the work of "seven of the most prominent New Left" historians—William A. Williams, D. F. Fleming, Gar Alperovitz, David Horowitz,

<sup>14</sup> This is true throughout the book, but see particularly *ibid.*, 63-94.

<sup>15</sup> *Ibid.*, 215, 202-05, 230.

<sup>16</sup> *Ibid.*, 21, 133-34.

Gabriel Kolko, Diane Shaver Clemens, Lloyd C. Gardner. He concludes that their "books *without exception* are based upon pervasive misusages of the source materials. . . . Even the best fails to attain the most flexible definition of scholarship. Stated briefly, the most striking characteristic of revisionist historiography has been the extent to which New Left authors have revised the evidence itself."<sup>17</sup> Since Maddox assumes that these seven writers make up a representative selection of New Left historians his charge is of enormous import for it encompasses, by inference, a vast body of historical literature dealing with all aspects of American history. Because Professor Maddox's major thrust concerns the misinterpretation of documentary evidence it is perhaps best to let him speak for himself.

He begins by dividing the New Left revisionists into two groups: "The 'soft' revisionists [who] place far more emphasis upon individuals than they do on the nature of institutions or systems" and the "hard" revisionists who argue that "the Cold War was the inevitable result of the American system as it developed over the years." Since "most of the revisionists frankly proclaim that they conceive of their work as a tool for change," Maddox concludes that they believe "the historian's job, therefore, is to create a version of the past which can be used to help achieve the goals his ideological preferences dictate." But Maddox does not limit his criticism solely to the predilections and goals (as he sees them) of New Left historians. After citing comments by Williams and Horowitz, which set forth their reasons for writing an interpretation of American cold war policy, Maddox also infers a professional insult. Their "conclusion," he says, "is inescapable: only the revisionists possess sufficient courage to reveal truths which must have been obvious to even the most dull-witted orthodox scholar."<sup>18</sup>

In addition to accusations of misuse of evidence Maddox also criticizes the significance that revisionists attach to certain events. "Revisionists almost always employ a double-standard: Russia's actions are justified or explained by reference to national security or *Realpolitik*, Western actions are measured against some high ideal and found wanting." Maddox then gives a few brief examples of revisionist use of loaded words to misrepresent the facts, such as Kolko's "outrage" at the suppression of the left in Greece, compared to his downplaying of the Katyn Forest "murder," and the New Left view that "the United States invariably wishes to 'penetrate' Eastern Europe whereas the Soviet Union seeks merely 'economic partnership,' a semantic distinction Eastern Europeans might grimly enjoy."<sup>19</sup>

<sup>17</sup> Maddox, *The New Left*, 10-11. Maddox discusses the following books: William A. Williams, *The Tragedy of American Diplomacy* (rev. ed.; New York, 1962); D. F. Fleming, *The Cold War and Its Origins*, 2 vols. (New York, 1961); Gar Alperovitz, *Atomic Diplomacy* (New York, 1965); David Horowitz, *The Free World Colossus* (New York, 1965); Diane Shaver Clemens, *Yalta* (New York, 1970); Lloyd C. Gardner, *Architects of Illusion* (New York, 1970); and Kolko, *The Politics of War*. I have referred to the edition Maddox cited most frequently, though he does make occasional references to other editions of those books.

<sup>18</sup> Maddox, *The New Left*, 4-6.

<sup>19</sup> *Ibid.*, 7-8. "Outrage" and "murder" are Maddox's terms.

After this tantalizing digression into the realm of differing interpretations (for surely the choice of emphasis is just that) Maddox returns to his basic argument: that New Left historians have distorted the evidence.

Before examining a few examples of Maddox's specific charges some general comments on his overall approach are in order. First, and most weighty, Maddox clearly implies that distortion of the historical record by the New Left was conscious and with malice aforethought. Although Maddox carefully avoids such a clear and unequivocal accusation the reader can entertain no doubts as to what he believes. At a recent meeting of the Columbia University American Studies Seminar (October 18, 1973) Maddox refused to affirm or deny such a view. His examination of the evidence, however, follows a discourse clearly criticizing the revisionist concept that history is a tool for change. Taken within the context of the book, that obviously implies that New Left historians care little about truth and hence would alter the evidence to suit their political purposes. Maddox comes close to leveling squarely the accusation in his closing sentence: "Perhaps, after all, the New Left view of American foreign policy during and immediately after World War II can *only* be sustained by doing violence to the historical record."<sup>20</sup> In a nutshell, Maddox has impugned the ethics and honesty of seven people by name and many others by implication. Those are not charges that can be lightly dismissed, nor can they be defended on the principle of the "greater good"—that is, by criticizing Maddox's vindictiveness, but then praising him for bringing about higher standards of scholarship. That sort of logic smacks too much of the defenses (made in the name of national security) of Senator Joseph McCarthy's actions, or of burning down school buildings in the name of education.

Every charge and specification leveled by Maddox cannot and should not be reviewed in this article since Maddox's method is to check footnotes and then determine whether or not the author misused the document.<sup>21</sup> He is particularly sensitive to partial quotations that he claims were used in order to fit the evidence to a preconceived pattern. Since Maddox questions the validity of the work of seven people based upon an

<sup>20</sup> *Ibid.*, 164.

<sup>21</sup> Those who wish to examine Maddox's charges in full detail will eventually have to gather together all the documents and dig through them. Some of the people criticized by Maddox have gone to great lengths to answer his specific accusations, and those answers would be a good place to begin a full and detailed analysis. Maddox's critique of Gar Alperovitz's *Atomic Diplomacy* appeared a few weeks before his book under the title "'Atomic Diplomacy': A Study in Creative Writing," *Journal of American History*, 59 (1973): 925-34. Alperovitz's response is in *ibid.*, 1062-67. See also Maddox's earlier summary of his own book published as "Cold War Revisionism: Abusing History," *Freedom at Issue*, Sept.-Oct. 1972, pp. 3 ff. In addition to the brief and incomplete responses from each of the seven subjects following an enthusiastic review of Maddox's book by Francis Loewenheim in the *New York Times Book Review* of June 17, 1973, three other responses have been privately reproduced and distributed. Although numerous copies are in circulation, the easiest way to obtain one would be to write directly to the three who have written them: Lloyd C. Gardner, Gabriel Kolko, and David Horowitz.

examination of only a tiny segment of their research (he concentrates almost exclusively on the period from the Yalta Conference in February 1945 to the Potsdam Conference in July-August 1945), it would seem fair to apply the same limited examination to his book—for in this sort of situation the accuser must be like Caesar's wife.

That task would be easy except that Maddox has chosen to group a curiously mixed bag. He seems to understand that those revisionists he critiques are "by no means monolithic in their own interpretations," but then muddies the water by ascribing some sort of unity with the assertion that "the New Left revisionists collectively have mounted a formidable attack."<sup>22</sup> His potentially useful division of revisionists into "hard" and "soft" categories might have established the philosophical foundation the book so sorely lacks, but his definitions are unsophisticated, and he fails to relate those terms to the people under discussion. The sense of confusion deepens when one reads the opening sentences of the chapter examining D. F. Fleming's study of the cold war:

Pinning labels on historians is a hazardous enterprise at best, but in general the terms "New Left" and "revisionist" are synonymous when applied to interpretations of how the Cold War began. D. F. Fleming is an exception. He wrote his massive, two-volume *The Cold War and Its Origins* as an unreconstructed Wilsonian, not as a critic of the American system as such.<sup>23</sup>

Historians should all applaud the definition of labelling as "hazardous"; they can only note with regret that Maddox failed to heed his own advice. Maddox believes Fleming's "interpretations of specific issues have become standard New Left historiography," which seems perilously close to guilt by association since both Maddox and those he attacks agree that Fleming proceeds from ideological premises vastly different from those of Williams, Gardner, Kolko, *et al.* If Fleming is admittedly not New Left, then why is he included in an examination of evidence titled *The New Left and the Origins of the Cold War*?<sup>24</sup> The answer seems to be that Fleming is a convenient straw man. The historical profession's opinion of the scholarly reliability of Fleming's study is best indicated by its disappearance from

<sup>22</sup> *Ibid.*, 3. The "hard" and "soft" labels are discussed on pp. 4-5. Williams himself seems to recognize the need for careful distinctions. He recently criticized Lloyd Gardner's emphasis on "the extent to which American policy makers were coping with external events rather than moving to impose their will upon reality." W. A. Williams, ed., *From Colony to Empire* (New York, 1972), 6. Maddox's belief that Williams provided the inspiration for "a number of younger scholars—some his own students" (*The New Left*, 13) suggests a quick review of the educational background of the other six authors Maddox discusses. Of the six, only Lloyd Gardner studied with Williams. As Norman Graebner has found: "Wisconsin graduates . . . do not dominate the New Left." Graebner, "The State of Diplomatic History," *The Society for Historians of American Foreign Relations Newsletter*, Mar. 1973, p. 4.

<sup>23</sup> Maddox, *The New Left*, 39.

<sup>24</sup> Indeed, Fleming's influence on the others Maddox examines is questionable. Other than a brief historiographical reference Fleming's book is cited in only one footnote and in only one bibliography out of the other six books.



recent bibliographies. Maddox hardly provides a service to the profession by including it in a study of New Left perfidy.

The inclusion of David Horowitz's *The Free World Colossus* in an attack on New Left scholarship is likewise curious. Though Horowitz attached the paraphernalia of scholarly apparatus the historical profession has rarely treated the book as a piece of objective research.<sup>25</sup> Neither edition was reviewed in either the *American Historical Review* or the *Journal of American History*. As Maddox correctly points out, Horowitz failed to use many of the printed primary sources, particularly the relevant volumes from *Foreign Relations*. To include an admittedly nonscholarly account in a critique of New Left scholarship is simply invalid.

Maddox's specific accusations begin with an examination of *The Tragedy of American Diplomacy* by William A. Williams. As in all the succeeding chapters Maddox deals with only that tiny portion of the book that posits a strong American interest in Eastern Europe, American violation of the Yalta accords, and a belligerent anti-Soviet policy on the part of Harry Truman. Maddox's complaints center around Williams's use of *ad seriatim* quotations, a method that troubles many historians including some of Williams's own students. There is no question that Williams strings together series of quotations on certain subjects without telling the reader when, where, and why they were said. The failure of Williams and/or the publisher to provide useful citations reduces the scholarly usefulness of the book. Nevertheless Williams's response to Maddox's criticisms is worth quoting: "Historians, like literary critics, sociologists, psychiatrists, or anyone else struggling to make sense out of reality, use *seriatim* quotations to document, illustrate, and communicate to the reader the substance and texture of the *Weltanschauung* of the protagonists—of the way the actors make sense out of reality."<sup>26</sup>

While it is easy to attack the stream of consciousness arguments of Williams as a total recreation of the physical or verbal facts, intellectual history—for that is what Williams writes—simply cannot be judged in that way. The probing of human minds requires the accumulation and study of the facts, but the key to true and useful intellectual history—as opposed to mere literary history—is the intuitive ordering of facts and ideas. No one is obliged to accept the interpretation that follows, but disagreement does not justify name-calling. Nor is Maddox accurate even in his footnote checking. He states that "Williams argued that the United States itself bore the primary responsibility" for bringing about the cold war. In fact,

<sup>25</sup> Of the others treated in Maddox's book none used *The Free World Colossus* as a source.

<sup>26</sup> William A. Williams, *New York Times Book Review*, June 17, 1973, p. 7. Williams's use of *ad seriatim* quotations can and has been effectively challenged, but only when the challenge is based on a full, scholarly appreciation of Williams's avowed methodology—not on the basis of shadowy allegations about motives. See J. A. Thompson, "William Appleman Williams and the 'American Empire,'" *Journal of American Studies*, 7 (1973): 91-104, for a balanced assessment of Williams's methods and conclusions.

Williams specifically denies that notion by pointing out that to say the United States "crystallized the cold war . . . is not to say that the United States started or caused the cold war." Rather the cause was Hitler's destruction of the existing structure in Eastern Europe. What Williams specifically rejects is not the idea of blaming the Soviet Union for contributing to cold war tensions, but the claim that Russia was *solely* to blame.<sup>27</sup> Given the state of conventional historical wisdom in 1959, when Williams's essay was published, it is not surprising that he paid less attention to Soviet than American contributions to the cold war.

The most serious flaw in Maddox's reading of Williams's argument, however, is his failure to comprehend how Williams uses the term "Open Door." He notes that Williams claims the policy had become "internalized," hence there was little need to discuss or defend it. Maddox claims Williams then contradicts himself by using "copious quotations" to show how Henry Stimson and W. Averell Harriman had to tutor Truman and his secretary of state, James Byrnes, about the Open Door in East Asia; for if the policy were so completely "internalized," Maddox argues, why would there be any need for further persuasion? That argument simply ignores the enormous difference between Williams's broad use of the phrase "Open Door" to describe the basic thrust behind American foreign policy and the discussion of tactics as well as the education of an inexperienced president concerning the traditionally held Open Door in China. The use of one term to describe two different levels of thought and action is both annoying and confusing, but that hardly warrants accusing Williams of "pervasive misusages of source materials."<sup>28</sup>

Equally interesting is that, while Maddox claims not to be concerned with differences in interpretation, it appears that just such differences lie at the heart of his criticism of Williams. Even when seemingly merely checking footnotes Maddox essentially deals with interpretations. In dismissing a summary of the foreign policy dispute between Henry Wallace and President Truman as "solemn nonsense," Maddox disagrees with Williams's interpretation of the basic nature of Truman's policy toward the Soviet Union. Maddox correctly points out that Wallace did not specifically refer to a loan to Russia in his letter of March 14, 1946, but the secretary of commerce did characterize the overall tone of American-Russian relations, and that general characterization is the issue with which Williams was concerned.<sup>29</sup>

Maddox begins his discussion of Gar Alperovitz's *Atomic Diplomacy* by stating that the "book has become a staple of 'New Left' historiography." That is a curious position since Clemens, Gardner, Kolko, and Williams—either specifically or by inference—have rejected Alperovitz's basic theme

<sup>27</sup> Maddox, *The New Left*, 14; Williams, *Tragedy of American Diplomacy*, 206-07.

<sup>28</sup> Maddox, *The New Left*, 16-19, 10.

<sup>29</sup> *Ibid.*, 34-36.

that Harry Truman radically altered the cooperative policies of Franklin Roosevelt toward Russia.<sup>30</sup> Since, as the title suggests, Alperovitz's entire thesis stands on the belief that Truman reversed Roosevelt's policies by practicing atomic diplomacy, Alperovitz seems to have "become a staple of 'New Left' historiography" only in the negative.

To return again, however, to the specifics of footnoting, Maddox claims that "one of the most common flaws in the book is Alperovitz's practice of citing statements in support of his arguments which, in context, refer to other subjects altogether." Truman's statement that the Russians "could go to hell," associated by Alperovitz with the president's views on the question of the reorganization of the Polish government, refers, according to Maddox, to the question of boycotting the San Francisco conference. Since Maddox follows by agreeing that Truman understood the Russians might boycott the conference if they did not get their way on the Polish question, Maddox seemingly contradicts himself.<sup>31</sup>

In spite of repeated protestations to the contrary,<sup>32</sup> Maddox deals primarily with interpretations—not falsification of the evidence. As Alperovitz showed in his rejoinder to Maddox's critique their differences related to the use of ellipses and the relationship between specific statements and broad policies. For example, Maddox claims that Alperovitz distorted history by applying a statement of Averell Harriman to Lend-Lease when it actually referred to postwar credits to Russia. That is simply not what Alperovitz did. Granted, Harriman was referring to postwar credits and not Lend-Lease, but as anyone who has studied economic diplomacy during this period knows, the extension of Lend-Lease and the question of postwar credits and loans were essentially one in the same.<sup>33</sup> Maddox may well, on the basis of the evidence, disagree with the theory that the United States used economic aid as a club and thus helped precipitate the cold war, but he must also permit others to accept that view. It is worth noting that Professor George Herring, who is cited by Maddox as having rebutted "the notion that Lend-Lease curtailment was designed to coerce Russia," strongly agrees that Harriman developed a "sophisticated strategy" that, while oversimplified by the left revisionists, nonetheless included economic coercion as a key element. Herring, whose book is an admirable example of original research that arrives at an eclectic combination of orthodox and revisionist interpretations, demonstrates that the actual abrupt halting of Lend-Lease to Russia was not part of any overall plan for coercion, yet he also argues

<sup>30</sup> Kolko, *The Politics of War*, 421–22, 538–43; Williams, *Tragedy of American Diplomacy*, 239, 244; Gardner, *Architects of Illusion*, 305–06; Clemens, *Yalta*, 279; Maddox, *The New Left*, 63.

<sup>31</sup> Maddox, *The New Left*, 65.

<sup>32</sup> Statements by Robert J. Maddox at the Columbia University American Studies Seminar, October 18, 1973.

<sup>33</sup> Letter to the editor from Gar Alperovitz, *Journal of American History*, 59 (1973): 1062–67; Maddox, *The New Left*, 66–67; Maddox, "Atomic Diplomacy," *Journal of American History*, 59 (1973): 927.

that just such a general plan did exist even though it was only sporadically carried out.<sup>34</sup>

As with Alperovitz's book, Gabriel Kolko's *The Politics of War* presents a highly controversial and rigidly applied thesis about American foreign policy and diplomacy during World War II. And, as with his examination of Alperovitz's book, Maddox's critique is so narrowly conceived that it cannot come to grips with the broad context within which most of Kolko's arguments are made. Though Maddox claims to be checking footnotes his case actually rests upon his own interpretation of admittedly ambiguous documents. In a treatment of "Kolko's most striking misuse of evidence" Maddox rests his entire argument on what was meant by the Yalta agreement to give the Poles "substantial accessions" of land in the west. Kolko argues that Truman's opposition to the Soviet proposal of the Oder-Neisse boundary reversed "clear commitments" made at Yalta. Maddox, calling Kolko's account "bizarre," asserts that no such clearcut agreements were made at the Crimea Conference. In fact, if one consults the basic documents—the Yalta Papers—the entire situation becomes most ambiguous. Churchill's only stated objection to the Oder-Neisse boundary was the presence of a large German population that would have to be transferred. When Stalin answered that most of those Germans had fled before the Red Army, Churchill remarked that that "simplified the problem." Although Churchill later repeated that objection, no other substantive arguments were made at Yalta against the Soviet proposal. Kolko assumes that Stalin interpreted all that as tacit approval of his suggestion; Maddox assumes that the Anglo-American refusal to consent formally to the Oder-Neisse line made it clear to Stalin that "substantial concessions" meant only up to the Oder River.<sup>35</sup> One could easily quarrel with Kolko's claim that Roosevelt and Churchill had made "clear commitments" regarding the Oder-Neisse boundary, but Maddox eschews any such argument over interpretations. A narrow examination of footnotes in the absence of a full understanding of the entire context of a book is an inadequate critique. To make a proper determination of such arguments as Truman's alleged violation of the Yalta agreements requires the exercise of what George Dangerfield has recently called "historical imagination"—the ordering of evidence based upon the broad context as well as the specifics of the event.<sup>36</sup> Based on this

<sup>34</sup> Maddox, *The New Left*, 70–71n.; Herring, *Aid to Russia*, 208–211, 290–91.

<sup>35</sup> Maddox, *The New Left*, 117–18. Kolko, *The Politics of War*, 576–78. Kolko points out—and the documents verify—that the United States opposed any cession of land to Poland west of the Oder River, but would not insist on that point should both Russia and Britain agree on the transfer. Kolko never claims that there was any specific, unequivocal agreement made at the Yalta Conference about the Oder-Neisse line; see *ibid.*, 356–57, 403. The documents are in U.S. Department of State, *Foreign Relations of the United States* (hereafter *FRUS*), *Conferences at Malta and Yalta, 1945* (Washington, 1955), 716–17, 725–26, 898–99, 911–13, 917–18. Though my own reading of those documents differs from that of Kolko, it does not support a finding of conscious distortion in *The Politics of War*. See also, *FRUS, Conference of Berlin (Potsdam), 1945*, 1 (Washington, 1960): 750–51.

<sup>36</sup> Dangerfield in the *New York Times Book Review*, Oct. 28, 1973, p. 2.

limited type of investigation Maddox—with an ambiguity that would have made Franklin Roosevelt proud—casts doubts on Kolko's veracity:

In his acknowledgments to those who had aided him in preparing his manuscript, Kolko called special attention to a colleague's "critical commitment to seeking truth. . . ." "Whole sections" of *The Politics of War*, according to its author, were written in response to that commitment. The response, one must conclude, was singularly inappropriate.<sup>37</sup>

Maddox's treatment of Diane S. Clemens's *Yalta* closes with a similarly worded inference of conscious dishonesty. After mentioning a "multitude" of misuses of evidence, he claims that the frequent occurrence of such errors, "which invariably lend the appearance of substance to her themes, renders implausible the explanation of mere carelessness." Though he gallantly mitigates that attack by suggesting "'overzealousness' is the most charitable word," there is no doubt as to Maddox's meaning.<sup>38</sup>

As I have stated elsewhere I disagree with Clemens's thesis,<sup>39</sup> but that is a far cry from lightly veiled aspersions about her academic ethics. As for the substance of Maddox's critique once again he fails to distinguish between differing interpretations and dishonesty. Again, a single example. Clemens and Maddox each look at a statement by Harry Truman and interpret it differently. Maddox's approach is to look narrowly at the exact quotation, which concerns the Yalta agreement to reorganize the Lublin government in Poland. Clemens writes that Truman demanded a "new" government, whereas the Yalta agreements called for only a "reorganized" government. Maddox quotes both the Yalta agreement and Truman's note to Stalin and compares just the two documents. His conclusion is that Truman merely paraphrased the Yalta agreement on Poland and was only asking that Stalin live up to his promises. Clemens finds a different emphasis in Truman's note. She argues that the Yalta agreement called for the reorganization of the Lublin government and later referred to that broadened structure as a "new" government, whereas the Truman note called for the reorganization of the provisional Lublin government "in order to establish a new government."<sup>40</sup> One could argue endlessly as to whether or not Truman's word order constituted a proper paraphrase or a substantive change, but the only way to draw any valid conclusion is to exercise that indispensable "historical imagination." The diplomatic historian must always view the documents within their overall context, and mere semantic arguments are not persuasive. Maddox may not agree with Clemens's perspective, but he claims that he is not concerned with perspective and interpretations—only footnotes.

<sup>37</sup> Maddox, *The New Left*, 121.

<sup>38</sup> *Ibid.*, 137.

<sup>39</sup> Review in *International Journal*, 26 (1971): 812-15.

<sup>40</sup> *FRUS, Yalta*, 980; *FRUS*, 1945, 5 (Washington, 1967): 258; Maddox, *The New Left*, 125-26; Clemens, *Yalta*, 269.

Maddox saves Lloyd C. Gardner's *Architects of Illusion*, the New Left's "most sophisticated and convincing account" of the origins of the cold war, until last, but the conclusions are the same. "Subtler and more persuasive than previous revisionist works, . . . it is history by irrelevant evidence, misused quotations, jumbled figures, and distortions of documentary materials. Gardner . . . squandered his gifts."<sup>41</sup> Gardner certainly must have been quite sophisticated, for the first six pages of Maddox's twenty-page essay deal with what he claims he is not dealing with—interpretations. The conclusion of that six-page discussion is that "the lack of hard evidence caused Gardner's broader thesis to suffer from malnutrition."<sup>42</sup> In spite of his disclaimers Maddox's analysis of Gardner's positive evidence is likewise an exercise in differing interpretations. Again a single example will suffice. Maddox chides Gardner for incorrect placement of the word "only" in a statement about the decisions on Poland made at Yalta. Since the correction is immediately preceded by the statement that Gardner has "made significant contributions" to "revisionist legendry" and persistently misrepresented what happened at the Yalta talks it would appear Maddox believes this to be an example of such a "contribution."<sup>43</sup> Gardner wrote: "While F.D.R. did insist at this plenary session that there be free elections as soon as possible in Poland, the final protocol of the Yalta Conference stated *only* that the new Polish provisional government should be a 're-organization' of the Lublin government."<sup>44</sup> As Maddox points out, "That is not what the protocol 'only' stated," since it also contained a pledge that the Polish provisional government would hold free elections.<sup>45</sup> Although Maddox does not comment further the reader is obviously led to believe that Gardner ignored the free election pledge because it did not fit his thesis. As with almost all of Maddox's charges against *Architects of Illusion* he ignores the context and point of the argument Gardner was making. In this case Gardner seems to be emphasizing that Roosevelt had assented to only a reorganization of the Lublin government rather than a completely new start. Therefore the American meaning of a "free election" had already been compromised. One can gently criticize Gardner for a poorly constructed sentence, but not for any sort of purposeful distortion.<sup>46</sup>

Maddox's concluding chapter is largely taken up with a condemnation of orthodox reviewers for their failure to check the evidence, as he has, and

<sup>41</sup> Maddox, *The New Left*, 139, 158.

<sup>42</sup> *Ibid.*, 144.

<sup>43</sup> Maddox, *The New Left*, 149.

<sup>44</sup> Gardner, *Architects of Illusion*, 52 (italics added).

<sup>45</sup> *FRUS, Yalta*, 980.

<sup>46</sup> If Gardner did create a piece of "revisionist legendry" by seeming to claim there was no promise of free elections, it was a short-lived legend even in Gardner's own writings. In a recently published text written before Maddox's book appeared, Gardner stated: "At the completion of the reorganization process the new Polish government was to plan for free and democratic elections—a perfect solution in theory, but one unlikely ever to be realized." If anything, that statement implies a criticism of Soviet policies. Lloyd C. Gardner, Walter LaFeber, Thomas McCormick, *Creation of the American Empire: U.S. Diplomatic History* (Chicago, 1973), 440.

for recommending that his own work not be published. He suggests two explanations of why orthodox historians have, in effect, endorsed revisionist works by failing to review them rigorously. The first, that reviewers were not qualified academically to examine a certain book, is obviously, though one hopes infrequently, valid. "The second possibility—far more intriguing—is that reviewers who were perfectly aware of the procedures employed, nevertheless concluded that it was unnecessary to share this information with their readers."<sup>47</sup> Since Maddox's book is based on the premise that "the procedures employed" were "*without exception . . . pervasive misusages of the source materials*," he has accused what is probably a majority of his peers of consciously aiding and abetting in the falsification of history. In short, they are frauds!<sup>48</sup>

As if such insinuations were not enough Maddox also broadly hints at mundane motives among revisionist historians. By writing that "it is certain that these 'bold' and 'provocative' interpretations have earned fame and academic advancement for their authors"<sup>49</sup> he obviously intends to plant a seed of doubt in the reader's mind. We all, including Professor Maddox, are gratified by any "fame and academic advancement" that our efforts bring, but to infer that the successful have prostituted their intellectual standards in order to achieve that success is tasteless and unethical. The left revisionists can and have been effectively and searchingly criticized,<sup>50</sup> but the Maddox book is not an example of how to do it. Left revisionism poses real questions of methodology—in the philosophical as well as the technical sense—for all of American history, but Maddox ignores them. Though he claims to demonstrate the poverty of left revisionist scholarship, he actually only demonstrates the poverty of his own evidence and logic.

At present all too many historians of the cold war seem inclined to follow that same self-destructive process they and their predecessors took after World War II when revisionism and its opponents battled over the question of Franklin D. Roosevelt's alleged dishonesty, deceit, and dissembling.<sup>51</sup> This time the dispute is over the origin of the cold war instead of American

<sup>47</sup> Maddox, *The New Left*, 161.

<sup>48</sup> *Ibid.*, 10. Maddox offers a possible motive for such actions in "Revisionism and the Liberal Historians," *Freedom at Issue*, May-June, 1973, pp. 19-21. He theorizes that liberal historians have a sense of guilt arising out of the Vietnam War and their support of policies—such as Wilson's Fourteen Points—which contributed, according to the revisionists, to that war.

To justify his accusations about liberal historians, Maddox relates some of his own experiences in trying to publish his work. *The New Left*, 161-63. His argument is, according to the then editor of the *AHA Newsletter*, John J. Rumbarger, based solely on Maddox's attempt to publish a portion of his book in the *Newsletter*. Rumbarger claims that Maddox completely distorted the thrust of the various readers' comments. Rumbarger's version of the story, "Robert J. Maddox and the Use of Evidence," may be obtained directly from Rumbarger.

<sup>49</sup> Maddox, *The New Left*, 159.

<sup>50</sup> For example see Robert W. Tucker, *The Radical Left and American Foreign Policy* (Baltimore, 1971), and the previously cited article by J. A. Thompson, "William Appleman Williams," *passim*.

<sup>51</sup> The places to begin an examination of this literature are Justus D. Doenecke, *The Literature of Isolationism: A Guide to Non-Interventionist Scholarship, 1930-1972* (Colorado Springs, 1972), and Warren I. Cohen, *The American Revisionists* (Chicago, 1967).

entry into wars against Japan and Germany, but the temper of the debate has taken on the same vituperativeness. The evidence has begun to mount steadily. At a recent historical meeting a revisionist was accused of consciously misleading his students. Another revisionist has recently circulated a private (not for attribution) memorandum that unquestionably infers the existence of a conspiracy to discredit his and similar critiques of American foreign policy. Professor Oscar Handlin asserts that revisionist arguments are "inherently absurd" and in print only because they are "the most salable commodity."<sup>52</sup> During a recent Columbia University Seminar, which featured Robert J. Maddox as the speaker, one member of the audience angrily called Maddox "a crude polemicist and hack." The *New York Times Book Review* editor apparently saw the possibilities of such bickering when he sought out a strongly opinionated reviewer who elaborated on and even exaggerated Maddox's arguments against the New Left, followed by brief retorts from each of the seven authors Maddox criticized. Most of those responses only heightened the unscholarly level of rhetoric and name-calling.<sup>53</sup>

Deep, emotional, personal involvement is, unfortunately, a characteristic among historians of contemporary history—particularly when they deal with the compelling and almost pre-emptive issue of war. The results of such intense subjectivity have often been tragic. Preoccupation with the mendacity of Franklin D. Roosevelt virtually destroyed the creditability of such seminal thinkers as Charles A. Beard and Harry Elmer Barnes and completely discredited an indefatigable researcher like Charles C. Tansill. (Beard, shortly before his death, stated that he wanted to write one more book about that "son of a bitch," Roosevelt.) In spite of occasional insights, numerous hard and valid questions, and a potentially viable thesis, these and other historians vitiated their own arguments by their intemperance and nastiness.

The philosophers and essayists who wrote in the genteel tradition were quite right when they claimed that style is often as important as substance. It is not hypocritical to disagree with someone in a gentle, courteous manner; rather that is how human society must function if it is ever to achieve peaceful relationships. Walk into a barroom with a chip on your shoulder and surely someone will knock it off. So it is with historians: intellectual belligerence merely begets more of the same. Even candor and bluntness can be couched in honest but mannerly terms. Granted, it is far easier and much more fun to be vitriolic and sarcastic, but such writing hardly qualifies as a dispassionate search for truth; rather it merely polarizes the argument.

Unfortunately the effects of *ad hominem* and belligerent historical disputes are far wider than just the immediate participants; our students and the

<sup>52</sup> Oscar Handlin, "Revisionist History: A Base for Neo-Isolationism," *Freedom at Issue*, Sept.-Oct. 1972, p. 2.

<sup>53</sup> Francis Loewenheim, review of *The New Left and the Origins of the Cold War* by Robert J. Maddox, in the *New York Times Book Review*, June 17, 1973.



general public are likewise affected. Generations of post-World War II graduate students were brought up believing that Barnes, Tansill, and Beard had prostituted themselves and their professional standards. As a result few students in the fifties or sixties read their work except to mock or scorn it in an historiographical paper for their methods course. Barnes's paranoid claims of a blackout conspiracy became a self-fulfilling prophecy as the consensus of the historical profession condemned him and others who indulged in intemperate and personal attacks on Roosevelt. Yet the quiet, positive, and persistent critiques by the late William Neumann—writings that raised all the important questions but never transgressed the bounds of taste and manners—were consistently cited as an example of moderate (if incorrect) revisionist scholarship.

We can hardly expect our students to understand or believe in the importance of civility and respect for the opinions of others if their teachers and intellectual leaders ignore it. Historiographical warfare is no substitute for scholarship; dark hints of conspiracy should not replace the awareness that our opinion might be wrong; name-calling and sarcasm must never be confused with careful criticism.

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## Reviews of Books

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### GENERAL

L. G. HELLER. *Communicational Analysis and Methodology for Historians*. New York: New York University Press. 1972. Pp. xii, 179. \$12.00.

Heller wishes to set forth the methods and objectives of linguistic and graphemic reconstruction. His book lacks an index, uses, in part, idiosyncratic approaches and a private terminology, and sometimes stops short of the essential point. It does touch on most of the topics that one would want to see included.

Reconstruction may establish a point in time at which an ancestral speech community split apart, or at which languages had contact with one another; techniques range from "simple dating procedures" to the sophisticated, if uncertain, inferences of glottochronology (based on assumptions about the persistence of vocabulary in time). In addition to such "Chronological Reconstruction" (chapter 2) there is "Geographical Reconstruction" (chapter 3), aimed at fixing the ancestral habitat and at tracing migration routes and the like. There follows (chapter 4) a discussion of the nature of language and change. How effective this is remains to be seen; the statement about reconstruction resting on the "observation that linguistic changes admit of no exception whatever" will cause confusion or pain or both—not to be relieved by the few lines of explanation that follow. The fifth chapter returns to the retrieval of meanings, this time of sociocultural ones like kinship vocabularies. Chapter 6 contains reflections on the possibility of knowing the personality and the personal status of the writer of a text. There is a chapter on decipherment (chapter 7) and a disquisition on external (nonlinguistic) causes of linguistic

change, illustrated not as one might expect with the usual references to substrata, area typologies, etc., but with a rather imaginative sketch of the background of the formulas of address (thou, ye) in some European languages. In chapter 9 the reader is promised a glimpse of the frontiers of linguistics and told about the rank-frequency law that is rightly described as a statistical concept of wide application in a number of fields but presented in a completely non-Mandelbrot version.

With allusion and example Heller succeeds in stimulating interest, but hardly in giving his reader a sense of the characteristic formal concreteness of linguistic procedures and of their power and limitations.

HENRY M. HOENIGSWALD  
*University of Pennsylvania*

KAJ BIRKET-SMITH. *Eskimos*. New York: Crown Publishers. 1971. Pp. 277. \$17.95.

Danish explorer-scholars were among the first to begin the scientific study of Eskimo culture nearly one hundred years ago, and they remain today among the most informed sources of information on Eskimo anthropology and archeology. Birket-Smith is of the best of the Danish school that has left the distinctive imprint on work in this field. A lifetime of research into nearly every aspect of Eskimo culture qualifies him pre-eminently for the task he has here set forth.

*Eskimos* is a unique scholarly study of the most fascinating and widespread of the world's ethnographic peoples. Stretching from southwest Alaska to southern Labrador and Greenland they encompass a culture area of some four thousand miles linear distance—surely the

greatest area occupied by any ethnographic group sharing a basic language and culture type. Yet within this unity there is important variation that Birket-Smith manages to incorporate within his catholic grasp of detail and compelling personal and narrative style. We learn of Eskimo subsistence adaptations, technology, religion, language, social organization, and mythology with a marvelous blend of historicism and personal experience; of Eskimo contacts with Siberia and medieval Europe, of the Norse colonization of Greenland and subsequent effects on its local Eskimo inhabitants, and of the important question of Eskimo origins.

This much has been told before, as the present Crown edition is basically a republication of Birket-Smith's 1959 *Eskimoerne*, which has also been available in English for some time. The major additions to this edition are the large format, the striking color and black-and-white illustrations, and the incorporation of part of a formerly published essay on the present status of Eskimo society in the modern world by the recently deceased Canadian anthropologist, Diamond Jenness. Jenness's eloquent plea for reasoned approaches to modern problems in the north is one of the finest pieces of literature a concerned humanist has ever produced in the field of anthropology, and its inclusion together with new illustrative material in this edition makes the work the most communicative statement available on Eskimo culture, history, and society in a day when Arctic development is no longer a remote frontier. Unfortunately Jenness's compassion is heightened by Birket-Smith's now outmoded use of such concepts as "infantile" and "primitive," regarding Eskimo cultural and physical anthropology and psychology (pp. 46, 58), and his reliance on antiquated diffusion theory, "cultural levels" (p. 213), and evolutionary determinism betrays a conjectural history approach to culture that has marked the Danish school of Eskimo ethnography. In addition the continued belief in his now disproven theory of Eschato-Eskimo (interior) origins takes little account of present evidence, furthering the immensity of the generation gap in Eskimo studies and pointing glaringly toward the need for a new synthesis.

WILLIAM FITZHUGH  
Smithsonian Institution

VERN L. BULLOUGH, with the assistance of BONNIE BULLOUGH. *The Subordinate Sex: A History of Attitudes toward Women*. Urbana: University of Illinois Press. 1973. Pp. viii, 375. \$10.95.

Anyone attempting to write a one-volume *History of Attitudes toward Women* shows, as the author concedes, "a kind of foolhardiness which is usually condemned in scholarly circles." Vern Bullough, a professor of history at California State University at Northridge, attempts to deal with this difficulty by citing the lack of research in this field and by including, presumably for the feminine point of view, a closing chapter by his wife, Bonnie Bullough, a nurse and sociologist. Predictably, this purely formalistic approach at remedying a basic conceptual weakness fails, and we are left with a textbook based on frequently inadequate secondary sources, superficially treated and marred by present-minded slanting. What is wrong is not the author's sex, but his simplistic concept of women's history as nothing more than a compilation of "compensatory" facts and an endless recitation of suffering, victimization, and oppression. Again, the title, *The Subordinate Sex*, expresses the author's concepts and their limitation quite adequately. To view women as "the subordinate sex" is to disregard their continuous contribution to the building of human culture, to ignore their unique and separate historical experience, and to fail to come to grips with the essential conceptual implications of including women in history, namely the challenging of male-defined concepts of what history is. Women's history is not the history of male attitudes toward women, nor is it the history of women as victims any more than it is the history of women as wives and daughters of the true "makers and shapers" of history. The wholesale omission of women from the work of historians cannot be rectified by compensatory listings of "achievements" and by a catalog of discriminatory practices. It cannot be adequately dealt with by including the concept of "sex and sex role indoctrination" in historical inquiry, although that is a worthwhile and helpful inquiry. Certainly, the injection of the terminology of the women's liberation movement can only serve to give the gloss of contemporary topicality, possibly in the hope of enhancing sales, but it cannot be taken seriously as scholarship.

Still, this book has its usefulness. As a survey

of the way in which men have regarded women it summarizes the social, legal, and economic status of women in various periods of history, ranging from antiquity to the present and including the major cultures, with the notable exception of Africa, but including Islam. As a scholarly contribution to the field it has nothing to recommend it. Given the absence of primary sources, the lack of a bibliography, and the superficial nature of the "Guide to Further Reading," it is very doubtful that it "might serve as a guideline for monographic studies." Those desiring guidelines that lay no claim to historical scholarship but abound in stimulating and original concepts valuable to the scholar might do better to consult the work of Simone De Beauvoir, Elizabeth Janeway, Mary Beard, and Viola Klein.

GERDA LERNER  
Sarah Lawrence College

OWSEI TEMKIN. *Galenism: Rise and Decline of a Medical Philosophy*. (Cornell Publications in the History of Science.) Ithaca: Cornell University Press. 1973. Pp. xvii, 240. \$15.00.

For well over a millennium medicine in Western and Central Europe (around the Mediterranean basin) and in the Middle East was pervasively influenced, and to a greater or lesser degree dominated, by the system called "Galenism." In 1865 the French medical historian, Charles Daremberg, noted that as late as the middle of the eighteenth century this system persisted almost in its entirety despite discoveries in anatomy, physiology, and pathology that invalidated its fundamental premises. Nonetheless this historical phenomenon has not attracted the attention and investigation it merits. The physician Galen (ca. A.D. 130-ca. A.D. 200) has been and is being studied by medical historians, classicists, and Arabic scholars, yet the system that Galen did not create, but that was developed from his teaching and his writings, still awaits equivalent attention. All the more welcome, therefore, is this study of Galenism by Professor Temkin. Originally presented in 1970 at Cornell University as the Messenger Lectures the published text has been considerably expanded and revised.

Temkin considers Galenism "as an intellectual phenomenon, as a philosophy in the sense of principles, beliefs, and facts, more or

less cogently connected to form a set and ascribed to Galen." The rise and decline of this medical philosophy, the vicissitudes of Galenism from its appearance in the later Roman Empire to its disappearance in the eighteenth century, are delineated sympathetically, clearly, and, as one expects of Temkin, with an obvious but unobtrusive command of a wide variety of primary and secondary sources.

Galen marks both an end and a beginning in the history of Greco-Roman medicine. He was very well acquainted with the writings of his predecessors as well as with those of his contemporaries and examined critically contemporary and past currents of thought and practice. In this sense Galen's works are a huge repository of ancient medical literature as well as of natural philosophy and were thus able to provide the basic substance for Galenism. Moreover Galen's reputation as a physician and natural philosopher grew after his death, and by A.D. 350 his place as the leading authority had been established. Temkin analyzes carefully and in detail the intellectual strands out of which the fabric of Galenism was woven and in passing refers to the period of civil strife, unrest, and insecurity, occurring between the death of Commodus and the conversion of Constantine, as having provided a receptive environment for Galenism. One may suggest in this connection that the emergence of Galenism paralleled and probably reflected the standardization and systematization of civic and cultural life that took place during this period. As H. P. L'Orange demonstrated the emergence of a trend toward simplified, static forms in art and architecture in the late Roman Empire accompanied the establishment of a new political structure. A similar process in medicine and natural philosophy, a movement away from the empirical and the relative to the dogmatic and the authoritarian, a movement toward uncomplicated absolutes in systematic form, produced Galenism. But as Temkin points out there is a scarcity of material for a century and a half following Galen's death so that the early development of Galenism remains obscure and requires further investigation.

Temkin traces the spread of Galenism as it traveled initially through the Greek centers of learning, Alexandria and Constantinople, rather than by way of Syria and the Muslim countries. Until the eleventh century there was

little Galenism in the Latin West, but thereafter Arabic influence made itself felt in Europe and the knowledge of Aristotelian natural science and metaphysics spread. But Galenism did not remain unchallenged. Rhazes, Ibn an-Nafis, Averroes, and Maimonides criticized aspects of Galenism, yet doubts and deviations from accepted doctrine were not fatal as long as physicians and the lay public found their way within the structure of the system. The decline of Galenism was not a cataclysmic event; it was a slow process due not to a single cause, but the interaction of a number of conditions. It survived into the eighteenth century because the new science could not provide a guide to medical practice while traditional methods did.

The rise, decline, and fall of Galenism as presented in this interesting and well-written book should be of interest to a large audience: medical historians; historians of ideas and of culture; classicists and medievalists; and historians of science, philosophy, and religion. Naturally those already familiar with some aspect of the subject will judge the author's achievement from their viewpoints. For the thoughtful reader, whatever his or her background, this original contribution by an outstanding scholar offers much of interest within a small compass.

GEORGE ROSEN  
Yale University

R. A. G. CARSON, editor. *Mints, Dies and Currency: Essays Dedicated to the Memory of Albert Baldwin*. London: Methuen and Company; distrib. by Barnes and Noble, New York. 1971. Pp. xv, 336, 23 plates. \$33.50.

This *Festschrift* volume is dedicated to an unusually competent coin dealer who worked closely with scholars and museums. The volume is handsomely printed on heavy paper and the plates are excellent. The book has all the usual virtues and defects of *Festschriften*: the contributions are uneven in length and in value.

There are two appreciations of Baldwin. The scholarly contributions are arranged chronologically and range from the period of the Roman Republic to the sixteenth century,

with heavy emphasis in the later articles on Great Britain.

The material on ancient coinage is mostly excellent. Charles A. Hersch has published some additions to the E. A. Sydenham catalog, *The Coinage of the Roman Republic*, in continuation of his earlier contributions. Some of the coins illustrated are not to be seen elsewhere. It is rather surprising that not he but Michael Crawford of Cambridge has produced a revision of the Sydenham corpus, and one hopes that Crawford had this information available to him. Curiously Crawford's forthcoming work is not mentioned by Hersch.

P. V. Hill's "The Dating and Arrangement of Hadrian's 'Cos III' Coins" bears upon the dating of the travels of that emperor; there are implications for other historical assessments of Hadrian's personality and policies. The article must be consulted by any future biographer. Carson's own chapter, "The Sequence-marks on the Coinage of Carausius and Allectus," also a continuation of earlier work, is a model of proper procedure to be used in examining a corpus of coins that include anomalous specimens.

As one would expect, some of the more important contributions deal with the coinage of Great Britain. "Variations in Currency in Late Anglo-Saxon England" by C. S. S. Lyon will be interesting to all numismatists because of his discussion of some of Sture Bolin's theses on coin weights and overvaluation. Interesting questions are raised that are yet to be answered on such matters as mint charges and exchange of obsolete coins.

The longest contribution is Ian Stewart's "Scottish Mints," a kind of supplement to the author's *The Scottish Coinage*. The earlier sections on the mints contain some historically interesting material, as for example the observation that the main mints tended to be shifted to sites strategically important in the contemporary historical context. For the general numismatist a most important section is that dealing with die-links. Some of these are die-links between mints. The possibilities in these relationships are detailed. A quite valuable and judicious section discusses the technical problems involved in identifying die-links, which is not as simple a matter as it may seem. Dies wear down or are dressed down, the

striking angle may vary, and the depth of the relief may cause varying appearance in the specimens.

Several of the other contributions are well done, though some are slight. All research libraries should acquire the book.

HENRY C. BOREN

*University of North Carolina,  
Chapel Hill*

ARNE BERG *et al.* *Architecture in Wood: A History of Wood Building and Its Techniques in Europe and North America*. Edited by HANS JÜRGEN HANSEN. Translated by JANET SELIGMAN. (Studio Book.) New York: Viking Press. 1971. Pp. 288. \$40.00.

This lavishly illustrated, handsomely produced, and expensive volume has the appearance of a coffee-table book. In reality it is both more and less. The editor, a well-known Continental scholar, collected a series of essays on building in wood from authorities in Europe and North America in their respective areas, and, with the exception of the contributions of J. T. Smith on the British Isles and Nancy Halvorson Schless on the United States, all are translated from the German. The original version was published in Hamburg. In terms of scholarly merit the best is unquestionably the section on "Wood Building in Northern Europe," by M. N. A. L. Berg, architect of the Norsk Folkemuseum in Bygdoy and the Toini-Inkeri Kaukonen, superintendent of the Suomen Kansallismuseo in Helsinki, and the editor. It would be hard to find a better or more succinct discussion of the famous stave churches of Norway, and the illustrations of the huge wooden churches of Finland are particularly welcome because they are so little known. The English material is also good but less well organized, and the sections on Central Europe and Russia, while provocative, are downright confused. The section by Nancy Halvorson Schless suffers from an overemphasis on the colonial work of the Eastern seaboard. While there is a nod toward Frank Lloyd Wright's extraordinarily creative use of wood in his Usonian houses, something should certainly have been said about the pioneering work of Ellsworth Storey in the Seattle area and the more recent, and extremely influential, homes and churches by Pietro Beluschi in Oregon. Wood has always been the

great American building material, and this point deserves to be emphasized.

With regard to the broad historical reach of the book, the most surprising omission is unquestionably the lack of any discussion of the theories of Professor Walter Horn, who holds that the essential structure of the Gothic cathedral was derived from the bay divisions of the great medieval barns. While Horn's evidence for this position is by no means universally accepted, his contentions should at least be mentioned. Almost equally surprising is the omission of any discussion of the major surviving medieval constructions in wood of a monumental nature, notably Westminster Hall and the crossing tower at Ely Cathedral. Clearly, in England during the Middle Ages the master carpenter was generally as important as the master mason, and since we possess a few excellent examples of his skill, these should be illustrated and discussed.

Most aggravating of all is the juxtaposition of photographs and drawings with regard to the text. Granted that the integration of text and visual material is always a major problem in an art historical work, the arrangement which we have here is so discordant as to be infuriating. Thus in chapter 2 the photographs skip, with marvelous illogic, from: (a) a barrel vault in the Church at Oosthuizen, North Holland, 1520 to (b) roof timbers at Santa Croce in Florence, fourteenth century, to (c) the interior of the Guildhall of the Lord Leicester Hospital, Warwick ca. 1390 to (d) the interior of the Breton church of Ste. Catherine, fifteenth century. Each photograph is excellent and would be exceedingly meaningful if it were tied in with the appropriate text (for example, the church at Honfleur with chapter 5, "Wood Building in France"). This kind of haphazard selection reduces the book to a kind of hideous jumble and often negates its usefulness.

In sum, this volume is a useful source for anyone interested in architecture in wood, which is indeed an important and too often neglected topic, but it is so badly put together that its utility is vitiated. It remains to be noted that each chapter has a good bibliography and that it is well indexed.

LEONARD K. EATON

*University of Michigan,  
Ann Arbor*

RALPH DAVIS, *The Rise of the Atlantic Economies*. (World Economic History.) Ithaca: Cornell University Press. 1973. Pp. xiv, 352. Cloth \$12.75, paper \$5.95.

This work will be welcomed as the best text we now have on preindustrial economic history. Its brilliant organization of complex materials, its unabashed eclectic approach, and its highly readable prose make it just the thing for courses in both Old Régime and economic history. It also can be used for adjunct reading in American and Latin American colonial history. And it is perfect for graduate students boning up for an exam in early modern history; they will turn to this from volume 4 of the *Cambridge Economic History of Europe* (and from volume 5, if it ever appears) with a sigh of relief. The series World Economic History, edited by Charles Wilson, of which the Davis work is the first volume to appear, is off to a good start.

"The rise of the Atlantic economies" is here presented as a semitopical, semichronological description of the main changes in the economic affairs of England, the Netherlands, Spain, Portugal, and France (and their colonies in North and South America), from the mid-fifteenth to the mid-eighteenth century. Chapters on the general economic history of these countries and empires are interlarded with other chapters or specialized sections on population, income, commerce, agriculture, and capital. This sort of organization allows Davis to develop selected illustrations of economic change in satisfying detail in spite of the relatively small size of the book: examples are the sugar industry, Amsterdam in the seventeenth century, and the economic decline of Spain.

Perhaps the outstanding achievement of this text is its incorporation of so many relatively recent or "post-Heaton" findings by specialists. There must be scores of such improvements; I gave up counting after the first two dozen. On the other hand certain minor aspects of the work are regrettable. The index is barely adequate; the five sketch maps are not worth the space they occupy; and the bibliography will please no one, since it is too short for advanced scholars and too concerned with French, German, Spanish, and Dutch items for beginners. Changes in the organization of manufacturing, so important in the older histories, are treated skimpily; economic policy and theory, treated

by some of us under the heading of "mercantilism" (but decidedly not by Professor Davis, who shuns the word), has just about disappeared from view.

Those who expect to find here a bold new synthesis will be disappointed. Professor Davis is interested in middle-range generalizations only. He uses a rather tenuous thread on which to hang his copious and valuable material: comparative economic development, or rather an unweighted listing of reasonable-sounding explanations for Iberian decline, Dutch stagnation, and English industrialization. For Professor Davis the search for general patterns, so dear to the hearts of the model builders, is of little importance. He is much more interested in divergence. He is quite serious in his use of the plural in the last word in his title. And indeed this may be the only sensible course to follow until the Braudelians on the Continent and the neoinstitutionalists in America turn up something more convincing in the way of a general theory of early modern economic history than is now the case.

MARTIN WOLFE

University of Pennsylvania

PIERRE LÉON *et al.*, editors. *L'industrialisation en Europe au XIX<sup>e</sup> siècle: Cartographie et typologie* (Lyon, 7-10 octobre 1970). (Colloques internationaux du Centre National de la Recherche Scientifique. Sciences humaines.) Paris: the Centre. 1972. Pp. 619. 128. 40 fr.

This colloquium volume, lavishly produced, contains contributions by scholars—many of first-rank reputation—from numerous non-Communist European countries. They are written in five languages, with French predominant and English and German widely used. Rondo Cameron was the only American participant, but frequent reference was made to methodological advances in American economic history and some useful analogies drawn to the European experience. The conference had a distinct comparative flavor within the European context. Discussion after the major papers were delivered brought out some contrasts in national approaches, though at times falling flat, as is so common in such gatherings, when discussants took the line: "how interesting, and here is what happened in the region I am familiar with." Generally the French were most con-

cerned with structural factors, even in non-quantitative papers, the English and Germans with a range including more intangibles.

The variety of subject matter and quality defies summary in a brief review. Cartography receives little attention, and that rather antiquarian, despite the subtitle. The remainder of the volume is idiosyncratically organized. Essays on demography or banking, for example, pop up at widely separate points. Many of the essays on the smaller countries are weak, involving purely descriptive material; a long quantitative study on Belgium, with many careful graphs, tells us essentially that the Belgian economy was different in 1900 from what it had been in 1800. Other essays provide excessively familiar summaries, such as several long discussions of manufacturing up to the end of the eighteenth century. Treatment of demography, including an article by Armengaud, is disappointing, reminding us of how little class-specific material has been developed for the nineteenth century.

But there are contributions of importance. Several monographic essays, including two on Swiss textiles, contain useful data, but might better have been placed in a more specialized collection. At least two essays, though not novel, provide useful summaries of major developments: Mathias on Britain's uniqueness and Crisp on Russian industrialization. Jean Bouvier provides a neat typology of the ambiguous relationship between banking and industrialization. A quantitative essay on French railroads is splendid, involving much new material. In general those essays that really do focus on typologies, including regional studies of Normandy, Wales, and the Alpine area, hit the mark well. Particularly important is the explicitly comparative work of Wolfram Fischer, isolating the cases of Saxon and Westphalian development. Generally the better essays lead, as concluding remarks suggest, to a sense of regional (not national) typology based to an important degree on preindustrial differentiation. It might have been desirable to concentrate on these essays, producing a volume less commemorative and more accessible, even at cost to some *amours-propres*.

The most interesting conceptual problem not raised by the volume as a whole concerns its chronology. Participants were quite ready to

move into prenineteenth-century materials as background. But despite wide recognition of industrialization as a process, not an event, there was a temptation to see it magically stopping in 1900 or 1914. An essay by Postan raises important questions about the applicability of the English industrial model to twentieth-century industrialization in underdeveloped countries, but this still does not challenge what is essentially an English-based chronology for Europe. Hence the most stimulating essay of all, in terms of raising new questions, is one by Hartwell that discusses the development of the tertiary sector in industrialization; frankly recognizing the limitations of existing knowledge, the essay sees industrialization in twentieth- as well as nineteenth-century terms, talking about what it was becoming as well as what it briefly was. Here, clearly, is an approach that grows increasingly necessary if we are to understand industrial society as an ongoing phenomenon.

PETER N. STEARNS

*Carnegie-Mellon University*

DOUGLAS H. ROBINSON. *Giants in the Sky: A History of the Rigid Airship*. Seattle: University of Washington Press. 1973. Pp. xxix, 376. \$15.00.

Airships are classified according to three structural types: nonrigid, semirigid, and rigid. Because of their reliance upon a pressure differential for the maintenance of envelope rigidity monrigids and semirigids are also known as pressure airships. This distinguishes them from the rigid airship, the hull of which, consisting of a rigid structural frame, maintains its shape irrespective of internal gas pressures. *Giants in the Sky* is the first scholarly study of the latter type and covers the period 1900-40.

A hundred and sixty-one rigid airships were built and flown, and according to the author, "all but two—which were metal clad—shared fundamental features evolved by the original inventor, Count Ferdinand Von Zeppelin." Because of General Zeppelin's experiments and the activities of the Deutsche Luftschiffahrts-Aktien-Gesellschaft, commonly known as Delag, Germany was to dominate the rigid airship industry. Some ten thousand paying passengers were transported by the Delag Airline during the 1910-14 period, and in the 1930s Germany developed a profitable trans-Atlantic service.



Numerous rigids were used by Germany during the First World War. Although the Zeppelin-type airship had great bomb carrying capacity its raids over England proved something less than a complete success. Not only was it vulnerable to attack from pursuit planes, but it also frequently became a victim of turbulent weather conditions and engine failure. Great Britain and France made small use of the rigid airship during the war and contributed little to its development.

Even though such famous U. S. Navy airships as the *Shenandoah*, *Los Angeles*, *Akron*, and *Macon* were copies of the German Zeppelin (the *Los Angeles* was actually built in Germany) the author has entitled one of his chapters "The Innovators: American Military Rigids." Indeed the United States did pioneer in the use of nonflammable helium gas instead of the somewhat lighter but dangerous hydrogen used in Europe. Also the Americans experimented successfully with the "hook on" method of carrying fighter planes and developed more effective mooring systems.

Perhaps the most interesting section of the book deals with the commercial operations of the *Graf Zeppelin* and the *Hindenburg* (over eight hundred feet long with a gas volume of some 7 million cubic feet) that were successful until the disastrous burning of the latter at Lakehurst, New Jersey, on May 6, 1937.

Airship buffs and aeronautical engineers will find *Giants in the Sky* a gold mine of information. The casual reader, however, may be overwhelmed by the clutter of material on dimension, gas displacement, and construction details that fill almost every page of the volume. Many historians will be disappointed in Robinson's failure to place the rigid airship in its proper perspective in relation to the overall military activities in World War I.

JAMES J. HUDSON  
University of Arkansas,  
Fayetteville

MONTE DUANE WRIGHT. *Most Probable Position: A History of Aerial Navigation to 1941*. Lawrence: University Press of Kansas. 1972. Pp. xi, 280. \$13.50.

Monte D. Wright, an air force lieutenant colonel and a Duke Ph.D., has had the benefit

of personal experience as well as of scholarly inquiry in the subject of this book. This is a history of aerial navigation in the strict sense, that is, of the development of instruments and methods for determining the "most probable position" of an aircraft with respect to the earth's surface. It covers the whole range from the birth of aviation to World War II, from the problem simply of finding one's way home in a flight of a few minutes or hours to the finding of military targets for bombers in World War I, and from the historic transoceanic and polar flights of the 1920s to the round-the-world flights of the thirties, and the long-range bombing of the late thirties and early forties. Although the book claims coverage only to 1941 the author cannot escape inevitable references to the World War II experience.

The story of the rise of aviation always has been a glamorous one, but a technical accounting of the problems of navigation might appear to many to be its least glamorous side. Certainly it is one that, as a whole, has been most neglected. Colonel Wright has been able to overcome the deficiency without loss of the glamour in the tale. While he writes of technical instruments and methods the human interest stories of the men and women who used them comes through. True to the instincts of a good teacher (he is associate professor of history at the Air Force Academy) he introduces his subject by providing a background explanation of marine navigation as applied before World War I and then shows how the free balloon introduced the third dimension that characterizes aerial navigation.

The development of airplanes and dirigibles before and during World War I brought along the necessity of improved methods for navigation. Both the German and British forces used radio direction devices, but often with error and ill effects. While further improvement in radio navigation accompanied the establishment of overland airlines after the war, celestial navigation—long the system of reliance for marine navigation—reached new levels of utility with the development of transoceanic airlines. With the bubble sextant and artificial horizon, and with aircraft such as Pan American's Boeing Clippers fitted with astrodomes for celestial observations, navigators were able to correct their dead-reckoning estimates with great accuracy.

Colonel Wright's method is to explain each new navigational device within his general chronological approach and then show how it was applied on historic or pioneering flights. Thus Commander Richard E. Byrd reported to the Navy Department in 1925: "Sun compass very good when sun is visible" (p. 139). Lindbergh had great praise for the earth inductor compass that he used on his trans-Atlantic flight in 1927, though actually it was less reliable than he at first indicated. It failed Chamberlin when he tried for Berlin that year, and it failed Byrd in the *America* over the French coast, but it served well for Kingsford-Smith and Ulm on their great flight in 1928 from San Francisco to Australia via Hawaii and Suva.

There is no general statement of conclusions, but the book does end with a pointed reference to the limitation on the effectiveness of strategic bombing during World War II: "The air commanders knew that a B-17 crew could find a precise target, given clear weather and daylight operations. They believed, rightly, that a bombardment campaign of sufficient intensity could produce significant military results. They also knew what flying weather over central Europe is like. The World War I experience was unambiguous. Commercial aviation had compiled additional meteorological data between the wars. The commanders did not see all those facts and assumptions together, and no one pointed with 'an insistence compelling action' to the central contradiction: that there would not be enough clear days to execute the massive campaign, that weather forecasts would be fallible, and that bombing through clouds and industrial haze would be necessary."

It is an accomplishment for a military man to escape the military language of the Pentagon for direct, active, clear, and concise English, or for the technician to explain technical matters in a way that holds the interest of the layman. It may be even more of an accomplishment for one to escape the pedantic and tortured prose of a doctoral dissertation. Wright has done all these.

The book includes the British, French, and German experience as well as the American. Extensive notes and an impressive bibliography testify to the scholarship. Numerous drawings

and photographs add to the clarity of the text. It is a work well done.

JAMES A. HUSTON  
*Lynchburg College*

MELCHIOR PALYI. *The Twilight of Gold, 1914-1936: Myths and Realities*. Foreword by DONALD L. KEMMERER. Chicago: Henry Regnery Company. 1972. Pp. xxiv, 365. \$15.00.

The late Melchior Palyi has written another history of the demise of the gold standard, and he clearly regards its downfall as a tragedy for the world. Monetary policy can stabilize either the domestic price level or the price of gold (foreign exchange rate), but not both. Adherence to the gold standard leaves domestic prices to be determined by foreign circumstances, and Palyi prefers this external stability to the goal of domestic stability that is emphasized by modern policy makers. The gold standard is preferred for two reasons. First, it is economically efficient because it promotes international mobility of goods and factors of production. Second, it protects the liberty of citizens from arbitrary taxation by imposing a monetary constitution on the government.

Palyi's distrust of government does not extend to central bankers, whom he regards as guardians of the soundness of money. His prototype of the pragmatic, responsible central banker is Montagu Norman of the Bank of England, and he sharply distinguishes the independent, apolitical character of Norman from the vacillation of elected officials who yield to political pressure. He regards the floating of the pound in 1931 as a tragedy from which sterling never recovered, and it occurred only because Norman was too ill to protest the action.

The book expresses a curious commitment to private markets, since it is critical of their ability to cope with uncertainty through such arrangements as insurance and futures contracts. Speculation is viewed not as a useful bearing of risk that stabilizes prices around their trend, but rather as antisocial mischief that must be controlled. Speculation has damaging effects on real estate and stock prices, and especially on foreign exchange markets, and it is concluded that destabilizing international capital flows made the inter-war experiment

with floating exchange rates a dismal failure.

As a practical man Palyi is suspicious of theory. Prices did not fall in the 1930s because of the sharp contraction of the money supply from 1929-33. The quantity theory of money is too simple. The Federal Reserve could not prevent the contraction since they could not force people to spend. Churchill was not responsible for economic stagnation in Britain by overvaluing sterling in 1925. Purchasing power parity calculations of the exchange rate are too simple.

Palyi is short on alternative explanations, but in general the problems of the 1930s were caused by the denial of discipline as exemplified by private speculation, deliberately unbalanced budgets, and a lack of commitment to gold. There is little new information in Palyi's chronicle, but the style is fluid and the anecdotes involving central bankers are entertaining.

THOMAS GRENNES

North Carolina State University

MICHAEL G. FRY. *Illusions of Security: North Atlantic Diplomacy, 1918-22*. [Toronto:] University of Toronto Press. 1972. Pp. xii, 221. \$12.50.

A considerable part of this succinct and highly significant book on post-World War I diplomacy is devoted to the concept of Atlanticism. Viewed by its proponents as a panacea for the major world problems, the idea prevailed well before 1914 but exercised a special fascination over some members of the British Empire elite following World War I. While that same elite also included men who were skeptical and even hostile to the Atlanticists, the latter were well entrenched in the Foreign Office, the Board of Trade, the diplomatic service and throughout the Dominions. Essentially the Atlanticists perceived the peace and hope of the world as hinging upon Anglo-American cooperation in concert with Canada, whose subordinate role in the alliance would be enhanced by Ottawa's potential service as mediator between Washington and London.

Because Atlanticists like Auckland Geddes, Lord Milner, and Arthur Balfour differed in their respective outlooks a general profile of Atlanticism would necessarily contain a number of inconsistencies. Milner's and Philip

Kerr's missionary idealism was, however, typical of most Atlanticists. They saw the Commonwealth as a model in which white, Christian, advanced peoples controlled and guided backward races toward higher values and self-government. A fusion of cultural concepts was neither sought nor desired by the English-speaking imperialists. They aspired instead to disseminate those "superior" Anglo-Saxon, political and spiritual ideals that, when universally embraced, would afford a self-perpetuating peace for all mankind. Several factors obstructed the Anglo-American harmony Atlanticists sought to promote during the 1919-22 period: President Wilson's personal and political collapse; United States isolationism and attitudes toward war debts; Middle East tensions; and the question of naval power for imperial defense, a matter which possibly touched England's rawest nerve. Fry convincingly argues that, despite the achievements of the Washington Naval Conference and a few other modest gains, the ultimate failure of the Atlanticists lay in their excessive and unrealistic expectations. Even less ambitious efforts at altering global policies, he reasons, might have enjoyed little success during a period of such intensive reconstruction.

The paraphrases of British ministerial reports, which comprise a substantial portion of the author's account, yield a somewhat mixed result: while the witticisms are engaging and the commentary revelatory of the basic objectives of the empire elitists the reader must contend with a distracting measure of superfluous detail in a number of loosely edited and wide-ranging dispatches. Perhaps the most serious limitation of this otherwise excellent study is the paucity of American source materials. We are told that United States Ambassador George Harvey warned British Foreign Secretary Lord Curzon of the further deterioration of Anglo-American relations in 1922; how Washington felt that Great Britain and Japan had somehow outwitted the United States at the Washington Naval Conference; and that the resolution of the Irish question had a salutary effect upon the overall state of British-American affairs. In each of these instances, however, English or Canadian sources are cited; only occasionally do a few published State Department papers appear as supportive evidence. The views of

American Atlanticists are largely ignored.

None of this seriously detracts from the book, and Fry demonstrates a thorough familiarity with the published and archival sources for his period. He possesses an impressive knowledge of the principal personalities of the era, more notably the empire elitists, whose strengths, weaknesses, and influence in the articulation of policy are ably perceived and assessed. The author concludes that the Atlanticists, in their mistaken belief that Great Britain and the United States shared common global purposes and goals, reached for an allusion after World War I. Fry suggests, possibly for the consideration of some future study, that this failure in perception did not prevent others from resurrecting the discredited Atlanticist theme after World War II. This monograph richly deserves a place on the selective shelf of scholarly studies in twentieth-century diplomacy.

THOMAS E. HACHEY  
Marquette University

HOWARD M. SACHAR. *Europe Leaves the Middle East, 1936-1954*. With an introduction by WILLIAM L. LANGER. New York: Alfred A. Knopf. 1972. Pp. xviii, 687, xxxviii. \$15.00.

In his new book Professor Sachar has examined the political and military machinations of those European powers—namely Britain, France, Italy, Germany, and Russia—for whom the Middle East was a major arena of conflicting interests and the process by which those powers withdrew from the region in the years between 1936 and 1954. It will come as no surprise that a considerable portion of Sachar's attention is, of necessity, devoted to the problem of Palestine.

This is largely a descriptive work, comprehensive in scope, well organized, and written in a clear and interesting style. While Sachar empirically offers little that is unknown or original, he has undertaken a problem worthy of consideration. It is a book also useful for the quantity of information contained therein, which is presented with care, accuracy, intelligence, and some insight. The maps are clear and supportive of the text; the bibliography is helpful, and the index usable. Here is an instance where the reader can safely accept the author's work but need not necessarily accept his ideas or conclusions.

A few tremolos of doubt are set in motion by Sachar's premise that Europe's "withdrawal was to be far more complicated and invidious an ordeal than the initial, rather straightforward and simplistic military conquest" (p. xvi). Then the first chapter commences with the assertion that after a half millennium of Islamic fatalism and stagnation in the Middle East, "the vanguard of progress was the Anglo-Indian infantry who marched into Baghdad in late 1917, the Anzac cavalry who stormed across Palestine in 1918, the French marines who disembarked at Beirut harbor in the last weeks before the Armistice" (p. 5). Further doubts are raised by Sachar's tendency merely to describe rather than to explain or analyze, as he does for other major parties concerned, the motives and attitudes of the Arabs toward various issues (especially chs. 1-4) and to place too much emphasis on xenophobia to characterize Arab responses to European domination (pp. xvii, 35, 56, 163, 331, 404, 554, 613). Finally, the author concludes that "even the most enlightened of Western efforts to modernize and liberalize the Middle East foundered on the rock of Moslem *immobilism*" (p. 609).

Surely the conflicting nationalist impulses that propelled Europe's expansion into the Middle East were at least as "complicated and invidious" as the process of withdrawal to which they were integral. And what ever happened to the preceding century of Ottoman and Arab reforms, both Muslim and secular, which in many basic ways changed and updated Middle Eastern societies and institutions? The fact that the Europeans occupied portions of the Middle East not primarily to "modernize and liberalize" but to serve their own economic and political interests, and often to discourage indigenous efforts that might have led to radical changes in the status quo, should be given at least equal causal weight with "Moslem *immobilism*," even allowing for the imprecision and stereotypical notions implicit in that phrase. In the last analysis, it was because the Europeans occupied their lands and controlled their destinies that the Arabs opposed them.

Professor Langer in his introduction has unwittingly done a disservice to Sachar and the reader by insisting on the "objectivity" of this book. While Sachar has obviously made every effort to show all sides of the issues involved

and has distributed his criticisms freely, nevertheless the ambience of the book is Western and pro-Zionist (but not without understanding for the Arab view). The Arab uprisings are consistently described in terroristic terms, while the Haganah and Irgun are labeled as "flying columns" and "mobile commando groups" and their actions are designated as "active defense" (pp. 90-93). Haj Amin was "notorious for his deviousness," but no mention is made of the devious tactics of Chaim Weizmann or Ben-Gurion. British appeasement of the Arabs, Sachar states (p. 99), was to be at Jewish expense. But, to the Arabs, was not every instance of appeasement of the Jews made at the expense of the Palestinians?

The point here is that although Sachar has given a particular coloration to his book, because he has at the same time made a sincere and largely successful effort to present a balanced account of the facts and events, he has produced something more interesting and honest than the "objectivity" claimed for him by Langer. Despite these criticisms, this book is welcomed both for the wealth of its information and for Sachar's undertaking of a complex problem.

THOMAS NAFF

University of Pennsylvania

JOSEF KORBEL. *Détente in Europe: Real or Imaginary?* Princeton: Princeton University Press, 1972. Pp. viii, 302. \$10.00.

This new book by the well-known specialist in international affairs dwells on the present state of affairs between East and West. The key word is détente, and Korbel dates its beginnings in the 1960s. Examining ideological, political, and economic aspects of this relaxation of tensions the author concludes that "the détente is real, although limited in nature and scope, but its durability is at best uncertain and at worst illusory." I find myself in agreement with this formula as well as with Korbel's final conclusion that real cooperation could only replace the present vague détente after an internal liberalization of the Soviet Union. No less a person than the famous Soviet scientist Sakharov recently stressed this very point.

Korbel's statement that "modern history has demonstrated the inability of a democratic

country successfully to infiltrate and subvert a totalitarian regime," may be understood to mean that the rest of the world is condemned to the role of a mere spectator of evolutionary processes in the Communist bloc. But the author is apparently warning here against facile beliefs in the efficacy of propaganda without necessarily negating the usefulness of subtler means of influencing the East.

The main value of Korbel's study, especially to a historian, lies in showing that détente means different things to various powers, particularly the West European states. Like the "spirit of Locarno" the term lends itself to divergent interpretations. The Soviet Union regards it largely as a means to achieve an international legal confirmation of the status quo in East Central Europe and to diminish American influence in Europe. Britain sees it in terms of accommodation and normalization of relations between the two parts of Europe. It is a low-key approach, and British involvement in East Central Europe appears minimal. France, during the de Gaulle period, promoted an ambitious program expressed through the slogan "détente to entente to cooperation." In the name of France's grandeur, Paris sought to achieve some restraints over West Germany, an extension of French influences to the states of East Central Europe, and a minimization of the American influence on the European continent. This policy produced few results, and from 1970 the focus of détente policies shifted to Bonn. Brandt's *Ostpolitik*, to which Korbel devotes a good part of his book, is represented as pursuing the solution of the problem of a divided Germany and establishing on a new footing the relations between Germany and her Eastern neighbors.

In reading Korbel's discussion of the policies of the three West European states I had occasionally the feeling that *plus ça change plus c'est la même chose*. Britain's relative aloofness to East Central Europe; France's vigorous diplomacy unaccompanied by economic achievements; German presence, at least economic, in Eastern Europe; Western fascination with the Russian market—surely we have seen all this before. But Korbel does not pursue these interesting historical parallels. His objective is to examine the present, and his thesis that the détente is a phase in the East-West relations, although a welcomed one, and not a panacea,

seems to be borne out by events that have taken place after the publication of his book.

PIOTR S. WANDYCZ  
Yale University

## ANCIENT

FRANK MOORE CROSS. *Canaanite Myth and Hebrew Epic: Essays in the History of the Religion of Israel*. Cambridge, Mass.: Harvard University Press. 1973. Pp. xviii, 376. \$14.00.

These essays reflect the impact of excavated inscriptions on biblical religion and institutions. The most important category of such inscriptions, which has revolutionized Old Testament studies, is from Ugarit. A better-publicized group of such texts is the collection of Dead Sea Scrolls from Qumran, a small archeological site whose early history is the topic of the final chapter.

This book deals with various aspects of Israelite religion against the background of newly found material, for example, concepts and names of the gods, cult, the tribal league and monarchy, prophecy, covenant, and literary criticism ("Documentary Hypothesis") of the Old Testament. The latter continues to exert so great a force on biblical historiography that it merits careful scrutiny.

The essence of the documentary hypothesis is that the Pentateuch has been compiled by a redactor from a number of unknown authors, notably "E" (who calls God Elohim), "J" (who calls Him Jahweh), D (whose input is Deuteronomic), and P (whose orientation is Priestly). The extent and dates of the J, E, D, and P documents are disputed, but the system is in general accepted.

The first creation (Gen. 1:1-2:3), in which God is called Elohim, used to be attributed to E; the second creation (Gen. 2:4-3:24), in which God is called Jahweh-Elohim, used to be attributed to JE because presumably J and E authorship had been combined.

The fact is that Genesis 1-3 portrays successive and interrelated creations: the first transitory one by Elohim and the second permanent one by his son Jahweh-Elohim. In Ugaritic El (Elohim) is the father of Yw-El (Jahweh-Elohim). Proverbs 30:4 alludes to the two crea-

tors as father and son. In the opening chapter of John, the Son is referred to as a creator.

Precisely what the author of this book means (on p. 303) by attributing parts of Genesis to JE is not clear. That the Pentateuch embodies earlier sources is certain; that any source such as JE ever existed is another matter.

On page 219 the office of "judge" ("ruler") is described as "charismatic." Charisma (divine inspiration) was necessary but not sufficient for qualifying the rulers known as the Judges. The latter had also to belong to the ruling aristocracy at least on their fathers' side. The candidate could be the youngest son of a small clan from the least of the noble tribes, but he had to be sired by a member of the aristocracy—or accepted as such.

CYRUS H. GORDON  
New York University

GEORGES DUMÉZIL. *From Myth to Fiction: The Saga of Hadingus*. Translated by DEREK COLTMAN. Chicago: University of Chicago Press. 1973. Pp. xii, 253. \$12.50.

GEORGES DUMÉZIL. *Heur et malheur du guerrier: Aspects mythiques de la fonction guerrière chez les Indo-Européens*. ("Collection Hier.") Paris: Presses Universitaires de France. 1969. Pp. 148. 20 fr.

GEORGES DUMÉZIL. *The Destiny of the Warrior*. Translated by ALF HILTEBEITEL. Chicago: University of Chicago Press. 1970. Pp. xv, 168. \$8.75.

GEORGES DUMÉZIL. *The Destiny of a King*. Translated by ALF HILTEBEITEL. Chicago: University of Chicago Press. 1973. Pp. 155. \$10.00.

For the past twenty-five years Georges Dumézil has been professor of Indo-European civilization at the Collège de France. With massive erudition and in rapid succession he presents us with a number of books translated and revised from his previous works. In spite of their seeming diversity, they all deal with his favorite theme, a comparison of the mythology of the various Indo-European peoples. His main theses, recently made available to the English-speaking world through the translation of *Archaic Roman Religion* (AHR, 77 [1972]: 1099-1100), is that the Indo-European peoples divided life into three functions—sovereignty, physical force, and fecundity. These functions can be found distributed among three or more

mythological personages such as the Vedic Mitra-Varuna, Indra, and the Nāsatya twins, and the Nordic Odin, Thor, and Freyr, or among legendary human kings, such as the Roman Romulus-Numa Pompilius, Tullus Hostilius, and Ancus Marcius. Dumézil is convinced of a "conscious authorship" responsible for the transposition of archaic mythology to legendary human history, but in most cases we are too far removed from the authors to perceive this transposition. The only exception is the Scandinavian world. Here we have preserved, on one hand, the Eddas and the Sagas, which retain much of the original mythology, and, on the other, the works of Snorri Sturluson in Iceland and Saxo Grammaticus in Denmark who transposed this mythology into purported history. Since Dumézil started his comparative research with a study of Scandinavian mythology (*Mythes et Dieux des Germains* [1939]), his views on the Scandinavian problems are particularly interesting and well founded. He has dealt with the authorship of Snorri in *Loki* (1948) of which he promises a radically revised edition. *From Myth to Fiction* deals with Saxo Grammaticus, the Danish medieval historian. The first half of the book contains a long essay analyzing Saxo's "Saga of Hadingus," first published in 1953 from lectures given at the Collège de France. The second half consists of appendixes of which all but the last deal with various aspects of Saxo's technique in transforming mythological material into "history." *From Myth to Fiction*, then, is a study of Saxo's literary procedures. That neither the title nor the subtitle makes this clear is probably due to the fact that in 1953 Dumézil kept the possibility open that the alterations made in Saxo's version were not his own but were derived from an unknown Icelandic source. His later studies leave no room for this hypothesis, and he now seems convinced that Saxo is directly responsible for the changes. Thus the transposition from religion to history that is beyond the pale of historical investigation elsewhere in Indo-European civilizations can be examined closely in the Nordic context. The story of Hadingus is found in the beginning of Saxo's *Gesta Danorum* where he is depicted as an early Danish king and a mighty warrior, a worthy predecessor to Saxo's own king Waldemar the Victorious. This idea has

generally been accepted in modern scholarship, but Dumézil returns to an earlier view that pointed to analogies between the saga of Hadingus and the myth of the god Njörðr. Beyond these Dumézil is able to show that in fact the career of Hadingus generally follows that of Njörðr with a clear division between two periods, one representing the function of sovereignty and the other that of fecundity. In the appendixes he treats other mythological material transformed by Saxo, for example, how the mild and peaceloving Baldr of myth was changed to Saxo's belligerent and jealous Balderus. Dumézil concludes that Saxo, indifferent to the former mythological value of the stories, took them apart and changed them according to his literary whim, while still leaving the mythological structure intact. The translation by Derek Coltman is done well, but Adam de Brême (p. 52) and Adam Bremensis (p. 86) should be Adam of Bremen.

*Heur et malheur du guerrier*, appearing simultaneously as *The Destiny of the Warrior*, is largely an updating of a previous work from 1956. The revisions have been done "in an effort to prepare for the inevitable autopsy as proper a cadaver as possible, that is, to deliver to the critic of the near future, in an organized and improved form, the results of the endeavors, of varying success, carried out over the past thirty years" (p. xiv). The subject of his examination is the function of force, and he shows in the first part of the book the similarities between the Indian warrior-god Indra and the legendary Roman warrior-king Tullus Hostilius. The second part focuses on the unique solitude of the warrior type, who represents force and opposes the representatives of the two other functions of sovereignty and fecundity, who often appear in pairs. Solitude means freedom that may lead to transgression. Dumézil shows how the theme of three successive sins committed by the warrior is found in the Indian god Indra, the Scandinavian Viking Starcathe-  
rus, and the Greek hero Heracles. The last and weakest part of the book clarifies some of Indra's exploits by drawing parallels with scenes from Iranian, Armenian, Scandinavian, Irish, and American Indian mythology and folklore.

*The Destiny of a King*, translated from the French edition of 1971, publishes the Haskell

Lectures at the University of Chicago. Of interest mainly to mythographers, this book contrasts the Iranian legendary king Yima with his three counterparts in Indian mythology—Yama, Yayāti, and Vasu Uparicara. The career of Yayāti clearly illustrates the author's trifunctional structure, and a comparison between Yayāti's daughter Mādhavī and the Irish queen Medb convincingly shows similarities within the vast Indo-European family. The book ends with some fascinating ideas on the phenomenon that kings often surrounded themselves with virgins to protect their kingship.

JENNY JOCHENS  
Towson State College

M. I. FINLEY. *The Ancient Economy*. (Sather Classical Lectures, volume 43.) Berkeley and Los Angeles: University of California Press. 1973. Pp. 222. \$8.95.

WILLIAM I. DAVISSON and JAMES E. HARPER. *European Economic History*. Volume 1, *The Ancient World*. New York: Appleton-Century-Crofts. 1972. Pp. xiv, 288. \$8.95.

In *The Ancient Economy* M. I. Finley brings together significant conclusions and insights he has put forth in individual studies over the past decades, adds a number that are new, and presents them all with the lucidity, trenchant style, and probing analysis that is the hallmark of his work.

Finley mercilessly delivers the *coup de grâce* to many a sacred cow that still disfigures current text and reference books. An initial chapter on social and economic class and status contains a discussion that should once and for all end talk of a clash of interest between the two great groups of Roman rich, the senators and the equites, the one being viewed as landholders and the others as moneyed interests; both were landholders, and they cooperated happily and profitably in financial ventures. Chapter 2, on slavery (and perhaps the highlight of the volume), points out hitherto unrecognized features revealed by recent scholarship, much of it Finley's own: that many slaves—for example, Roman slaves who had a *peculium*, property of their own—were hardly slaves at all; that in the overall picture, other forms of dependent labor—debt-bondage, clientage, helotage—were at most times as important as chattel slavery and at certain times

more important: that there was never a significant free labor force, hence all argument about slave labor driving out free is pointless and all conjecture about their comparative costs meaningless; that Roman Stoicism and Christianity, contrary to what is so often said, had scant effect on the decline of slavery. A chapter on landlords and peasants emphasizes the overwhelming importance of agriculture in the ancient economy and demonstrates that the ancient attitude toward landholding was based on social and psychological values far more than economic: people held on to land for emotional reasons and acquired it for status; they rarely if ever looked for the maximum profit that could be wrung from it. Indeed, the whole thrust of Finley's book is to make patently clear that in ancient times both individuals and states thought primarily in political, social, and psychological terms and only secondarily in economic.

There are areas where Finley seems to press his views too far. In emphasizing agriculture he rightly scales down the importance of manufacture—yet perhaps more than is called for. There is a significant new form of evidence that he does not cite, the cargoes found in ancient wrecks. At least three have been found loaded with thousands of roof tiles, another with thousands of lamps, yet another was carrying hand grain mills—all this may point to a volume of manufacture and movement of its products beyond what Finley cares to allow. Again, in emphasizing, and rightly so, the basic self-sufficiency of most ancient communities, he sets stringent limits (compare p. 165) to the nature and extent of seaborne trade. For example, he limits the Greek traffic in wine to famed regional vintages, asserting that *vin ordinaire* was normally produced at home (p. 133). Yet, as the ubiquitous Cnidian and Rhodian shipping jars reveal, it went wherever nothing better was available, and that amounted to no negligible volume.

Finley has given us an important and provocative book. It is a pity that it did not appear in time to be digested by the authors of volume one, *The Ancient World*, in a new series on Europe's economic history. For Davison and Harper repeat much of the time-honored doctrine that Finley has so effectively demolished. They have, as a matter of fact, dared



to tread the length and breadth of a highly controversial field relying almost wholly on secondary sources, including some that are mere popular works or undergraduate texts. In order to set the scene of their story, they provide rapid surveys of political events that all too often come out as indigestible globs of names, dates, and actions (compare pp. 74-75).

All this is too bad since the authors, when they can leave the trees of their narrative and survey the forest, reveal clear and accurate vision and arrive at conclusions close to those reached by Finley, as in their review of the role of commerce and manufacturing in the economy of the Roman Empire (pp. 209-14) or their final chapter summarizing the limitations of the ancient economic system (pp. 235-40). But these bright spots are few and far apart; in between lie a welter of highly questionable statements, misunderstandings (compare, for example, their remarks on Greek economic thought [pp. 122-28] with Finley's [pp. 19-21]), exaggerations (for example, on the economic aspect of the Peloponnesian War [p. 127]), and, inevitably, uncritical use of sources (compare p. 184 n.24, with Finley, p. 183 n.52, for a glaring example).

LIONEL CASSON  
New York University

MOLLY MILLER. *The Thalassocracies*. (Studies in Chronography, 2.) Albany: State University of New York Press. 1971. Pp. xii, 185. \$10.00.

Along the lines of earlier researches published in *The Sicilian Colony Dates* (1970), Dr. Miller here takes up the problems of chronology presented by the ancient registers in which were recorded the traditional dates of the sea powers that succeeded one another in the Greek world from the Trojan War to the Persian Wars. The concept of thalassocracy, Dr. Miller believes, represents a principle of universal history in the Greek world that will provide a truer picture of early Greek history than the established tradition of the development of the city-state or the sum of local histories. In addition to being incomplete and in part damaged, our surviving records of the early chronographic traditions present difficulties because the compilers arranged events in periods which prove to be groupings that are based on dif-

ferent methods of reckoning. These periods comprise either twenty-seven or thirty-nine years, so that according to the chronographic conventions employed in different traditions, the duration of individual thalassocracies may vary.

Dr. Miller undertakes to correct these discrepancies and to synchronize the entries in the belief that an original "Register of Thalassocracies" may be visualized. The effort involves a formidable exercise in speculative arithmetic and number theory, combined with what are thought to be necessary restorations and rearrangements in the texts. It is assumed that the reader is a scholar who is at home in this method of research and who shares Dr. Miller's earnest conviction that the method is valid and that, by means of the ingenious manipulation of figures, it will produce acceptable conclusions. There will be others, however, who in spite of Dr. Miller's learning and enthusiasm will share the skepticism of scholars who have dealt with this problem, notably J. L. Myres (whose studies Dr. Miller cites, p. 2 n.4; p. 180). In any case some scholars will be dubious about a tradition of early Greek history that has to be reconstructed in this fashion.

GLANVILLE DOWNEY  
Indiana University,  
Bloomington

JEAN DES GAGNIERS. *L'Acropole d'Athènes*. Quebec: Presses de l'Université Laval; distrib. by International Scholarly Book Services, Portland, Ore. 1971. Pp. 108. \$9.95.

C. A. DOXIADIS. *Architectural Space in Ancient Greece*. Translated and edited by JAQUELINE TYRWHITT. Cambridge, Mass.: MIT Press. 1972. Pp. xxxvii, 184. \$12.50.

Jean des Gagniers's *L'Acropole d'Athènes* is a popular publication, fitted out with the usual encomiastic illustrations, indifferently chosen and uneven in quality, with a simplistic, gushy text unworthy of a university press, and clearly inflated to a trim size unjustified by substance. This volume purports to be an introduction to the history and development of the Athenian Acropolis from the earliest phases in the second millennium B.C. to modern times. It fails to achieve this purpose, being neither adequately

descriptive nor historically comprehensive, as references to John (not Jean) Travlos's *Pictorial Dictionary of Ancient Athens* (1971) and to the still vital but old book by Gerhart Rodenwaldt (not Rodenwalt) and Walter Hege on the Acropolis (1930) make all too evident. It is impossible to recommend des Gagniers's book to the general public as either a coffee-table treasury or a solid introduction to the monuments of the Acropolis because the volume so completely lacks both the comprehensive coverage and intellectual content already available in other publications that are similarly intended for the interested lay reader.

Very different, however, is the publication of C. A. Doxiadis's dissertation on ancient Greek site planning, which has now been translated from German and handsomely produced by the MIT Press. In 1937 Doxiadis—now certainly better known as an architect and regional planner and as the leader of a theoretical group concerned with the entire physical environment in which man functions (Ekistics)—undertook his challenging study of Greek architecture without limiting himself to the traditional examination of individual buildings, usually a temple, conceived as an independent architectural item. Instead, convinced by his theory that the Greeks disposed buildings in space according to principles of cognition, Doxiadis occupied himself with defining the spatial, visual, and mathematical organization of twenty-nine Greek sites, for the most part sacred precincts and, with the exception of Palmyra, located on mainland Greece or in the Greek towns of Sicily and western Asia Minor. He tried to expose the rational system of architectural siting in Greek design by which the ancient architect sought not only to create a coherent architectural ensemble within a site, but, also, to make that coherency visually and conceptually accessible to the observer from significant points of view determined largely by the placement of entrances, the temporal path of ritual, and an insistence upon the solid, three-dimensionality of architectural form.

Doxiadis presented his theory and its ramifications in a brief introduction, while the greater part of his study consisted of an empirical demonstration of the validity of that theory based on a careful examination of the

selected sites, using the best available archaeological information and constructing illustrative plans and reconstruction. The basis of this theory was the discovery that the human viewpoint was the determining factor in Greek design and that point was established primarily at the place from which the site as a whole could be observed, usually the principal entrance. Subsequently the following principles were used: each important building was visible as a solid body from the viewpoint, because three corners were revealed, or the building was completely concealed; the radii from the vantage point that determined the position of these three corners formed certain specific angles, equal in size on each site, and these angles in turn fell into two categories, corresponding to a division of the total field of vision (a circle) into ten or twelve parts; the twelve-part division was used for the Doric style, the ten-part for the Ionic. Furthermore, the position of buildings within the site was determined not only by the angle of vision but also by their distance from the viewpoint, and those distances were based on simple geometric ratios derived from the angle of vision; one such angle, often in the center of the visual field, was left open as a "sacred way," usually oriented toward east or west according to the demands of the ritual. And last, the buildings were frequently disposed so as to incorporate or accentuate features of the landscape and thus develop a unified (natural) composition.

Although the rational and humanistic tendencies of Greek thought have long been recognized, Doxiadis's attempt to demonstrate that Greek artists wanted to objectify the architectural experience through a comprehensive system whereby the constituent buildings on a site, the site itself, and the surrounding landscape could be brought into a stereometric whole, has met with neither widespread response nor acceptance. To many archeologists and classicists his ideas were controversial and unproved, lacking admittedly any explicit textual basis in Greek literature, although Doxiadis influenced R. D. Martiensen, *The Idea of Space in Greek Architecture* (1956), and especially V. J. Scully, Jr., *The Earth, the Temple, and the Gods. Greek Sacred Architecture* (1962). Perhaps his is a still precocious idea of general as well as specific applicability, and the

MIT Press is to be complimented on the republication of this important treatise. Correctly, the editor preserved the original text without revision and thereby fixed its place in the history of ideas and of classical and archeological scholarship. Nevertheless, although a few, inadequate citations to work on Doxiadis's sites after 1937 were added in this edition, it would have been very useful to list, and possibly even summarize, the principal reviews and characterize the reception this book has received in the past thirty-five years.

RICHARD BRILLIANT  
Columbia University

N. G. L. HAMMOND. *Studies in Greek History: A Companion Volume to A History of Greece to 322 B.C.* New York: Oxford University Press. 1973. Pp. xvi, 574, 7 plates. \$40.00.

As the author explains in the preface, this book is a companion volume to his well-known textbook, *A History of Greece to 322 B.C.* It takes up selected controversial points in Greek history and is intended to help students by giving detailed evidence for, and reasons behind, conclusions presented in the textbook. The book is divided into sixteen chapters, treating subjects, arranged chronologically, beginning with the Mycenaean world and concluding with the Battle of Chaeronea. The first three chapters are concerned with the earliest periods of Greek history. They present essentially new material and are based on recent archeological discoveries. The remaining thirteen are articles published by the author between 1937 and 1968, revised and brought up to date for republication. The material is illustrated with twenty-three figures and maps of high quality and by seven pages of photographic plates, some of which, unfortunately, are unclear.

Discussion of subject matter will be restricted to the first three chapters, since the others develop viewpoints already expounded in the *History of Greece*. In treating the "Origins of Some Mycenaean Rulers" (chapter 1) the author notes the evolution of mortuary chambers into tholos tombs and a marked conservatism in burial practices at Mycenae in the period from around 1700 to 1300 B.C. He believes that such practices were not indigenous to Mycenaean Greece and that they were imported

by intruders. These, he thinks, came from central and southern Albania, spreading southward gradually in the Middle Helladic period. In a related chapter (chapter 2), "The Arrival of Greek Speech in the Southern Balkans," he develops the view that speakers of proto-Greek were present in Albania, southwest Macedonia, and Leukas around 2600 B.C. and that they evolved the dialects spoken in this historical period before penetrating the Greek peninsula beginning around 1900 B.C. Before the end of the Bronze Age, speakers of the Ionic and Achaean dialects were established in southern Hellas, those speaking Doric remaining in their original homeland, Epirus. In discussing "The Impulses which Started the Dorian Invasion" (chapter 3), the author argues that archeology and literary tradition suggest that the Dorian invasion of Greece began around 1140 to 1120 B.C. and that this, in turn, was caused by pressures on the Dorians by other peoples coming from the north.

These and other arguments are buttressed by meticulous scholarship characterized by a command over original sources and a painstaking attention to minutiae, which do not, of course, preclude disagreement with his conclusions on the part of other authorities. As might be expected, the general thrust and coloring of the author's *History of Greece* tend to be reflected in this companion volume. His main interests are clearly topographical and military, and he is at his best when reconstructing battles and bringing to bear his unexcelled firsthand acquaintance with the Greek countryside. His interests also affect the balance of topicality, eight of the chapters having a strong topographical or military orientation; only one, treating "Personal Freedom and its Limitations in Aeschylus' *Oresteia*," deals with a cultural subject.

This book will serve the specialist who seeks a handy, one-volume anthology of the more important papers of a distinguished Hellenist. Whether it will be very useful for students as a companion to a textbook of Greek history is another matter. The choice of topics is much too limited and even idiosyncratic to provide a reasonably wide range of collateral reading that students might expect, even though the author cannot be faulted for choosing to include in an anthology of his own articles those on topics to

which his contribution has been the most original. The high level of sophistication of the arguments in many cases also precludes their being of much help to the average American undergraduate, even if he has Greek.

WILLIAM G. SINNIGEN  
Hunter College,  
City University of New York

T. B. L. WEBSTER. *Athenian Culture and Society*. Berkeley and Los Angeles: University of California Press. 1973. Pp. 309. \$10.00.

The cultural achievement of classical Athens was firmly rooted in its political, economic, and social life and in the nature of the citizens it produced. To describe the manner in which Athenian poetry and drama, art and architecture, history and philosophy, and education and religion were created by and reacted upon the social fabric of a small society in the 150 years from the Persian Wars to Alexander is, however, no easy task. It is precisely this that T. B. L. Webster has attempted here.

Webster begins with a brief overview of political, economic, and religious life and then proceeds to a topical analysis of the Athenian cultural achievement. For him the relationship between culture and society is to be found by examining, first, the nature and competence of audience or patron and the social position of the practitioners of the art; second, the aim and achievements of the practitioners; and, finally, the growth of critical theory and the interchange of methods and ideas among the various arts. Within the boundaries thus set, Webster creates a number of interesting and valuable chapters, especially those on pottery, architecture, sculpture, and painting, drama, and geography, medicine, and history.

Webster's program is, however, limited. It tends to treat the "society" portion of the formula "culture and society" as if it consisted of nothing more than "audience" and patron in addition to some immediate political influences. This is apparent, for example, when Webster writes: "A small city with many meeting places, an open society, a viable social structure, intelligent patronage, educated audiences, the spirit of competition, these are the things that classical Athens offered to artists, poets, and thinkers, and they are partly responsible for

the flowering of culture" (p. 265). Surely there was a deeper relationship between culture and society than this would imply. The very categories of Athenian culture (drama, history, rhetoric, painted pottery, and so forth) are by no means obvious or necessary, and they clearly derive from the felt needs and requirements of social and economic life. To raise the question "Why does a society produce the particular cultural forms it does?" takes us to the heart of the culture-society relationship, and it is precisely there that Webster refuses to go.

This refusal is doubtless a product of Webster's shortcomings as a historian of political and economic life; the chapters on these topics are indeed inferior to those on art and literature, Webster's main interests. The book does not, therefore, offer quite as much as it initially promised. Nevertheless, Webster succeeds to a large extent in portraying the richness of Athenian culture in the refreshing perspective of its ancient audience, and the book is well worth reading for those (mainly nonspecialists) who wish to learn more about the civilization of classical Athens.

ROBERT A. PADGUG  
Rutgers University,  
New Brunswick

D. A. RUSSELL. *Plutarch*. (Classical Life and Letters.) New York: Charles Scribner's Sons. 1973. Pp. 183. \$8.95.

This is the latest in a series of books on Plutarch that has appeared in the last few years. The first three chapters contain general background information on Plutarch's life, career and family, his language and style, the extent of his reading, and the question of his use of sources. In chapters 4 and 5 Russell discusses Plutarch's philosophy and ethics. He accepts the view of Plutarch as "a declared and consistent Platonist" (p. 63) without acknowledging the opinions of Heinrich Dörrie and others who have recently attempted to qualify this somewhat too rigid analysis of Plutarch's philosophical orientation. The discussion of Plutarch's ethical ideas in chapter 5 is basically sound. In the course of these two chapters the reader is casually introduced to many of the works in the *Moralia*.

In chapter 6 we have a good study of the

*Lives*, especially with regard to their structure and Plutarch's purpose in writing them. Historians should find this discussion valuable as a guide to the use of Plutarch as a source for the individuals about whom he writes. Chapter 7 provides a detailed analysis of the *Alcibiades* as a sample biography, and in the following chapter Russell remarks upon various Roman *Lives*, in particular the *Antony*. He closes with a survey of Plutarch's influence on later antiquity and succeeding ages and takes note of the work of Jacques Amyot, Thomas North, and Philemon Holland in translating Plutarch. The brief conclusion in chapter 10 is mostly a continuation of the preceding chapter with a minimal summation at the end. The appendix contains useful catalogs of the individual works of the *Moralia* and the *Lives* in pairs, with a third section on editions and translations. The bibliography lists some of the more important works on Plutarch with very concise evaluations of several.

The author's objective, as stated in the preface, is "to explain what it is like to read Plutarch, and what I think one needs to bear in mind in order to read him with understanding." In keeping with this purpose the book abounds with passages from Plutarch's writings, some a page or more in length. Russell's presentation is at times sketchy and discursive, especially in the early chapters. In general the book is too brief and hardly does more than touch on most aspects of Plutarch and his works. It adds little to the recent work of R. H. Barrow and C. P. Jones. It is to be recommended to the nonspecialist with little or no knowledge of Plutarch as a pleasant but limited introduction to that author.

L. J. SIMMS  
College of Charleston

MICHAEL GRANT. *Roman Myths*. New York: Charles Scribner's Sons. 1971. Pp. xviii, 293. \$10.00.

In the modern connotation, the study of myths is used to signify a systematic examination of the traditional narratives of a particular group of people in order that their concepts concerning religion, origin, and relations might be known. It is assumed by mythologists that the basis of mythology has a natural development

among the ordinary populace. But in his investigation of Roman legends, Grant points out that their mythology in the interest of national or family patriotism was artificially created in a piecemeal fashion over a long period of time. Thus Roman mythology must be approached with these considerations in mind. The purpose of Grant's latest work is "to offer a much more detailed treatment of the Roman mythology, treating it as an entity in its own right, and seeking to explain how it acquired its very remarkable characteristics, and how and why it became such an enormously influential factor" (p. xviii).

Accurate Roman myths deal only with stories relating to the origins of Rome itself rather than accounts of the Olympian gods. In the beginning of his study Grant considers all the Greek, Etruscan, and Latin sources of information for Roman legend. The Etruscan influence is given adequate recognition, while the Sabine element is de-emphasized. He next takes up the Trojan origin and background of Aeneas, his relationship with Dido, and the development of Roman virtue. This is followed by a review of Romulus's birth, the legendary foundation of Rome, and the affiliation with the Sabine culture. Grant rejects as pure fiction the concept that Rome was founded in the eighth century B.C.; archeology has proven that the first settlement upon the Palatine took place about 1000 B.C. Numa and the Etruscan monarchy are also given full treatment. In the conclusion the author reflects upon the question of myths surrounding the establishment of the Republic, Camillus, and the legends of later Roman history, in which era the greater Roman families effected and adjusted mythology for their own ends.

A very few Roman myths contain grains or enclosed fragments of historical truth. However, Grant's analysis produces an imaginative parahistory, which is informative because of its illumination of the Roman mentality.

The book is well written, as are all of Grant's undertakings. The work includes an excellent bibliography of recent writings, numerous plates, and detailed maps. The study will be of great value to the comparative mythologist.

STEPHEN J. SIMON  
Appalachian State University

D. R. SHACKLETON BAILEY. *Cicero*. (Classical Life and Letters.) New York: Charles Scribner's Sons. 1971. Pp. xii, 290. \$10.00.

Professor Shackleton Bailey aims to value Cicero "not as statesman, moralist, and author, but as the vivid, versatile, gay, infinitely conversable being who captivated his society and has preserved so much of himself and it in his correspondence." He has accordingly quoted generously, in translation, not only from the *Letters to Atticus*, which he has edited and translated, with commentary (7 vols., 1965-70), but also from the rest of the correspondence. His translations, as always, are fluent, lucid, and exact and often catch so admirably the tone of the letters.

The method, however, has its drawbacks, as the author himself recognizes, for the space assigned to events in Cicero's life is made to depend on the extent to which they figure in the letters. That in turn depends not only on such variables as whether Atticus and Cicero were separated from one another at the time but also on the mere accident of survival. Hence Cicero's consulship, otherwise relatively well documented, receives a little over three pages, while about two-thirds of the book is devoted to the last ten years of Cicero's life. Further, though Cicero's correspondence is essential evidence for the social life of the late Republic, the Latin-less reader, for whom the book is written, needs more information than can be provided within the compass of this work.

In pursuance of his purpose the author explores Cicero's relations with others: his imbroglio with Mark Antony is well handled, and the causes and nature of his quarrel with Quintus Cicero are perceptively discussed. In the latter case, however, Professor Shackleton Bailey may rely too much on the silence of the letters. More questionably, still, he sees in Cicero's desire to excel the complement to his inner insecurity, inferring from the absence of references to Cicero's mother an early emotional lack. The suggestion is undeniably thought-provoking but less cogent if one recollects that for the ruling class of Cicero's day political ambition was a way of life, and that for the "new man," in particular, the desire to excel was a constant goad.

The author's estimate of Cicero's literary

works is distinctly cool, and he sets no great store by his actions as a politician. One is finally driven to ask, however, whether Cicero really deserves to be commemorated principally for his familial and social character, though the volume itself affords ample pleasure and amusement.

G. M. PAUL

McMaster University

JOHN BRISCOE. *A Commentary on Livy, Books XXXI-XXXIII*. New York: Oxford University Press. 1973. Pp. xviii, 370.

Books 31-33 of Livy contain a detailed narrative from 201 B.C. to 195 B.C., the beginning of one of the most exciting and most politically important eras in Roman Republican history. It begins with Rome, just after her victory over Hannibal, poised at the threshold of her adventures in the complicated stadium of Greek politics. She enters it with no realistic eastern policy at all and at the end of the game finds herself mistress of the whole eastern Mediterranean world. Livy is our primary source, along with Polybius, for this early part of the era, and generations of scholars have poured over his text and expounded its meaning from many different points of view, one of the most important being the motive or motives for the steady change in Roman policy during this second century. Some historians have seen a steady deterioration of the character of the ruling class in Rome, corrupted, we are told, by Greek luxury, aflame with new and dangerous political philosophies, conscious of Rome's manifest destiny, and convinced of its own ability to rule or dominate.

In 1965 R. M. Ogilvie published a new and detailed commentary on the first five books of Livy, and it was at once hailed in many quarters as an excellent beginning to a full and more modern commentary on Rome's greatest historian. But since then the work has not gone on, and others will have to pick up where Ogilvie left off. Briscoe's present work will fill a part of what still must be done with Livy. Preceded by forty-eight pages of introduction concerned with sources, methods of composition, language and style, speeches in the text, politics of the age, a general account of the events, and the text used by Briscoe, the

bulk of the book is confined to a very detailed and reliable commentary on every important phrase or word found in books 31–33. Briscoe has given us a very sound guide to what Livy tells us about events and politics and about the people and society of that age. Whenever the modern historian wishes further information about the meaning of what Livy narrates, he will do well to examine Briscoe's commentary with its wealth of bibliographical information. There are a few omissions, but none that should discourage use of the commentary.

It may be noted here that in the all-important matter of the Roman senate's motive for declaring war on Philip V, Briscoe believes that Maurice Holleaux was right, that the "news brought by the Rhodian and Pergamene ambassadors in the autumn of 201 had a serious effect on the senate, though they were as much affected by the news of Philip's actual aggressions as by his reported pact with Antiochus."

Adequate indexes are provided and the proofreading had been done very, very carefully; as one would expect from such a publishing house. All serious students of Roman involvement in the Greek east will have to have this book. And let us hope that Briscoe continues to work on other parts of Livy. That would permit a greater and more desirable kind of unity that would otherwise be lacking.

ROBERT K. SHERK

State University of New York,  
Buffalo

MICHAEL GRANT. *The Jews in the Roman World*. New York: Charles Scribner's Sons. 1973. Pp. xv, 347. \$10.00.

MICHAEL GRANT. *Herod the Great*. New York: American Heritage Press. 1971. Pp. 272. \$12.95.

These two recent and excellent books by Michael Grant treat the same subject in different, but complementary ways. Each one is fascinating to read by itself; together they illuminate a profoundly moving and important theme in ancient history. The broad subject is the relationship between the Jews in the Near East, particularly in Palestine after it became the client-kingdom of Judaea, and the Roman government. The story is told, not as is so often done by classical historians, from the Roman vantage point, but rather from the inside out.

The focus is on the Jews and how they viewed and coped with their confrontation with Roman power.

*The Jews in the Roman World* opens with an introductory section that includes a chapter on Jewish traditions. In it Grant emphasizes a central point of his study—that since the Jews alone among the Greeks and Romans have had a continuous history, despite the paucity of sources for the Roman period and despite the difficulties of interpreting some of them (Josephus, for example), it is possible to reach back into Jewish history for an understanding of the inner workings of the Greco-Roman Empire. In the remaining introductory chapters Grant describes the Jewish attitudes toward Hellenization and the dissensions that emerged in Palestine during the Maccabean period. The major part of the book covers the era that began when Pompey annexed Syria in 63 B.C., and it became clear that Palestine, inevitably, would be next, and that ended when the Empire was converted to Christianity.

Roman-Jewish relations cannot be understood without first understanding the whole matrix of Greek, Oriental, and Roman rivalries that existed in the Near East. It is a complicated, though necessary, background, and in lesser hands it might simply have remained complicated. But Grant has threaded his way carefully and with great clarity through that tangle of relationships by centering on the political rivalries and providing religious and philosophical content only where they were actually relevant. For Judaea entered the Empire as a self-governing client-kingdom, and its status owed less to its distinctive religion than to its position as a buffer against the Parthian Empire. Judaea was critically important to the Romans once they became embroiled in the Near East, and few people among the Jews seem to have assessed that situation as accurately as Herod.

Grant's biography *Herod the Great* is a deeply interesting study of a man whose career was a microcosm of all the problems that confronted Judaea throughout the entire course of its relations with pagan Rome. Herod had to find a balance between the demands of his Jewish subjects that Judaea retain its Jewishness and its integrity and the demands of the Romans that he administer a loyal, pro-Roman

state. As Grant makes clear, Herod did remarkably well. His bad press has been based on the lamentable last years of his life when his family crises seem to have deranged him, on the so-called Massacre of the Innocents (for which Grant finds no reliable evidence), and on the fact that he, like Josephus, was considered a traitor by many Jews. Whatever one's sympathies are about the last charge, Herod managed to keep the Romans reasonably satisfied for most of his rule, and the Judaeans, if not satisfied, were at least economically prosperous and free from disaster.

His career illustrates some of the other complexities of Jewish-Roman relations. Herod had to maintain his throne in Judaea, but his own Jewishness did not meet the standards of most of his subjects, largely because he was a Grecophile. He spent a vast amount of energy and money to persuade the Jews of his Jewishness—one reason he lavished so much money on rebuilding the temple—at the same time that he tried to persuade the Romans that he was also indispensable to them. He wanted, or seems to have wanted, very much to be recognized by the Romans as a major figure in their world. The interesting fact that emerges from reading the biography against the background provided by *The Jews in the Roman World* is that Herod's failure in the long run emerges not as his alone, but as the failure of the Jews and Romans together to solve the problems that erupted when Jewish tradition, Hellenistic civilization, and Roman power confronted each other as they did in Judaea.

The two books are based on careful analysis of primary sources and on the latest scholarship. They are, therefore, eminently useful, and, above all, they are highly literate and a pleasure to read. Grant has maintained the high standard that he has set for himself in his previous books.

JILL N. CLASTER  
New York University

PAUL MACKENDRICK: *Roman France*. New York: St. Martin's Press. 1972. Pp. 275. \$10.95.

This book, amusingly written and filled with much important data, is well worth reading for anyone with a general interest in the history and civilization of the area that is now France. In seven well-illustrated chapters—"Before the

Romans," "Caesar Slept Here," "Augustan Cities, The Fruits of Romanization," "Country Houses and Late Empire Cities," "Shrines and Statues," "Gallic Arts and Crafts," and, finally, "Roman Inspired Architecture in Modern France"—Professor MacKendrick provides us with a glimpse of his ongoing love affair with Rome and all that she influenced. In a not entirely serious vein, one hopes, MacKendrick would have us believe in the existence of a French national character going back to prehistoric times and having as its most noteworthy attributes talent in art, good taste in wine, and better in women. Most readers will be amused, few will be convinced, and the humorless *engagé* may well be *enragé*.

It may well be unfair to criticize *Roman France* from a scholarly point of view since it is manifestly a popular work. Yet all scholars, regardless of the audience for whom they are writing, have a responsibility to keep their conclusions within the limits of the evidence. Unfortunately MacKendrick often permits his imagination or his desire to tell a good story to distort his historical judgment. For example, in discussing a rich grave from near Châtillon-sur-Seine, MacKendrick concludes that its female occupant was a "queen" and that "her great wealth was no doubt derived from levying tolls upon the tin traders" from Cornwall. In another instance, while discussing the vast complex dedicated to Mercury at Puy-de-Dôme, MacKendrick concludes that "the aim of all this grandeur was no doubt to console the Arverni for the prestige they had lost when their leader Vercingetorix was defeated." The artifacts neither lie nor speak, but the archeologist or historian who would speak through them must be prudent above all else.

For teaching purposes *Roman France* can be useful as supplementary reading for survey courses in ancient civilization. Even undergraduates, however, may wonder what to think of Diocletian when they read that Julian the Apostate was "the anti-Christian Emperor" or how someone, whom MacKendrick neglects to name, spoke of Burgundy wines in 312 A.D., when the Burgundians from whom the region takes its name were settled there in 443 A.D.

BERNARD S. BACHRACH  
University of Minnesota,  
Minneapolis



RENÉ POTIER. *Le génie militaire de Vercingétorix et le mythe Alise-Alésia*. [Clermont Ferrand: Éditions Volcans. 1973.] Pp. 367. 65 fr.

The military fame of Vercingetorix rests ultimately upon the new tactics he taught the Gauls, particularly the complex techniques of siegecraft as used above all at Alesia, where the cause of a united Gaul was finally lost to the Roman invader. Caesar's narrative is the starting point for any inquiry, but excavations begun a century ago at Alise-Sainte-Reine have convinced a majority of scholars that here we can base historical judgment upon the actual site of Vercingetorix's last stand.

In a passionately argued analysis Potier categorically rejects this identification. The trouble began, he argues, with the minor ninth-century poet Héric, who confused Alesia of the Mandubii with Alisia of the Lingones, and was then compounded by Napoleon III, who simply gave orders to dig up at Alise the earthworks of the opposing forces. Let one confront the site and excavations at Alise with the "portrait-robot" of Alisia, rigorously established on the basis of *Commentaries* 7. 66 ff., and it will be immediately clear that there exists no correspondence whatsoever—not unless one is to take liberties with the text, geography, history, logic itself. In particular, the famous ditches of Alise show discrepancies in number, proportions, and extent, being rather in the nature of drainage channels. Where then was the Gallic Alesia? Like Heinrich Schliemann, Potier scans the terrain, text in hand, sizing up the various elements of the problem—linguistic, topographical, strategic—before opting for Cornu in the Jura, a location originally proposed by A. Berthier. Everything here fits the requirements of the ancient texts, even down to the folklore of the locality, and to prove his case Potier treats the reader to a re-enactment of the campaign leading up to the siege of Alesia, the last stages of which he lets unfold about Cornu.

The author is at his best in shouldering the burden of disproof. The inconsistencies he isolates show clearly enough that "the second battle of Alesia" has still not been fought to a finish: common sense cautions against blaming Caesar wherever his information is inconveniently incongruous. Yet nagging doubts persist when one reads (p. 156) that Berthier eliminated over three hundred possible sites before

hitting upon his "ideal Alesia." For all the attractions of Cornu, irrefutable confirmation depends in the long run upon the indispensable evidence of archeological exploration. So far no dig has been possible, though Potier has scouted the area noting works still visible to the naked eye, such as remains of walls and what could be ramparts and ditches. Potier-Berthier may yet be proved right. In the meantime the prudent reader will suspend judgment.

DUNCAN FISHWICK  
University of Alberta

### MEDIEVAL

J. M. WALLACE-HADRILL. *Early Germanic Kingship in England and on the Continent*. The Ford Lectures delivered in the University of Oxford in Hilary Term 1970. New York: Oxford University Press. 1971. Pp. viii, 160. Cloth \$9.75, paper \$3.25.

One of the major themes of this book is that the early Germanic kings were able to convert to Christianity because certain pagan activities, especially warfare, overlapped with certain Christian duties like warfare for Christ. The author traces the progression of fifth-century barbarian overlords to ninth-century Christian monarchs with precision and elegance. We are shown how the Roman *Curia* and the Germanic kings came to terms and why the barbarians found it advantageous to be converted. A subtheme within the larger topic is the importance of unction. The author takes a skeptical view of the necessity of anointment, putting himself in a revisionist position vis-à-vis Walter Ullmann. Another subtheme is the question of military skill as an explanation of royal power. As with unction, the author is chary of sweeping claims that kings owed their office to their prowess on the battlefield.

Since the Ford Lectures traditionally represent the distillation of a scholar's career, they are not for the beginner. The first three, at least ("Germanic Kingship and the Romans," "King Æthelberht," and "The Seventh Century"), are at times difficult to follow because of the intricacy of the argument. The references to recent German scholarship are helpful, though two omissions might be noted: apropos of Marculf (p. 49), where it is said that the

*arengae* of his charters amount to a mirror for princes, there is no mention of Heinrich Fichtenau's *Arenga* (1957), and apropos of royal titles (p. 111), no reference is made to Herwig Wolfram's *Intitulatio* (1967), even though Wallace-Hadrill has elsewhere shown himself familiar with these works.

In comparing kingship in England with the situation on the Continent, the author tells us that it is virtually impossible to explore this topic to its ultimate origins in "proto-Germanic history." "We cannot define it in terms of, say, the Germans of the first century B.C.; we do not know what word or words they used to describe their kings; we do not know what those kings were for" (p. 1). Nevertheless, we do have the technique of comparing lexical items in the Germanic languages for the purpose of making hypothetical reconstructions that might illumine the subject, and the author makes scant use of it. In commenting upon Tacitus's statement that the Germans had *reges*, Wallace-Hadrill offers the following: "*Rex* was an ancient and curious word. Its stem was cognate with the Sanskrit, which was also the ancestor of the Celtic word *\*rīgs*, from which in turn all the Germanic languages borrowed it" (p. 2). Actually it is not "curious" that Latin and Sanskrit should share the same root for the word king since both languages are descendants of Proto-Indo-European (PIE). What is unusual is that the word for king based on the PIE nominal stem *rēg-* is found only in Celtic, Italic, and Sanskrit—fringe areas where the societies retained archaic religious bodies—and that the central languages of the Continent did not inherit it (see Emile Benveniste, *Le vocabulaire des institutions indo-européennes* [1969], 2: 9–15). Moreover, to state simply that "all the Germanic languages borrowed it" might be misunderstood without some qualification, since initially, in the case of Gothic, the inherited word *thiudans* is commonly used for king. The discussion of *rex* therefore does not convince the reader that the search for proto-Germanic kingship is really in vain, whereas the treatment of the institution of king in the historical period is most informative.

BOYD H. HILL, JR.  
University of Colorado,  
Boulder

JOSIAH COX RUSSELL. *Medieval Regions and Their Cities*. [Bloomington:] Indiana University Press. 1972. Pp. 286. \$8.95.

Professor Russell is no Johnny-come-lately to the use of quantitative methods in studying the Middle Ages. Since the appearance of his pioneering work, *British Medieval Population* (1948), his name has been associated with the application of demography to history. This latest book, an extension of his demographic interests, attempts to delineate urban regions in the century before the Black Death of 1348. The two chapters on Egypt and India are not strictly relevant to his main thesis.

Any discussion of his thesis—that the region was the "basis for European life" before 1348—must center on the methodology that underlies this conclusion. The concept or theoretical model applied to this study is that the central place or city dominates a region with which it is interrelated and thereby gives definition to the region. Other cities within the region are affected in their size and location by the central city and will tend to be distributed according to a rank-size pattern throughout the region.

One basic objection that could be raised is that a model derived from modern city patterns and verified to some extent by such criteria as circulation of newspapers or distribution of types of industries might not apply to the thirteenth century. In fact, Russell recognizes that the rank-size pattern does not hold for the earlier century: "For the medieval period it is therefore necessary to reduce the numerator [in the formula that expresses the relationship of cities]. Since the city is a two dimensional area, some form of square root reduction of the numerator seems in order and is given below." Second, it is a little disconcerting to find in some of the books cited on methodology that scholars working in the modern period are not in agreement about the model.

If Russell is right about applying the rank-size pattern, then medievalists have been given a powerful new tool that allows them to determine population figures for a city or region by extrapolation from the known figures by use of the theoretical model. The trouble with this procedure is that it sometimes leads to circular reasoning. When Russell found too many large cities too close together along the Rhine to fit

the model, he circumvented the difficulty by grouping some cities that may have functioned as a unit and, thereby, produced a reasonable pattern and region. Because the population of the region is completely unknown, he assumes that the population of the largest city (in this case Cologne) was the normal 1.5 per cent of the region, and he calculates the regional population. Later he derives an "urban index" by determining the ratio of the top ten cities to the population of the whole region and compares the density of population with northern Italy. No warning is given in the text, but on a chart printed 140 pages later the Cologne region is one where the urban index has a footnote warning that "this has little meaning. . . ." Unfortunately, this example of the treatment of figures is not an isolated one, but the saving grace is the author's complete candor which insures that the reader need not be misled by the methods being used. Although the author is clearly right in pointing out the uncertainty of many statements about the Middle Ages that are generally not questioned by scholars, the trouble with numbers and percentages is that they have an aura of precision that can be seriously misleading.

Even though the book will not lessen the skepticism among many medieval historians about the quantitative methods the author uses, he makes a valuable contribution in calling attention to the importance of regionalism in the High Middle Ages. Almost as an afterthought he poses useful questions about why and how regions developed that go beyond the correctness of each and every region he has identified. Medievalists would do well to heed his plea to examine regionalism instead of organizing all their thoughts around the theme of protonationalism. His suggestion that the region provided a barrier to the development of overpowerful monarchies is an interesting one and should not be dismissed lightly just because he overstates his case by exaggerating the democratic character of regionalism. Perhaps the best hope for quantitative methods, once proponents become less enamored with the methods themselves and opponents begin to feel less threatened by the novelty, is that new approaches will serve to break old patterns of thought and raise new questions that all will agree are worth further examination. Russell

in this book has raised some of those questions.

CHARLES R. YOUNG  
Duke University

A. IA. GUREVICH. *Istoriia i saga* [The History and the Saga]. (Akademiia Nauk SSSR, Series "Iz istorii mirovoi kul'tury.") Moscow: Izdatel'stvo "Nauka." 1972. Pp. 196.

A. IA. GUREVICH. *Kategorii srednevekovoi kul'tury* [Categories of Medieval Cultures]. Moscow: Izdatel'stvo "Iskusstvo." 1972. Pp. 317.

Both of these books, written by a well-known Soviet historian who specializes in the Scandinavian Middle Ages and who teaches in Kalinin, deal generally with the same problems: the world view and mentality of the medieval man as well as his attitude toward his immediate and remote environment. The first, *The History and the Saga*, deals with the rather limited area of Norway and Iceland and discusses the people of these countries from the eleventh through the thirteenth centuries primarily on the basis of *Heimskringla*, or the saga of the kings (of Norway), which was a record of Norwegian history, particularly of her royal dynasty from the earliest times to 1177. It is traditionally attributed to Snorri Sturluson (1178-1241), an Icelandic aristocrat and historian who wrote, as well, the *Junior Edda* and who participated in the struggle between the Norwegian royal power and its feudal antagonists. Gurevich covers a wide spectrum of basic notions and attitudes of medieval Scandinavians as they are presented in *Heimskringla*—the notion of time, the function of prophetic dreams and visions, death and posthumous glory, myth and actual history, the ideal of the Konung and his relations to his countrymen, truth and fiction, and paganism among the Christianized population. Among the subjects treated, I became particularly interested in Gurevich's discussion of time in the saga. Indeed, the Scandinavians even of the thirteenth century had not yet adjusted to the Christian or any other calendar or chronological system and therefore did not place events in definite years but merely measured time in very indefinite terms of life- or reign-span. In order to show the sequel or sequence of events, the saga writer would not indicate the date or year of a given event but would recur to such

expressions as "next winter," "during that summer," "thereafter," "then," and so forth.

Exploring such notions of the writer's and of the saga's heroes, Gurevich comes to the conclusion that even in the thirteenth century, Scandinavian man underwent little effect from Christianization, despite the fact that Norway and Iceland were officially converted to Christianity two centuries before, and that he visualized the world and his relations with his surroundings primarily in terms of his original pre-Christian, pagan world view.

The scope of "The Categories of the Medieval Culture," which treats primarily the more thoroughly Christianized world of Western Europe, is wider geographically and intellectually. Gurevich deals in it with a medieval notion of the world, time, law and right, poverty and wealth, the accumulation of profit (God and Mammon), and the slow but steady crystallization of the idea of individuality. He proceeds to categorize most of the notions of medieval Western man into pre-Christian and Christian, and he tries to discover the real impact of Christianization on the people of medieval Western Europe. Both books follow a wise French principle, "populariser sans vulgariser," and are written in a clear, readily understandable style without sacrificing true scholarly methodology in exploring the subject.

SERGE A. ZENKOVSKY  
Vanderbilt University

JOSEPH DAHMUS. *Seven Medieval Queens*. Garden City, N.Y.: Doubleday and Company. 1972. Pp. 333. \$7.95.

*Seven Medieval Queens* is an obvious, almost an inevitable, sequel to the author's *Seven Medieval Kings*. The format is the same, except that the heroes have undergone the appropriate surgery and become heroines. The book consists of "vignettes of seven outstanding women of the middle ages," as it says on the dust jacket: Theodora, Brunhilde, Theophano, Zoë, Eleanor of Aquitaine, Margaret of Denmark, and Margaret of Anjou.

When I reviewed *Seven Medieval Kings* (*AHR*, 73 [1967-68]: 114-15) I suggested that the book would be improved by the inclusion of maps, dates, an explanation of the primary sources, a prologue and/or epilogue, and a

bibliography and critical apparatus. This second work has the faults of the earlier volume and almost none of its virtues. Where the earlier volume had one eye on the scholarly audience and one on the popular, this book is completely a work of popularization. The insights are fewer and more widely separated. The writing is poor: it ranges from the simplistic to the garrulous. The lives of Theodora, Brunhilde, and Zoë are but rehashes of the relevant sections of Procopius, Gregory of Tours, and Michael Psellus, respectively, and since the primary sources are all now available in inexpensive translations, we might fairly ask for something more enterprising. Superb popular writing in a biographical mode has been done by such medievalists as E. S. Duckett, Helen Waddell, and Eileen Power. This book does not contribute to the great tradition.

I also doubt if a group of medieval queens, chosen so haphazardly, is worth our attention. Their stories are too largely the stories of their husbands' reigns. We learn about neither women, people, nor politics. There is a need for more work on medieval queenship. There is an even greater need for more studies of average women and their lives, whether done from charters, wills, dispensations, chronicles, hagiography, bourgeois literature, tax returns, court rolls, cartularies, or any other source that can be squeezed. Professor Dahmus is a solid medievalist with a useful study of Archbishop William Courtenay to his credit. He can offer us deeper and livelier stuff than sad tales of dead queens.

JOEL T. ROSENTHAL  
State University of New York,  
Stony Brook

H. P. R. FINBERG, editor. *The Agrarian History of England and Wales*. Volume 1, part 2, A.D. 43-1042. New York: Cambridge University Press. 1972. Pp. xviii, 566. \$37.50.

*The Agrarian History of England and Wales* was planned in 1956 by an advisory committee of nineteen of the leading scholars in the field with R. H. Tawney, chairman. H. P. R. Finberg, professor of English local history at the University of Leicester, was named general editor. Beginning with the New Stone Age the work will extend, in eight volumes, to the present. Volume

4, which covers 1500 to 1640, Tawney's own period, was published in 1967 (*AHR*, 73 [1967-68]: 805-07)). Volume 1 is to be in two parts, issued separately. Part 1 will deal with the New Stone Age, the Bronze Age, and the pre-Roman Iron Age, and it will also include a history of livestock from the beginning to A.D. 1042. Part 2 is the book before us, and it has three sections: Roman Britain by Shimon Applebaum, professor of classical archeology, University of Tel Aviv; post-Roman Wales, by Granville R. J. Jones, reader in historical geography, University of Leeds; and Anglo-Saxon England to 1042, by Professor Finberg, who also is the editor of the book. Inasmuch as this book is priced at \$37.50 (volume 4 costs \$25.00), it would appear that the eight-volume set will be beyond the reach of most individuals. (Part 2 has already disappeared from the shelves of the Bodleian—"presumed stolen.") Applebaum's essay takes up 265 of the 526 pages in this book; the other half is shared equally by Jones and Finberg. One is puzzled by this allocation of space; the time span, roughly half a millennium, is the same in each case.

Applebaum's evidence is mainly archeological, and one feels certain that he has garnered every grain of it. He makes some reference, by actual count, to 455 sites (see the alphabetical list, pp. 268-77). All of his conclusions are tentative in some degree. The small rectangular fields of the pre-Roman period spread from the lowland to the highland zone. But consolidated land blocks divided into plow strips made their appearance in the Roman period, and some of these were cultivated by *coloni* on capitalist estates. The failure of Roman rule was due to the collapse of the superstructure followed by a slow decay of the villa system. At the grass roots level there was much more agrarian continuity between the Roman and Anglo-Saxon periods than has previously been supposed. Even so, one suspects that when Rome fell the Romano-Britons knew it.

For agrarian conditions in post-Roman Wales, that is, from the fall of Rome to the Norman Conquest, the evidence is not so much archeological as documentary. Something can be gleaned from Gildas and Nennius, and the bardic poetry of the ninth century affords sidelights. The chief source, however, is the Welsh

Law Books. Professor Jones's conclusion is that Wales was "overwhelmingly agrarian" (p. 281), and he points out that forty-two per cent of the country is below the five-hundred-foot level. But the agrarian economy was and has always been of the pastoral variety with the arable in a secondary role. The balance between plowland and pasture differed markedly during the course of the post-Roman period as palynologists have been able to show from a study of pollen stratification in peat bogs. There is an interesting exposition of the laws of cotillage, especially as they applied to bond tenants in plowlands at some distance from their village.

In "Anglo-Saxon England to 1042" Professor Finberg traverses a field where Seeböhm, Vinogradoff, and Maitland trod, but he does not follow in their footsteps. When evidence was lacking they looked backward to the unknown from the known and with the inspiration of genius sketched in missing parts of the picture. But Finberg made "a resolute rejection of hindsight"; he bases his exposition squarely on the sources. It is safe to say that no tiniest bit of documentary detail has escaped him, and he knows, too, what can be learned from archeology and field evidence. All too often, however, he finds the facts "admittedly inadequate," and one wishes he would tell us what he thinks.

If anyone still believes that free peasants with no lord over them but the king once formed the basis of Anglo-Saxon society, as even Sir Frank Stenton thought (*Anglo-Saxon England* [1943], p. 275), he should now give up. Finberg says that "for positive evidence of independent and self-governing rural communities we seek in vain" (p. 448). Lordship was basic from the start. The invaders found in Roman Britain the small rectangular fields with which they were familiar, but they also found some larger fields divided into long strips produced by a heavy plow, a more advanced agrarian procedure. By the tenth century the small field was "a thing of the past" (p. 488). Finberg's general conclusion is that at the Conquest Anglo-Saxon England had a more advanced economy and social structure than Normandy.

W. O. AULT

Boston University

M. M. POSTAN. *Essays on Medieval Agriculture and General Problems of the Medieval Economy*. New York: Cambridge University Press. 1973. Pp. 302. \$14.00.

M. M. POSTAN. *Medieval Trade and Finance*. New York: Cambridge University Press. 1973. Pp. vi, 382. \$15.00.

In no field of English medieval history have greater advances been made since World War II than in that of economic history, particularly in its agrarian and pastoral aspects. Previously the influence of William Stubbs and his writings had long given constitutional history pride of place in teaching, final examinations, and research in British universities. When that soil showed signs of exhaustion, governmental and ecclesiastical administration took up attention, but this proved less intellectually satisfying. Economic history had long been taught and examined, but emphasis had fallen upon the modern centuries from Tudor times onward. When attention was given to the medieval period the favored topics were feudalism and manorial organization as affecting agrarian husbandry, together with guilds, merchants, and urban crafts and industries. Certain broad generalizations were assumed without proof: that the quantity of free labor in England was small as a result of strict feudal control after the Conquest; that monetary and credit transactions were rare, save among merchants; and that population, wealth, and trade grew slowly but with unbroken development over the centuries between the Conquest and the Reformation.

Medieval agrarian historians were also for long mesmerized by Domesday Book. That massive, unique, and enigmatic work seemed to hold the key to every feudal, demographic, and agricultural problem of English economics of the age, and a succession of outstanding scholars—Frederic Seebohm, F. W. Maitland, Paul Vinogradoff, and later H. L. Gray and the Orwins—concentrated their attention on the circle of light it provided and discussed villis, manors, open fields, and villeins. Inevitably students and even scholars came to regard Domesday as if it were a system to which Old English practice led up and from which English agricultural, pastoral, and mercantile methods and activity developed.

In consequence, the study of the vast mass of later sources—legal, cadastral, financial, and

the rest—from which the techniques and changes of agriculture and pastoral farming, together with social and demographic changes, could be extracted was long in coming. It began with the often meritorious work of isolated researchers, such as Miss Elizabeth Levett, Miss F. M. Page, and Miss Nellie Neilson, whose work resembled exploratory borings rather than systematic exploitation, giving no scope for broadly based conclusions. The breakthrough, to use the current jargon, came between the wars from a group at the London School of Economics who were inspired by Eileen Power, the first holder of a chair of economic history to concentrate on the medieval period. Among her pupils and associates were Miss E. Carus Wilson and M. M. Postan, whom Miss Power married a few years before her untimely death. Shortly before their marriage Postan had moved to Peterhouse, Cambridge, and became in 1938 Clapham's successor in the chair of economic history there.

Postan's familiarity from boyhood with semi-medieval conditions in Russia, his command of Russian, in addition to the languages of Western Europe, and his competence in the fields of statistics and modern economic theory, added to what became a very wide acquaintance with documentary sources and his own powers of analysis, made of him a brilliant teacher. The influence of his writings and his personal contacts has been very great all over the English-speaking world and Western Europe, and his former pupils hold chairs all over the British Isles, with outposts in America. His earlier work was concerned with trade and finance, but at Cambridge he turned to agrarian and social history and to demography.

Postan's major contribution to historical scholarship is found in his long articles and essays in learned journals and in cooperative works rather than in books. For this reason, few even in academic circles are familiar with all his expositions that lie behind the conclusions or opinions now widely taken for granted by medievalists. The two volumes under review are therefore extremely welcome and should be precious to the younger as well as to the older generation of those concerned with English medieval economic history. Few scholars would wish or care to republish writings on controversial topics now more than forty years old.

Postan can afford to do so because he provides the essential basis and explanation of what is now the common doctrine.

The first volume, dealing with agricultural and allied subjects, contains several of his seminal expositions. That on the rise of a money economy firmly and finally dispelled the view current among students and teachers that transactions and calculations in current coin (not values only used as terms of account) were confined to fiscal circles before the gradual growth of the wage system a century or so after the Norman Conquest. The allied illusion that all agricultural and pastoral work on the manor was accomplished by customary tenants without any wage-earning labor was implicitly demolished in the brilliant article on the chronology of labor services, which was primarily concerned to show that commutation of services was relatively common in the later twelfth century, when demesnes were let out to farm, to be followed by a reaction in the thirteenth century that followed the prevailing tendency to draw back leased land into demesne in the boom period of high farming. Equally important is the introduction to an edition of the Peterborough *Cartae nativorum*, which demonstrates, in the teeth of legal theory, that villeins could acquire and transfer property by charter, a position that led to the wider conclusion that from the mid-twelfth century onward villeins without change of legal status could thrive by property transactions. They could thus ultimately reach a position of local prosperity and influence that made a rise of social and legal status possible, as one of Postan's pupils, Dr. Edmund King, has recently shown more clearly. English medieval society, though static by present-day standards, was not frozen into strata as rigid as those in some countries of Europe, both then and later. Another article shows that the then conventional view of a spectacular growth of population between 1100 and 1300, halted and reversed by the great plague of 1348-49 and its successors and thereafter gradually repaired in the century and a half that followed, must be considerably revised. The growth, slowing down considerably from the last decades of the thirteenth century, fell catastrophically with the plagues, climbed slowly up until the end of the century, and then suffered another recession till about 1470. An article on heriots and

prices on Winchester manors has an esoteric title but is full of interest, and I have never seen such direct and compelling evidence of the vulnerability of rural life to disasters of climate and disease as the graphs that show the exact correspondence of the almost vertical rise of the curves of high price in corn (that is, scarcity demand) and increased mortality around Winchester and Taunton in the wet and cold summers of 1316-17 and the plague year of 1348-49. Dr. Ian Kershaw, in a recent work, has shown that more than two hundred miles to the north, the canons of Bolton in 1316-17 lost two-thirds of their normal cereal crop, two-thirds of their sheep flock, and half their herd of cattle. Yet another article gives convincing agrarian evidence for the declining population and rising wages from the turn of the fourteenth century onward.

The second volume, with essays on trade and finance, opens with the earliest examples of Postan's expertise in discussions of credit and partnership as features of medieval mercantile and general usage from the twelfth century onward. The bulk of the volume, however, consists in three long essays: that on England and the Hanse from the *Power-Postan Studies in English Trade in the Fifteenth Century* (1933); that on the trade of medieval Europe in the North from volume 2 of the *Cambridge Economic History* (1952); and that on economic relations between Eastern and Western Europe from a cooperative volume of 1970. To these may be added a brilliant piece of condensation, a lecture on the medieval wool trade (1952). All these cover a wide field and absorb without references the results of the latest available research. They are not analytical or critical in form but the writings of an eminent historian surveying his territory, and they should be required reading for all students of economic history. Indeed, they might well provide matter for tutorial essays over a couple of terms.

These two volumes gather their contents from various sources that few scholars would have in completeness on their shelves. To those who have read only a few of Postan's articles, they will show him for what he is, a great historian capable both of extracting conclusions by thorough and careful critical examination of many original documents and also of presenting authoritative surveys of wide tracts of

history. These essays should find a place in every collection of historical works and in the bookcase of every specialist in medieval economic history.

DAVID KNOWLES  
London, England

PHILIPPE ERLANGER. *Margaret of Anjou: Queen of England*. Coral Gables, Fla.: University of Miami Press. [1971.] Pp. 251. \$7.95.

If there was a time when the university press imprint guaranteed the reader that the book at hand was respectable scholarship, that is no longer true, not after the appearance of this translation of Erlanger's biography of Margaret of Anjou (*Marguerite d'Anjou et la guerre des deux roses* [1961]). The ambitious sweep and breezy character of Erlanger's writing should have made any prospective publisher wary, although today's financial exigencies may be forcing even university presses to print anything that promises a profit. Most deplorable is the intellectual defilement suffered by the unsuspecting reader who swallows this pseudo history on the assumption that the university press label constitutes the historical profession's imprimatur. In this instance that danger is doubly real since the book promises to right all the slanders Margaret has suffered at the hands of earlier writers.

Actually Erlanger suggests several slanders of his own. He assigns Margaret more lovers than does even Shakespeare, whereas in all probability she had none. The author has her striking the bloodied face of the slain Richard of York, then on the chance the morbid among his readers would prefer something more savage, Erlanger cites Shakespeare's lines that have her stabbing Richard to death. The remorse that Henry VI felt over the bloody manner in which his grandfather secured the throne was what drove him to prayer and asceticism. Humphrey of Gloucester, although "outraging any woman who took his fancy" (p. 58), was immensely popular with Londoners. Englishmen of the fifteenth century continued to treasure the liberties assured them by the Provisions of Oxford. Still, these English were scarcely civilized. "In England a meal was composed of nothing but highly spiced meats, and it was customary for the guests to get drunk thereafter. . . . To unleash a pack of hounds and hunt

some poor wretch over the countryside was taken for an amusing pastime to which one invited one's neighbours" (p. 84). When Cardinal Beaufort died—he had once planted a would-be assassin under Henry V's bed—"no man who beheld the dying rictus could have doubted that he had gone straight to hell and damnation" (p. 103). Margaret's son Edward did not die on the battlefield at Tewkesbury. He was captured, abused by Edward (IV), and then hacked to pieces by Edward's retainers.

These and other historical monstrosities suggest the plea: if society ever decides it proper to burn books, let this be among the very first to go.

JOSEPH DAHMUS  
Pennsylvania State University

G. W. S. BARROW, editor, with the collaboration of W. W. SCOTT. *The Acts of William I, King of Scots, 1165-1214*. (Regesta Regum Scottorum, volume 2.) Edinburgh: Edinburgh University Press; distrib. by Aldine Publishing Company, Chicago. 1971. Pp. xvi, 549. \$17.50.

Now that printing costs threaten to make the publication of texts *in extenso* prohibitively expensive, it is a peculiar pleasure to come across a substantial and elegant volume of texts at a price that is comparatively modest, thanks to a generous grant from the Carnegie Trust for the Universities of Scotland and financial support from the British Academy.

Five hundred and twenty-seven *acta* are transcribed in full, each preceded by a summary of the contents and followed by an elaborate *aparatus criticus*. A further seventy-three *acta*, which have not survived in full, are calendared from partial transcripts. Over a hundred pages of introduction are devoted to "The Life and Reign of William I," "Scottish Royal Government, 1165-1214," a diplomatic analysis of the *acta*, and a skeleton royal itinerary. There are also four pages of additions and corrections to volume 1. There are separate indexes for persons and places and subjects.

The long reign of King William the Lion saw the consolidation of the Scottish monarchy, and it removed the uncertainties as to the survival of the dynasty, which had plagued the reign of his predecessor and brother, Malcolm the Maiden. "Paternalistic and autocratic, conservative in ecclesiastical affairs," as Professor Barrow writes, "William upheld for nearly half



a century the proposition that there was such a thing as the kingdom of Scotland and that he was in charge of it."

The publication of this second volume of the *Regesta Regum Scottorum* is by any reckoning a notable event in Scottish historiography. The political and ecclesiastical aspects of the reign are well enough known, but much of the fundamental work of consolidating the monarchy lay in the encouragement of royal burghs, the fashioning of a workable system of royal justice, and the establishment on the royal demesne of a close-knit ruling class holding by military tenure. In illuminating these lesser-known aspects, this edition of William's *acta* places our understanding of his achievements on a footing of impeccable scholarship.

It is not only Scottish historians, however, who will find this collection of texts invaluable. Its numerous illustrations of the process of Norman settlement offer many interesting parallels, and equally interesting contrasts, to Norman settlement in England, Wales, and Ireland. On the estates of the king of Scots the displacement of thanes by feudal lords was an inexorable but gradual process. In document 281, for example, we see them side by side. In a mandate to his faithful men of Moray, King William commands that if a serf refuses to pay tithe, the thane in whose jurisdiction he lives, "or his lord if he have a lord," shall distrain upon him to do so.

A minor criticism of this edition is the frequent use of Scottishisms without explanation. *Merks*, *toun*, and *brieve* may be readily intelligible, but considerably more difficulty may be caused, for example, by *teind* (tithe), *neyf* (serf), *stank* (millpond), *poind* (a pledge). The use of Scottish technical terms is, of course, justifiable, but a concession might have been made to non-Scottish readers by reprinting in this volume the glossary that appears in the first volume of the *Regesta*.

A second criticism is the placement of the extensive notes to the introduction at the end of each section. Since they are more than references and frequently carry a major part of the argument, they deserve a place at the foot of the page. It is a blemish on an otherwise immaculate production.

W. L. WARREN

Queen's University of Belfast

KATHLEEN HUGHES. *Early Christian Ireland: Introduction to the Sources*. (The Sources of History: Studies in the Uses of Historical Evidence.) Ithaca: Cornell University Press. 1972. Pp. 320. Cloth \$11.50, paper \$4.95.

If it is agreed that documentary materials are critical for historical study, the student of Irish history from the sixth to the twelfth century is in a weak position. When, in an earlier volume in this series, G. R. Elton reviewed the sources for the history of England, 1200-1640, he allotted seven out of eight chapters to documentary sources. Miss Hughes, by contrast, is forced to offer a different proportion: only five of nine chapters can be called hard-core historical material. Clearly, then, the historian of early Christian Ireland must rely on the data and implications of ancillary sciences.

Historians can learn much from their sister disciplines, and when written sources are scarce or inadequate, they must. Miss Hughes has intelligently and informatively surveyed current scholarship in the fields of archeology, literature, hagiography, and the plastic arts as it bears upon her prime target. She is refreshingly clear and exact as to the nature of the information available and its utility for the professional historian.

Chapters on ecclesiastical learning and historical writing in the eleventh and twelfth centuries are useful surveys that are augmented by Miss Hughes's attempts to relate the nature of intellectual activity to the social conditions of the time. For instance, she offers cogent suggestions as to why the material is written in Irish rather than in Latin when the reverse had been true in an earlier time.

In dealing with secular laws, ecclesiastical legislation, and the annals, Miss Hughes shows what historians have available now and what remains for them to do. Most of the sources, in her opinion, need either to be edited and published or mined anew. The sort of task that awaits is amply illustrated in her chapter on the annals, a chapter twice as long as any other. Step by step she reviews the work of scholars who have established manuscript traditions to show how diverse annals were united to form the tenth-century Chronicle of Ireland. To many it may seem that this chapter is overly detailed, but by way of compensation it may be argued that the exposition of methodological

problems is as valuable as a catalog of sources, especially when that catalog is as sparse as this one.

Irish history in this period is little known, and so this guide to the sources will prove useful. Beyond this, however, the book is a mine of information on early Irish life, traditions, and institutions. Written with clarity and restraint, this volume sustains the quality of the series in which it is numbered.

COBURN V. GRAVES  
Kent State University

SUMNER MCK. CROSBY. *The Apostle Bas-Relief at Saint-Denis*. (Yale Publications in the History of Art, 21.) New Haven: Yale University Press. 1972. Pp. xvi, 116, 86 plates. \$10.00.

In 1947 the author, continuing work begun in 1939 at the church of Saint-Denis outside Paris, uncovered "a modest plaster sarcophagus sealed with a stone cover" (p. 9). The present monograph gives an account of the discovery, a description of the bas-relief that was used, upside down, as a coffin lid, and an analysis of the style and original function of the carving. The grave is dated to the late thirteenth century, the relief on stylistic grounds to the 1140s, the period of Suger's renovation of the church. From the edge carving Crosby interprets his find as part of the altar Suger built over the *confessio* of the saint. Stylistically the carving seems to have more in common with metal than with stone work. Crosby conjectures that the relief was abandoned as the result of the unexpected volume of gold contributed to Suger's project.

In its main lines the argument is neat and attractive, though weak in structure and use of sources. Compare the tentative suggestion that "[various stylistic elements] add supporting evidence to a Lorraine provenance for the artist" (p. 56) with the unequivocal statement on the "artist . . . of Mosan origin" (p. 65). In writing of paleography, by which he means epigraphy, Crosby explains the crude inscriptions by calling the craftsman "[u]nlettered, as were most of his fellow artists" (p. 56). How, then, may one account for the truly calligraphic inscriptions on the contemporary ivory casket from Saint-Denis (fig. 67) and the tympanum from Saint-Bénigne (fig. 82), to which the author refers in another

context? The central difficulty in the vogue word "program"—patron or artisan?—is not recognized. In the representation of twelve apostles it involves the elimination of one from the total of thirteen, counting Matthias and Paul but omitting Judas Iscariot. In the bas-relief the first on the left is unnamed and can be either Thaddaeus or James the Less, not James alone (p. 11). In the ivory mentioned, Paul is again included but Matthias is omitted. Crosby is probably right in saying that except for Peter and Paul in the middle, the apostles are arranged without reference to seniority of call, family relation, or liturgical commemoration. On the other hand, Crosby's invalid reference to the liturgical year (p. 53 n.61) should serve as a caution against self-help instead of expertise. By the same token the unrecognized echo of Gen. 28:17 (p. 80 n.5)—"terribilem esse . . . locum," used as an antiphon for the dedication of a church—would be better rendered as "awesome" rather than "horrible." Similarly the equation of apostles with columns (compare p. 88 n.32) is scriptural rather than exegetical (Gal. 2:9).

Medieval personal names will always be a problem for a copy editor, but Abbot Pierre de Saine Fontaine (p. 61), Jocelinus, bishop of Soissons, and Rupertus of Deutz (both on p. 92 n.61), together with Bernard de Clairvaux (p. 94 n.10), are an unlovely anthology asking for uniform treatment.

J. D. BRADY  
American Numismatic Society

J. N. HILLGARTH. *Ramon Lull and Lullism in Fourteenth-Century France*. (Oxford-Warburg Studies.) New York: Oxford University Press. 1971. Pp. xxvii, 504, 16 plates. \$34.00.

As the author remarks in his preface, it was an afterthought to begin his book with two long chapters on the life and world of Ramón Lull. Otherwise his title could mislead one, for while the opening chapters probably constitute the best-informed and most up-to-date survey of the Catalan mystic and his program, the centerpiece of the study, occupying an equally large section, concerns the Parisian doctor of medicine, Thomas Le Myésier, whose *Electorium magnum*, based as it was on Lull's Art, receives here its first scholarly treatment.

As a biographer Professor Hillgarth is a partisan of his subject: he cannot be faulted in his knowledge and understanding of Lull, but his attempts to change the traditional view of Lull as an idealist and dreamer, a man after his time and out of touch with reality, will probably find hard going. The old view needs some amendment, but not quite as much as he provides us with. As a realist Lull might have taken his degrees at Paris instead of trying to gain audience there late in life after he had been tutored and largely self-educated on the frontiers of Christianity in Majorca.

It was fortunate for Lull that he acquired a stout and well-placed disciple in Thomas Le Myésier, whose work is the "Lullism" of the book's title. Having studied theology at the Sorbonne, he knew the theologians, although his vocation was medicine and he served as personal physician to Mahaut d'Artois for most of her adult life. Believing in Lull but realizing that the obscurity of his language and the complexity of his Art repelled most students, Le Myésier resolved to make Lull easier to understand and more accessible to the university world. He anthologized Lull's works in a series of four compilations whose sizes ranged in descending order, the smaller anthology distilling and drawing from the larger. Of these, the largest, called the *Electorium*, and the third, known as the *Breviculum*, have survived, and Hillgarth gives a thorough analysis of the former. Le Myésier intended through the *Electorium* to convince the Parisian masters of the supreme value of Lull's Art. Superior to Aristotle's logic on which it was based, the Art was a system of education that enabled man to get at the unity of all knowledge and divine truth, and so to convert the infidel by demonstrating the proof of the Christian faith. Unsuccessful in the fourteenth century, Le Myésier's work may have influenced Pier Leoni in the fifteenth. But Lullism, condemned by Jean Gerson and the university, found more important followers in later centuries, and this is the subject of a final section.

Hillgarth's work is a remarkable accomplishment of scholarship. Two hundred pages of appendixes include a folio analysis of the *Electorium*, an edition of Le Myésier's "Introduction to Lull's Art," and a valuable study of the twelve miniatures of the Karlsruhe *Bre-*

*viculum*, which the author believes was presented to Jeanne of Burgundy-Artois, Philip V's queen and the daughter of Mahaut d'Artois. Plates of the miniatures complete the book.

FRANKLIN J. PEGUES  
Ohio State University

SALVATORE FODALE. *Comes et Legatus Siciliae: Sul privilegio di Urbano II e la pretesa Apostolica Legazia dei Normanni di Sicilia*. (Università di Palermo, Istituto di Storia Medioevale. Studi, number 2.) Palermo: U. Manfredi Editore. [1970.] Pp. 148. L. 3,500.

The special relationship between the kings of Sicily and the papacy and their exceptional jurisdiction over the church within their kingdom engendered a controversy that lasted well into the eighteenth century, for an ecclesiastical-diplomatic problem that originally involved the Norman rulers eventually became an issue for the Spanish monarchy. Moreover, misunderstandings about the relevant historical facts have persisted even to the present. The root of the controversy was the question of the authenticity and/or interpretation of the bull, *Quia propter prudentiam tuam*, issued by Urban II in July 1198.

This extraordinarily confused, yet highly significant problem is analyzed in this thoroughly documented monograph by Salvatore Fodale. He explains how the bull was included in a collection known as the *Capibrevi* (ca. 1508) whose compiler, Giovan Luca Barberi, was delegated by Ferdinand the Catholic to document all the relevant royal prerogatives. No source was given, but Fodale believes that the text came from a passage in a fourteenth-century or later edition of Geoffrey Malaterra's chronicle. Barberi interpreted the bull and other documents, including one dated 1477 that Fodale discovered in the Palermo archives, as the concession of an ecclesiastical authority amounting to a permanent legateship to Count Roger and his heirs. Fodale also maintains that the novelty of Barberi's conclusions has not been sufficiently emphasized and that, in general, the Sicilian rulers did not claim legatine powers before the edition of the *Capibrevi*. Moreover, before Caesar Baronius no other authority had cited the bull and no original copy had been found.

Curial opposition to the legatine claims and

challenge to the authenticity of the bull began in earnest after the Council of Trent, notably in the work of Baronius. He was in turn answered by champions of the Spanish monarchy, and the controversy continued until the end of Spanish-Bourbon rule in Sicily in 1860.

At this point an important work by F. J. Sentis, *Die "Monarchia Sicula"* (1869), opened what Fodale calls the "modern historiography of the Legation." Here for the first time a letter of Paschal II (dated 1117), which referred to the concession of his predecessor, was cited. This, according to Fodale, seemed to lay aside any doubt as to the authenticity of Urban II's bull, but a careful examination of its wording does not substantiate Malaterra's statement about the granting of a legation. Rather, it appears that Urban, probably on his own initiative and in order to normalize relations with the Sicilian ruler, agreed not to send a legate without the count's approval and to rely on him to implement any directives coming from Rome. Thus the pope accepted a limitation on papal intervention in Sicily, a limitation, as Fodale notes, that was not exceptional and not unlike that demanded by Norman rulers in England. Moreover, except for an appeal by Roger II that elicited the letter of Paschal II, Urban's bull was not cited, nor was it mentioned by other contemporary chroniclers. In fact it became an issue only in the sixteenth century.

Fodale has given a convincing presentation of his conclusions. But since it scarcely seems likely that a controversy that has lasted from the Middle Ages to the present will die down immediately, these conclusions may well provoke further discussion. In any event, this monograph, based as it is on careful research, should place the so-called Norman legation in its proper perspective. Fodale has made a significant contribution to the understanding not only of Norman-papal diplomacy, but of the relations between the curia and the later Spanish monarchy, as well as the involved historiographical controversy that followed.

MARSHALL W. BALDWIN  
New York University

ROBERT LAWRENCE NICHOLSON. *Joscelyn III and the Fall of the Crusader States, 1134-1199*. Leiden: E. J. Brill. 1973. Pp. viii, 232.

Joscelyn III de Courtenay, seneschal of the Latin kingdom of Jerusalem and titular count of Edessa, was an important and at times powerful figure in the twelfth-century crusading states. Relatively little hard evidence about his career has survived, so that writing a biography of him in a modern sense is really not possible. What Professor Nicholson has essentially tried to do is to bring the scanty evidence together and to write a history of the Latin settlements in the East during Joscelyn's lifetime—roughly from the mid-1130s to the end of the century. This is a natural outgrowth of the author's earlier studies of Tancred and Joscelyn I.

The aim is admirable. The result is not wholly what one might have wished. The book needed the attention of a rigorous editor, but it failed to get it. This is a pity, for the book is blemished by stylistic lapses that could easily have been corrected. More distressing still are the shortcomings in research. Nicholson has combed the older editions of the sources and the older studies of the crusading states with reasonable diligence. He has not taken account of recent work dealing with the period he covers. Thus, for example, he writes at some length on the Battle of Hattin, in which Joscelyn took part. But the author fails to take account of the very important treatments of that battle by C. R. Smail (*Crusading Warfare* [1956]), Joshua Prawer ("La Bataille de Hattin," *Israel Exploration Journal*, 14 [1964]: 160-79), and Peter Herde ("Die Kämpfe bei den Hörnern von Hittin und der Untergang des Kreuzritterheeres (3. und 4. Juli 1187): Eine historisch-topographische Untersuchung," *Römische Quartalschrift*, 61 [1966]: 1-50), which present quite different interpretations of the course of the battle and the leadership of the armies, based in part on fresh examination of the terrain itself. Recent editions of sources also have escaped the author's notice, for example, the new edition of the original version of the *Itinerarium peregrinorum* by Hans Eberhard Mayer (1962). Likewise, Nicholson seems not to have consulted much of the abundant recent literature on the society, economy, and institutions of the Latin states. There is not even a reference to the massive two-volume history of the Latin kingdom by Prawer (*Histoire du royaume latin de Jérusalem* [1969-

70]), to name only the most obvious of the missing references.

JAMES A. BRUNDAGE  
University of Wisconsin—  
Milwaukee

#### MODERN EUROPE

ERNLE BRADFORD. *Christopher Columbus*. (Studio Book.) New York: Viking Press. 1973. Pp. 288. \$16.95.

This book is a popular retelling of the life of Christopher Columbus that throws no new light on the Admiral of the Ocean Sea, but that is partially redeemed by the quality of its illustrations. Sumptuously produced in Italy Ernle Bradford's *Christopher Columbus* is filled with beautiful color reproductions of early paintings and maps and color photographs of some of the West Indian beaches discovered by Columbus, as well as with reproductions of woodcuts from the literature of the period of discoveries. Unfortunately the illustrations, as well as the text, occasionally mislead. Thus the Genoese world map of 1457 is captioned "Paolo Toscanelli's world map, c. 1457" and related to the map sent by the Italian savant to the king of Portugal along with a letter describing the possibility of sailing across the western ocean to Asia. Bradford's caption flies in the face of the prevailing scholarly opinion that denies the attribution to Toscanelli and that particularly rejects the view that this map was the map sent to the king. On the subject of Columbus's navigational ability Bradford shows himself more skeptical than most scholars. Columbus's choice of a northern route home on the first voyage, for example, is attributed to miscalculation rather than to skill. Bradford's criticism of Columbus's administration of the Indies in the early years, while following the prevailing scholarly opinion, shows little sensitivity to the Admiral's still valid complaint that he was being judged as if he had been a governor sent to Sicily "or to a city or town under regular government, where the laws have long been laid down and people are used to observing them." Whatever the scholarly limitations of the text (which is well written throughout) the book delights by its illustrations.

WILCOMB E. WASHBURN  
Smithsonian Institution

J. H. HEXTER. *The Vision of Politics on the Eve of the Reformation: More, Machiavelli, and Seyssel*. New York: Basic Books. 1973. Pp. xii, 243. \$8.50.

Professor J. H. Hexter enjoys on both sides of the Atlantic a high reputation for characteristic and distinguished essays on the thought and practice of early modern society, and from time to time he gathers a group of related essays into a book. The volume under review has as its centerpiece a long study of Thomas More and his *Utopia* set within the framework of Europe in the early sixteenth century. It was originally published as the historical introduction to the Yale edition of *The Complete Works of St. Thomas More*, but it is here somewhat enlarged, since the section on "Utopia and Geneva" (originally excised from it and published elsewhere) is now restored. It also includes two appendixes, one of which suggests the time order in which *Utopia* was written (important in itself but inevitably speculative), while the other deals with the chronology of More's stay in Antwerp. The remaining essays in the book comprise an introduction that considers the basic assumptions of early sixteenth-century political theory; a close analysis of *Il Principe* in its use of the words "*lo stato*," meaning not, Hexter argues, the modern political and territorial entity, but rather "an instrument of exploitation, the mechanism the prince uses to get what he wants"; an examination of the language of *Il Principe* and *Utopia* in search of the "fabric of imperatives," that is, the governing principles of their policies; and finally, a reconsideration of the philosophies of these two works and a discussion of Seyssel's *Monarchie de France*, representative of a conservative approach to the role of kingship in the state. These essays are not of the same high order as the central study, and there is also a certain amount of repetition, but they are stimulating and provocative, and they will encourage reflection and debate.

But the central essay on *Utopia* is masterly in its study of the evolution of More's philosophy. Hexter's dissection of Chambers's version of More, as the great medieval figure standing out against the advancing modernity of the state, leaves little of it standing; nor, he shows, is More the Christian humanist and Erasmian

*tout simple*, though he owes a great deal to both. Rather, More went back to the roots of Christianity and to the Bible, but here Hexter stresses a profound yet subtle difference between More and his like-minded contemporaries. *Utopia* is not one more account of the education of a Christian prince; it is the rejection of a compromise between Christianity and the unequal and unjust society of early modern Europe. In that sense it was, and remains, a revolutionary work.

It is impossible to do justice to Hexter's patient, lively and refreshing account that raises More above both the hagiographers who overlaud him and the disparagers who see him as a threat to the emerging sovereign state. I continue to hope, as I did when I first read it, that Hexter's study of *Utopia* will also appear as a paperback. Then indeed it will reach, as it deserves to, the wide audience of students of history, literature, and political theory of the age.

JOEL HURSTFIELD  
University College London

FRIEDRICH HEER. *Europe, Mother of Revolutions*. Translated from the German by CHARLES KESSLER and JENNETTA ADCOCK. New York: Praeger Publishers. 1972. Pp. 368. \$12.50.

Compared with Heer's other writings, especially *The Intellectual History of Europe* and *The Medieval World*, this book, which covers major European intellectual developments from the French Revolution to World War II, is disappointing. Science, technology, and their impact on European thought are given short shrift, while theological issues take up fully a third of the book. Freud, Weber, the existentialists, and a host of others who revolutionized modern European thought are also passed over. Except for one chapter on Russia, the book deals almost exclusively with Western European developments: romanticism, Marxism, the German national movement, the Saint-Simonians, and late nineteenth-century utopian thought. And what Heer has to say about them, although often suggestive, is largely unoriginal. As for the title, who ever doubted that modern Europe fostered and spread revolution? The more important question, which this book raises but does not confront, is

why, in the face of fierce and sustained resistance, the forces of revolution won out in Europe, and whether this development was a unique phenomenon or a process possible anywhere in the world under the right circumstances, with or without Europeanization. Heer is sparkling on premodern Europe, less so on the modern period.

Still, there is much to commend this book. The longest and best chapter by far, "Problems of Catholicism," does on a small scale for modern European Catholic thought what H. S. Hughes did for social theory and Marcuse for radical thought. Heer's treatment of the politicization of French Catholicism, Catholic attitudes toward the Dreyfus affair, modernism and reformism within the Church, Russian attitudes toward Rome, and many leading recent Catholic thinkers succeeds in structuring a surging, complex world of thought, compared with which much of the rest of the book seems fragmented and aimless. The last chapter, "Russia in Europe," a searching examination of ambivalent Russian attitudes toward the West from Belinsky and Gogol to Trotsky and Stalin, is also outstanding. Both chapters cover ground on which Heer is at his best: religious thought and controversy with their intellectual and existential ramifications.

The book as a whole cannot claim to be an original contribution to our thinking about recent European thought and culture and their current significance. But it is the work of a distinguished intellectual historian and prominent figure in contemporary Austrian cultural life, who almost always has something worthwhile to say.

ROBERT ANCHOR  
University of Southern California

HILDA I. LEE. *Malta 1813-1914: A Study in Constitutional and Strategic Development*. Valletta: Progress Press. N.d. Pp. 292.

The French expedition to Egypt in 1798 served to alert Britain to the importance of the Mediterranean for her imperial purposes. Even though in 1801 Pitt asserted that "the Mediterranean is but a secondary consideration" he had by 1803 come to the conclusion that "our possession of Malta was essential." Gibraltar, Malta, the Ionian Protectorate (even though

relinquished in 1863), and Cyprus (after the opening of the Suez Canal) are the stages through which the Mediterranean came to be the lifeline of the British Empire during the nineteenth century when Britain was the chief naval power in that sea. The position of Malta was central in all senses of the word.

Until the last decade of the period France was the obvious threat, especially in possible conjunction with Russia. But the decline of French power, replaced by the rise of German might, eventually placed the two chief imperial powers in the same defensive position. This found expression first in the 1904 Entente Cordiale, then in the 1912 naval arrangements.

Malta was not a Crown colony, and considerations of defense had first priority in the rule of the island. But the people of Malta are sufficiently numerous to make the problem of their governance different from that of Gibraltar. Thus the issue arose of the degree of control that could be granted them in the management of their own affairs. Given also the overwhelmingly Catholic allegiance of the Maltese people and their common language (Italian among the educated), religion and language were often the concrete issues around which Maltese politics centered.

Constitutional arrangements, the make up and powers of the Council of Government, and the numbers and powers of its elected members, were the subjects of unending discussion between the British government and those elements in Malta desirous of extending the range of Maltese control. The detailed discussion of these problems, in the context of overriding strategic requirements, is the substance of this book. Despite the annoying typography the task is done in thorough fashion, furnishing in the small an interesting and useful case study of the operation of that impressive creation, the British Empire.

RENÉ ALBRECHT-CARRIÉ  
Columbia University

IRBY C. NICHOLS, JR. *The European Pentarchy and the Congress of Verona, 1822*. The Hague: Martinus Nijhoff. 1971. Pp. xiii, 363. 49.50 gls.

This study is more than just a factual account of the Congress of Verona; it is a diplomatic historian's dream come true—the unrestricted

freedom to elaborate one's treatment of the subject. The author in this case was permitted to have two background chapters, two more on the Vienna preliminaries, six chapters on the Verona deliberations, four chapters on historiography and interpretations, a concluding chapter, and, in eleven of these chapters, subsections devoted to still more retrospections. No subject could have been more thoroughly narrated and evaluated. With convincing substantiation the author tends to justify the attitudes and policies of Wellington, Villèle, and Metternich, but to criticize those of Alexander I, Montmorency, and Chateaubriand. His conclusions seem sound when he asserts (p. 135) that France was not commissioned by the Alliance to attack Spain, that by the withdrawal of Britain the Alliance was broken (p. 324), and that out of the ruins of the congress system arose the concert system of conferences (p. 325). In addition to the main question regarding Spain the book deals at length with those of the Near East, Italy, Latin America, and the slave trade.

On one point, however, the author seems to be in error. He asserts (p. 59) that "recent historians have ignored completely the Vienna conference," but he overlooks Bertier de Sauvigny's study of a year earlier, *Metternich et la France*, which devotes twenty pages to it as compared to his own twenty-two. Also Schroeder's study of 1959, *Metternich's Diplomacy at its Zenith*, which the author does utilize, devotes five pages to the so-called "ignored" conference. He criticizes a few other historians (p. 59) for calling Vienna a "preliminary" conference, but nothing in this book gives the impression that it was more than preliminary.

As to sources it is disappointing that, while dealing with Europe's "Pentarchy," the author used only the British and French archives. Although he did consult a considerable amount of Russian printed documents he did not seem to have used any Prussian printed or archival sources and neglected entirely the rich Austrian archives, except by indirect use through Schroeder. These omissions and the failure to consult the accessible Piedmontese and Spanish archives or Becker's *Relaciones exteriores de España* makes this study less than definitive. Yet, at the same time, it must be conceded that this work deserves to be recognized as the most

exhaustive study of the Congress of Verona and probably the last one that will be attempted.

LYNN M. CASE  
University of Pennsylvania

ULRICH KRÖLL. *Die internationale Buren-Agitation, 1899-1902: Haltung der Öffentlichkeit und Agitation zugunsten der Buren in Deutschland, Frankreich und den Niederlanden während des Burenkrieges.* (Dialog: Schriftenreihe für Publizistik- und Kommunikationswissenschaft, 7.) Münster: Verlag Regensburg. 1973. Pp. 478. DM 48.

The South African War, 1899-1902, marked a watershed in the history of British imperialism. The war started as a colonial conflict fought by British professional soldiers against an enemy thought to be hardly superior to warlike Indian mountaineers. It expanded into a South African civil war that pitted Briton against Boer, and sometimes even Boer against Boer. It culminated in a huge logistic enterprise involving the dispatch from Britain and her Dominions of nearly 400,000 men to fight on the other side of the globe, a feat then unprecedented in the annals of war. The Boer War finally turned into a classical guerrilla struggle that engendered bitter political dissensions in Britain herself. British wartime chauvinism soon evaporated. Wartime disillusionment seeped into anti-imperialist critiques such as Hobson's *Imperialism: A Study*. This influential book was in part a product of the author's South African experience and was praised by Lenin.

The Boer War also aroused widespread agitation on the Continent, where popular sympathies were almost entirely with the Boers. The author's study, originally a doctoral dissertation, provides a detailed account of the various pro-Boer movements in Germany, France, and the Netherlands. His is a thorough, well-documented monograph, based on a wealth of published and unpublished sources in South Africa, France, Holland, and East and West Germany. Kröll stresses the extraordinary political diversity that distinguished the pro-Afrikaner camp. Boer sympathizers included pacifists anxious to put an end to bloodshed on the veld; pan-Germans who gloried in the military might of the fatherland; humanitarians

hopeful of establishing a new international rule of law; and French chauvinists determined to avenge Fashoda. For a short time the Boers became the most popular people in Europe, lionized in the same fashion that Polish and Hungarian insurrectionaries had been lionized during the earlier part of the nineteenth century. But the pro-Boers were usually quite ignorant of South Africa's wider social and racial problems. In this respect they resembled Polonophiles and Magyarophiles of an earlier period, men and women unacquainted with the realities of Eastern Europe, where the insurgent nationalism admired abroad often represented no more than the interests of locally dominant socioethnic minorities.

While the pro-Boers differed vastly among themselves they had one feature in common: a marked proclivity to project their own domestic preoccupations onto an African screen. Admirers of the German folk soul spoke of the Transvaalers as if the Boers somehow formed part of a greater German people, an assumption unrelated to the facts of history. Socialists accused the British of shedding blood for the sake of gold; yet the war had nothing to do with the proprietary rights in the South African mining industry. Romantics enamored with the mystique of blood and soil created for themselves an idealized image of virtuous Boer patriarchs, untainted by the love of gain, but these were mythical figures who existed nowhere except in newspaper columns and novelettes. Moreover the pro-Boer sentiments were, in the long run, apt to run counter to the particular national interests of their foreign spokesmen. Once the war was over international agitation died down and the romance went out of Afrikanerdom. In the final analysis the pro-Boer campaign in Europe was an essay in futility.

L. H. GANN  
Stanford University

FRANCIS ROUTH. *Early English Organ Music from the Middle Ages to 1837.* New York: Barnes and Noble. 1973. Pp. x, 305. \$17.50.

As an impressive work of reference and a perceptive commentary by an experienced performer on a great corpus of music, this book places in its debt students of British culture.



Mr. Routh, an organist and musical editor, has sought both to locate the manuscript sources and to provide a critical listing of editions for the hundreds of organ compositions from around 1400 down through the death of Samuel Wesley in 1837. Routh, as well as having compiled a catalog, compels our admiration for his delineating the historical position and illuminating the artistry of many a composer, as for example the case of the transitional eighteenth-century figure John Keeble.

Routh upon occasion also succeeds in tracing a series of interactions between music and general history that result in the art's developing a resiliency enabling it to survive more directly cataclysmic general events. The best example is his analyzing, first, the development after the Reformation of the "voluntary" as nonliturgical music, based on free inspiration, not plain chant, for performance in places of worship and, second, the rise, also after the Reformation, of domestic music making, which of necessity put a premium on keyboard skill, both upon the virginal and the chamber organ (the latter of which was recognized by the mid-seventeenth century as the instrument better serving as a foil in concerted works to the viols so prominent under the early Stuarts). The conjunction of these two lines of development, Routh argues, produced a musical school of such strength that Puritan pressure for removal of organs from places of worship could not eradicate it.

In Routh's opinion such resiliency is simply a manifestation at one point in time of the strength of "an unfolding, immensely variable, yet continuous, tradition" of English organ music, which forms the central thread of his book. Now it is about Routh's articulation of this "tradition" that I must entertain grave reservations. It seemingly embraces both the most brilliant and the most indifferently gifted of composers. The reader is at a loss to discern any consistent line of reasoning either as to the sources of the tradition's strength or the causes for its demise—especially so for the latter, as the tradition is said to have been both consummated and terminated in the career of Samuel Wesley. How odd that Wesley should have failed to propagate the "fresh techniques and styles of organ composition" with which Routh credits him! Did the fault lie in the

deterioration of the traditional matrices of musical education? Routh is concerned about the absence in the eighteenth century of the best organ composers from the Chapel Royal. Or could the fault have lain in the social character of music by Wesley's time, or in the activities of early nineteenth-century musical organizations? Routh suggests no answers along these lines, and his misunderstanding of the nature of the entertainments at the Argyll Rooms and of the foundation of the Philharmonic Society indicates the work that remains to be done.

DAVID WARREN HADLEY  
Wake Forest University

STEPHEN J. GREENBLATT. *Sir Walter Raleigh: The Renaissance Man and His Roles*. (Yale Studies in English, 183.) New Haven: Yale University Press. 1973. Pp. xii, 209. \$7.95.

A prime merit of this book is that it is short. Its thesis glares: there is a tense, tight relationship between art and life, imagination and action in Raleigh. Since the words "public" and "private" did not have the same meaning in Raleigh's day that they have since acquired, Raleigh's private verses were in effect public virtues. Raleigh was above all an actor playing parts for an audience, in particular the queen. If he studied most to please himself, to play the role of the man he wished himself to be, his failure lay not in lack of skill, but, ironically, in his inability to separate imagination and illusion from calculation and reality. Historians have noted Raleigh's dramatic flair and have stressed the imagery of the stage in the *History of the World*. There is little, except perhaps the exaggerations, that can be called original in this book.

Greenblatt has subtitled his essay "The Renaissance Man and His Roles." Presumably this is meant to underscore the fact that Raleigh played many parts. So did others. The book is not a biography, although it discusses Raleigh's life and work. It is not intellectual history, although it deals with Raleigh's ideas and attitudes. And it is not old-fashioned literary criticism, although there are some tedious examples of *explication de texte*. What is it? An uncharitable answer might be that it is a neatly packaged product of the Eng-Lit industry. The author is fond of departmental clichés—ambiguity, ambivalence, dramatic sense

of life, role playing, synthesis of the objective and subjective will, tragedy of imagination, and so on. These terms are like staples, fastening live emotions and thoughts to professorial file cards. When Greenblatt discards the trappings of his trade he writes clearly and makes telling arguments. He is especially good at challenging Pierre LeFranc's brilliant but sometimes perverse criticism; Greenblatt argues convincingly that Raleigh's best known poem, "The Lie," was indeed by Raleigh.

An illustration must suffice to show what is lacking. Greenblatt argues that "the inner tensions and conflicting world views that we have examined in his poetry and his career reappear tied to his shifting interpretation of history." To a degree this is true, but only to a degree. There are obvious inconsistencies in the *History of the World*, but inconsistency is not necessarily evidence for any of Empson's Seven Types of Ambiguity. Raleigh's apotheosis of death at the end of the work is not, summarily, just a "bitter assertion of human emptiness." Raleigh's words are not so far from tradition as this thesis makes them appear. Raleigh wrote that "it is therefore Death alone that can suddenly make a man to *know himself*" (the italics are mine), and among other things, death "humbles" men and makes them "repent." By selectively ignoring various traditions and customs (especially medieval ones) upon which Raleigh drew, Greenblatt creates a literary model, not a living human being, much less a tragic figure set in his time and place.

To sum up in a sentence is to exaggerate, but historians should at least be cautioned: the author interprets Raleigh's life by his writings, not his writings by his life. Men of action are seldom as consistent as men of words would have them. Traditions themselves are apt to be ambiguous because they are shaped less by reason and logic than by custom and emotion. Raleigh was, like many practical men, at once a visionary progressive and a profound traditionalist.

F. SMITH FUSSNER  
Spray, Oregon

BRIAN P. LEVACK. *The Civil Lawyers in England, 1603-1641: A Political Study*. New York: Oxford University Press, 1973. Pp. viii, 311. \$16.00.

A study of those men trained in civil law during the Tudor-Stuart period is long overdue. An examination of the attitudes and activities of this professional group is essential for an understanding of the circumstances that led to the conflict between king and parliament in seventeenth-century England. Brian P. Levack has attempted to remedy this deficiency by providing a study of "all those civilians who received their doctorates before 1641 and who resided in England between 1603 and 1641." Included among the two hundred civil lawyers who form the basis of this work are those who were incorporated at English universities after having been trained on the Continent. Attempting to explain the royalism of the civilians prior to the English Civil War, Levack has approached them as "officials in the King's governments, lawyers in financial difficulties, political theorists, critics of the common law, and defenders of the English Church." A biographical dictionary, consisting of thumbnail sketches of the civilians' careers and comprising almost one-third of the text, concludes the work.

Levack persuasively assumes that the royalist tendencies of the civilians must not be attributed to any one factor. Instead, he suggests that an interaction of ideas, economic necessity, and professional considerations must be taken into account. He argues that during this period civilians differed from common lawyers less regarding political fundamentals, such as royal authority, sovereignty, and resistance, than they did in matters that pertained to their professional advancement and success. The civil lawyers, Levack contends, did not have the luxury of independent incomes. When confronted with diminishing opportunities for employment in those courts that utilized civil law procedure, they necessarily identified their interests with the court and, more importantly, with the Anglican Church. Thus they became some of the staunchest supporters of the Stuart monarchy and the Laudian church.

However, questions may be raised about Levack's assumptions with respect to the composition of the civil law profession during this period and with regard to the thoroughness of his selection of civilians. The repeated warnings issued to recipients of only the baccalaureate degree in civil law, which attempted to

prohibit them from performing the functions of civilians, raise doubts as to whether Levack's selection indeed represents the political ideas and career objectives of the profession. Moreover, contrary to the author's assertion about the completeness of his study, a careful scrutiny of the matriculation registers apparently utilized by him reveals twenty-three recipients of doctorates in civil law who are not included in the work.

CECILE ZINBERG  
California State University,  
Fullerton

ROBERT W. MALCOLMSON. *Popular Recreations in English Society, 1700-1850*. New York: Cambridge University Press. 1973. Pp. x, 188. \$11.95.

Brian Harrison has written that "nineteenth century historians usually study social class in relation to work rather than leisure." Fortunately, there have been indications in the last few years that popular games and recreations, for too long the province of antiquarians, are finally being accorded the attention they deserve from serious social historians.

This short monograph consists of two parts. The first is a description of traditional, pre-industrial popular recreations, including bull-baiting, several early forms of football, wakes, fairs, and local holidays. Professor Malcolmson goes beyond mere description, however, and places these amusements into the social context of rural society. He suggests the ways that these recreations filled the social needs of laboring people, providing them with opportunities for self-expression, outlets for aggression, and even, on some occasions, camouflage for social and political protest.

These recreations, he notes, were deeply rooted in the natural rhythms of the agricultural year and could survive, despite some opposition, because they received the support of the gentry, many of whom "were not entirely disengaged from the culture of the common people" and accepted traditional recreations both as part of the natural order of things and as useful social tranquilizers.

The second part of the book describes the undermining of popular recreation in the late eighteenth and early nineteenth centuries by

such "modernizing" forces as enclosure, urbanization, evangelicalism, and the growing need for rigorous labor discipline. The picture presented by Professor Malcolmson here is less clear, though he is probably not to blame. Each of the modernizing forces he describes contributed to the elimination of many popular recreations, but the precise role of each may be beyond sorting out. There were regional differences, but the sources really do not allow for more than a somewhat impressionistic account of a general national trend. It may be, as Professor Malcolmson's account suggests, that the greatest factor behind the elimination of traditional rural recreations was the elimination of the countryside as the home of the majority of the population.

This reservation aside, Professor Malcolmson has made a welcome contribution to our understanding of the life—and death—of traditional society.

DAVID C. ITZKOWITZ  
Macalester College

GEOFFREY BENNETT. *Nelson the Commander*. New York: Charles Scribner's Sons. 1972. Pp. xii, 322. \$12.00.

Nelson is a figure of perennial fascination. His victories, unorthodox private life, and melodramatic death created a legend that has enshrined him in popular memory and attracted a succession of writers, from Robert Southey on, who have built up a very formidable library of Nelsoniana. As a result there is little that we do not know about Nelson, his captains, his ships, or his mistresses, and the merit of new books about him must rest upon their literary quality or the freshness of the synthesis they provide, the archives having little left to yield. In recent years we have had a full-scale biography by Carola Oman and a shorter, more incisive "portrait" by Oliver Warner, not to mention individual studies of Nelson's battles by Warner, Dudley Pope, and David Howarth. In the background there stand the serried volumes of documentation published by Sir Harris Nicolas and the Navy Records Society, as well as Mahan's classic study of Nelson as a commander. Geoffrey Bennett does not really add anything new to all this, either in detail or interpretation. The book is certainly well

written and the publishers have been lavish with illustrations, diagrams, and maps. Bennett, who has several good books on British naval history to his credit, has not forgotten, as many writers do, that naval warfare under sail is a very arcane subject, and he has provided an excellent, concise account of the organization, administration, strategy, and tactics of the Royal Navy during the years when Nelson was winning his great victories. The description and analysis of the campaigns and battles is clear and convincing. Nor are the warts left out. The rashness that produced the fiasco at Turks Island in 1783 was repeated at Tenerife (1797) and Boulogne (1801). The humanity that held floggings to a minimum in the early stages of his career was not so noticeable at the end, when H.M.S. *Victory*'s log recorded an "appreciable number." The less pleasant side of Nelson's ambition, his vanity, is not glossed over, nor are the unattractive aspects of his relationship with Lady Hamilton. It is nice to see Lord Keith (commander-in-chief in the Mediterranean, 1799-1801) given full credit for a forbearance that many senior officers would not have shown even to a Nelson. Yet there is nothing in any of this that will be new to serious students of British naval history. This book is an excellent introduction to Nelson, rather than a must for scholarly readers.

RAYMOND CALLAHAN  
University of Delaware

T. J. RAYBOULD. *The Economic Emergence of the Black Country: A Study of the Dudley Estate*. Newton Abbot: David and Charles. 1973. Pp. 272. £5.25.

Landowners for centuries in that area of Staffordshire and Worcestershire that came to be known as the Black Country, the Dudleys found that, when the pace of economic growth began to quicken in England in the later eighteenth century, they were sitting on the metaphorical gold mine. Starting in 1774 when the second Lord Dudley and Ward came into the estate, Mr. Raybould considers particular elements of the exploitation of their estates in turn. By enclosing their lands the Dudleys were able to exploit the mineral resources

underground to become the leading mine owners in the Black Country. Clay was used for brickmaking, and limestone, coal, and iron provided the raw materials for the iron works they established. The Dudleys also had a share in the improvement of roads, in the construction of canals, and in the building of both narrow-gauge local and standard-gauge main line railways necessary to permit the economic development of an area that had poor natural communications. While their principal efforts were devoted to industry, they also tried initially to farm their estates more efficiently. But Mr. Raybould argues that for the years 1774-1833, the period he considers in most detail, "the administration of the Dudley estates was not adapted to meet changing conditions, traditional practices remained and management was generally inefficient." Although the estates continued to be profitable in the later nineteenth century when the economic tide in the Black Country had already begun to turn, the prosperity did not continue, and, like many other aristocratic landowners, the earls of Dudley suffered from the legislation of the early twentieth century and the bleaker economic conditions after 1920. Further affected by the nationalization of coal and iron after the Second World War, the estates of the Dudleys in the Black Country had shrunk to but a few acres as the family turned from landowning to a new role, predominantly financial and *rentier*. Thus Mr. Raybould's account extends beyond the economic emergence of the Black Country into its decline, and it virtually ends with the death of the late earl in 1949.

More limited than the main title suggests, this book does not, on the other hand, provide an effective picture of the operation of the Dudley estate. The bits of the jigsaw are never fitted together. Although he has had access to the Dudley papers, Mr. Raybould was apparently not able to come across much in the way of direct evidence to show on what basis the Dudleys conducted their estates. In particular, the claim that the third earl of Dudley was probably the most successful aristocratic entrepreneur to survive into the twentieth century is largely unsubstantiated, while the earl himself, like his predecessors, whose relationships would have been illumi-

nated by a genealogical table, remains a shadowy figure.

WALTER E. MINCHINTON  
*University of Exeter*

ERNEST S. DODGE. *The Polar Rosses: John and James Clark Ross and Their Explorations.* (Great Travellers.) New York: Barnes and Noble. 1973. Pp. 260. \$9.25.

In the nineteenth century, polar exploration was the challenge that lunar exploration is currently. Two of the outstanding leaders of that period were Captain Sir John Ross and his nephew James, who later became a rear admiral. Dr. Dodge, director of the Peabody Museum in Salem, Massachusetts, has written the first book-length biography of the two men.

Captain John commanded the Admiralty's Arctic expedition of 1818 that restored Baffin Bay to the maps. James sailed as midshipman. On their return John ran afoul of crusty old John Barrow, second secretary of the Admiralty, who saw to it that he never received another naval Arctic command. James, however, sailed on two expeditions under Parry. The two Rosses were together again on a privately financed expedition in 1825-33, on which James reached the North Magnetic Pole. Sir James's most notable voyage was a magnetic survey in Antarctic waters in 1839-43, during which he discovered the Ross Sea, Ross Ice Barrier, McMurdo Sound, and Victoria Land. Both Rosses participated in searches for the lost Franklin expedition.

Dodge penetratingly observes that John was an eighteenth-century man in his outlook, controversies, loyalty, and diversity of interests. James, he concludes, was a sailor's sailor and a nineteenth-century man "in his reticent severity, his cool efficiency in his commands, his quiet competence in his scientific pursuits."

We can be grateful that Dodge undertook to cram the essence of two active lives into this small volume. The book will introduce the general reader to a series of most interesting expeditions. Historians will find it useful in giving the Rosses due credit for their substantial accomplishments. They will regret that the limitations of the Great Travellers series

have precluded footnotes or an adequate listing of manuscripts.

JOHN E. CASWELL  
*California State College,  
Stanislaus*

ROBERT E. ZEGGER. *John Cam Hobhouse: A Political Life, 1819-1852.* Columbia: University of Missouri Press. 1973. Pp. vi, 312. \$11.00.

The intractable problem in writing historical biography is always that of the decision as to how much history should be included for the assistance of the reader. Perhaps in an effort to meet this difficulty Robert E. Zegger has subtitled his biography of John Cam Hobhouse *A Political Life, 1819-1852*. This definition enables Zegger to concentrate on the development of Hobhouse's political career. Nevertheless, the dilemma remains. If the experience of Hobhouse as parliamentary candidate and member of Parliament for Westminster is recounted, how much of the history of that unusual parliamentary seat should be provided? Zegger, in his introduction to this study, devotes over fifteen pages to the radical fortunes of Westminster from the time of Wilkes. Similarly, the author finds the treatment of Hobhouse and India tricky. This political life is rather an account of various themes in Hobhouse's life, his radical career, his activities on behalf of nationalist groups abroad, his work for the Select Vestries Bill of 1831, and his periods in office. The emphasis on each theme varies.

Despite certain problems of balance, this book makes a useful contribution to the history of early nineteenth-century radicalism. It adds a number of illuminating details, in particular to the passing of the 1832 Reform Bill. Zegger emphasizes from the beginning the basic difficulty for the historian interested in Hobhouse's career, the apparent inconsistency of his political opinions, his "trimming" to the political wind. In chronicling these shifts of opinion, he does not, however, succeed in explaining Hobhouse's swing from extreme radicalism to something like middle-of-the-road Whiggery. Zegger pinpoints the crucial role played by the campaign for parliamentary reform of 1831-32 in persuading Hobhouse of the undesirability of rousing violent mob action. The coincidence of the offer of ministerial of-

fice to him is not really assessed. Hobhouse may have been "bought off" to encourage his silence or have been recruited simply to placate the radicals.

In the absence of a biography of Joseph Hume, Zegger's account of Hume's activities in the raising of funds for Greek and Spanish rebels is particularly welcome. The dust jacket commentator expects that this book will be found "intriguing" by "Byron scholars and other scholars of nineteenth-century English letters and literature," but I fear they will be disappointed. Hobhouse's relationship with Byron is not explored extensively. According to the *Dictionary of National Biography*, Hobhouse is thought to have invented the phrase "His Majesty's Opposition." Zegger quotes (p. 104) Hobhouse's use in 1826 of the phrase in the Commons and then remarks that "Canning's and Tierney's use of the phrase confirmed the acceptance of opposition within the constitutional framework of nineteenth-century Britain." A footnote referring to the work of A. S. Foord is the reader's only evidence for such a brave assertion. The omission of date and place of publication for many of the works cited in the footnotes, as for instance on page 5, is very irritating but may have been controlled by the publisher. The full references are provided in the bibliography, but, even so, a university press should surely conform to accepted convention.

VALERIE CROMWELL  
University of Sussex

THOMAS J. SPINNER, JR. *George Joachim Goschen: The Transformation of a Victorian Liberal*. (Conference on British Studies Biographical Series.) New York: Cambridge University Press. 1973. Pp. xii, 263. \$14.50.

Since Lord Randolph Churchill forgot Goschen he has seemed eminently forgettable. The two-volume biography by A. R. D. Elliot, which appeared in 1911, set forth with amplitude the steps by which the reforming Liberal of 1866 became a conservative Liberal in 1880, a Liberal Unionist in 1886, and, as a Liberal Unionist, a member of Salisbury's governments in 1887 and again in 1895. On the face of it there was no crying need for another biography, even after the passage of sixty-two years.

The author of this new life of Goschen

certainly has been assiduous in exploring the available papers of contemporaries, although it is not clear whether Goschen's own account of the mission to Constantinople (1880-81), which Elliot "somewhat abridged," was available to him. Nor has Spinner's reliance upon W. N. Medlicott's studies in the diplomatic history of the period produced a picture of Goschen the diplomat that is significantly different from that supplied by Elliot.

As a defender of laissez faire, Goschen perceived the Irish Land Act of 1881 as a "gigantic invasion" by government, but he appears oblivious of the long record of government intervention in the Irish economy. His own understanding of laissez faire permitted government intervention to penalize slum landlords, but not to subsidize public housing. While he understood very well the drift of Fabian thought, he ignored the changing conditions of business organization: the growth of corporate activity and the decline of rugged individualism.

Goschen's consistency as a Liberal Unionist and antisocialist comes across more clearly at some points in this new work than in Elliot. For example, Goschen's opposition to Chamberlain's tariff reform rested not only on the traditional free-trade conviction of many Liberal Unionists but also on his dread of protection as a step toward state socialism (p. 229). But there is nothing novel about the theme of the aging Liberal who cooperates with Conservatives. The work will probably be most useful to libraries lacking a copy of Elliot.

BARRY MCGILL  
Oberlin College

W. J. GARDNER *et al.* *A History of the University of Canterbury, 1873-1973*. Christchurch: University of Canterbury; distrib. by Whitcombe and Tombs, Christchurch. 1973. Pp. 530. \$9.75.

Among the colonies of England in the nineteenth century, New Zealand was the most fortunate in finding patrons and advisers in the mother country. Each of the provinces had connections through its settlers with organizations or families who took a deep interest especially in its churches and schools to offer advice and (less often) material help. This was particularly true of the two provinces in the

southern island. Otago was founded by the Free Kirk of Scotland, and Canterbury, the last offspring of Edward Gibbon Wakefield, had the Church of England (in theory at least) as its creator and the Archbishop of Canterbury as the titular head of the Canterbury Association. What was even more important, the first leaders had the blessing and promises of help from Oxford and Cambridge. Because of natural advantages in the production of wool, these two provinces were securely established without the growing pains suffered by the northern provinces. The short-lived gold rush in the sixties benefited Otago in particular and made Dunedin the most beautiful city architecturally of New Zealand. The steadfast determination of the Scots everywhere in the world to forward education made the miracle of a university in 1869 possible. But a group of men in Canterbury began planning for a college or university in the sixties and were able to admit some students by 1874.

The man in Canterbury who did most for what seemed at times a lost cause was H. J. Tancred, a Rugby graduate who had not gone on to Oxford or Cambridge and did not therefore strive too hard to imitate in every detail the old English universities. Tancred was a politician rather than a scholar and was able to foil in the New Zealand Parliament the efforts of Otago to make itself the arbiter of university education in New Zealand. Tancred worked for a University of New Zealand that should have the right of setting examinations and granting degrees and of which Tancred himself was chosen the first chancellor. The University of New Zealand had no buildings and no permanent home, but the stage was set for the development of university colleges in all the provinces that would have the same standards of examination.

It was at this point that the English universities made their greatest contribution. London recommended one of its own faculty to teach science and engineering and Oxford sent out J. Middleton Brown, who had a brilliant record in classics and who fitted into the community in Christchurch to the satisfaction of everyone. Cambridge contributed C. H. H. Cook, a mathematician and scientist.

This volume deals with almost every aspect of the growth and influence of Canterbury Col-

lege (to become a university in its own right in 1951) and stresses such matters of interest as the admission of more women than any other college even at its opening, the erection of dormitories attracting students from distant provinces, and the high quality of some of the students graduating. The most famous is Ernest Rutherford (later Lord Rutherford), the leading nuclear physicist in England for whom Canterbury showed more appreciation than Cambridge before his outstanding reputation was established. In reminiscing in later years, he said that C. H. H. Cook had given him greater inspiration than any teacher he had had. It would seem that the relaxed and broad curriculum of his New Zealand alma mater was more conducive to original thought and ambitious research than the more rigid requirements of Cambridge, which paid little attention to his genius until his reputation was established in the world.

HELEN TAFT MANNING  
Bryn Mawr College

KENNETH O. MORGAN, editor. *Lloyd George Family Letters, 1885-1936*. New York: Oxford University Press. 1973. Pp. x, 227. \$14.50.

Students of Lloyd George's career know only too well how rare are confidential letters from "the Goat." Lloyd George must have written letters to his colleagues, but few of importance have survived. This may have been because Lloyd George was essentially an oral communicator who preferred to deal directly with politicians over the breakfast table, rather than indirectly through letters. Up to now, the lack of such letters has forced historians to rely on the memoirs and diaries of those around him. These sources are variable in quality, and quite apart from being hearsay evidence, many have put unduly critical constructions on Lloyd George's motives. Now, for the first time, we have in this collection of letters to his wife firsthand evidence about Lloyd George's views throughout his career.

The most noticeable thing about the letters is that they seem to be vacuous and selfish. There is almost no mention of the arts or of intellectual, social, or economic currents. Religion appears, but only as an adjunct to political maneuvers; and politics comes down to

almost exclusively party matters, jostling for position, and electioneering. *Family Letters* thus provides ammunition to unfriendly critics of Lloyd George, and one might even say, as was said of Douglas Haig and others, that with the publication of this book Lloyd George has committed suicide many years after his death. This would be misleading, since Lloyd George was writing to his wife, and despite their deep quarrels over his sexual lapses they still shared many common views; Lloyd George did not have to state in so many words that he was defending nonconformity and trying to lift up the poor. It is only when the letters deal with new subjects that a false note sets in. There was no clear understanding between husband and wife over the outbreak of the First World War, and the phony sentimentality of Lloyd George's comments often sound as if he is trying to convince Dame Margaret when he is only partly convinced himself.

*Family Letters* deals mainly with Lloyd George's early career; it is especially illuminating on the Welsh background. On the other hand there is comparatively little material on the major crises involving Lloyd George between 1914 and 1931; but Dr. Morgan fills many of the gaps with excellent introductions to each section.

MICHAEL KINNEAR  
University of Manitoba

ROSS TERRILL. *R. H. Tawney and His Times: Socialism as Fellowship*. Cambridge, Mass.: Harvard University Press. 1973. Pp. x, 373. \$15.00.

R. H. Tawney, the democratic socialist, is hard to pin down. A tough-minded saint, he was revered in England by most factions on the Left who agreed on little else, and his career was multifaceted: political thinker, academic, historian, gadfly to the Labour party, and author. The most influential and best-known of his books were *The Acquisitive Society* (1920), *Religion and the Rise of Capitalism* (1928) and *Equality* (1931). In them, and elsewhere, he continued in the line of Carlyle, Ruskin, and Morris, believing as they did in the moral necessity for a changed society but in a more evenhanded way. He was less emotional and less abstract than his Victorian predecessors—

or most English socialists—recognizing and respecting the actuality of the ordinary Englishman, whom he called "Henry Dubb," neither romanticizing his nature nor abstracting his plight from the gritty realities of everyday existence. As a socialist he got beyond the limitations of guild socialism, at the same time as he recognized the dangers of too much power accruing to the state and the soul-limiting qualities of modern collectivism. On the whole, critics of society tend to be pragmatists or visionaries; Tawney was remarkable in being something of both, a practical thinker with a vision of what British society should be. Unfortunately he did not ever really enunciate, if such could be done, the exact program to achieve his goals.

Ross Terrill, in his study of this truly noble man, "blends history and biography with a study of his [Tawney's] ideas." The blend, however, is achieved only through an adding up of the quite separate and distinctive parts of the study; a more historical, integrated treatment of life, times, and ideas might conceivably have proved more rewarding. As it is, the first sections consist of an extended biographical sketch—Tawney's Indian civil service family background; his education at Rugby and Balliol; experiences at Toynbee Hall; teaching in the North of England in the very earliest days of the Workers' Education Association; his marriage to William Beveridge's sister; his service as an enlisted man in the First World War, where he was severely wounded at the Somme. After biography comes analysis, and in his next section Terrill presents a clear, schematic, non-chronological discussion of the ideas that went into the making of Tawney's socialism, placing him in the tradition of political thought. Historical-minded readers may be disappointed that little attention is paid to Tawney's work in Tudor and Stuart history, but, as Terrill states candidly, he is not himself an expert in that field, and in any case, he makes very clear in what ways Tawney's conception of the earlier period was important for his thought. Terrill's own deep knowledge of China serves him admirably when he comes to Tawney's writings on China and provides him also with some intriguing comparisons with Mao. The last of the book is an assessment of Tawney's importance, followed by a bibliography listing 572



items of his published writings. Taken all together the result is a valuable study of Tawney's thought, enriched by a picture of the man, much of it based on interviews with those who knew him well.

PETER STANSKY  
Stanford University

PETER DENNIS. *Decision by Default: Peacetime Conscription and British Defence, 1919-39*. Durham, N.C.: Duke University Press. 1972. Pp. xi, 243. \$9.75.

By 1932 the attitude of Germany and Japan forced the British government to look more carefully at its arrangements for national and imperial defense. There were bitter disputes about the best way to achieve national security. *Decision by Default* makes significant contributions to the literature of this complicated subject. The book deals with much more than "peacetime conscription and British defence." Based upon massive collections in the Public Record Office and other unpublished sources, it makes clear the dilemmas of British defense policy in the period between the wars.

When the need for rearmament made itself felt two schools of thought emerged among the planners. Upon the one hand were those who urged a policy of "limited liability." They knew Britain's resources were not boundless and had to be exploited as efficiently as possible. They wanted the government to build up the navy and air force, allotting a minor role to the army. This, they felt, would avoid serious injury to the nation's financial position, its "fourth arm of defence." Their opponents disagreed with this strategy. They held that the only way to render the country safe was to create an expeditionary force that could play a significant part in a European war and that could serve also as the basis for further expansion of the army. They advocated a "continental commitment" as the key to Britain's security, arguing that the British Isles could only be defended on the Continent.

It was Neville Chamberlain who played a curious and significant role in deciding the issue. He dominated the opinions of his colleagues, allowing his policy of appeasement to transform the strategic situation in Europe. After Munich the French saw the collapse of an

effective eastern front against Germany, and when thirty-five Czech divisions were lost to their cause it was demanded that Britain restore the balance of forces. There was fear that France might come to a separate arrangement with Germany rather than fight without the support of a large British army. It thus came about that Chamberlain, the champion of "limited liability," became the first prime minister in history to introduce compulsory military service in peacetime. It was a gesture to satisfy opinion in France and in Britain. The analysis of these terrific events in *Decision by Default* is acute and the narrative excellent.

ALFRED GOLLIN  
University of California,  
Santa Barbara

CLIFFORD GULVIN. *The Tweedmakers: A History of the Scottish Fancy Woollen Industry, 1600-1914*. (David & Charles Library of Textile History.) New York: Barnes and Noble. 1973. Pp. 240. \$12.50.

Surprisingly few very good regional histories of the British textile industry have been published since the 1930s. Though not so massive as theirs, this monograph will stand with Herbert Heaton's on Yorkshire or Miss Julia Mann's on the West of England.

Using the methodology of traditional economic history Dr. Gulvin provides a lucid and balanced analysis of the emergence and decline of Scotland's mainland tweed manufacture, a small industry that in 1907 (just past its prime) accounted for seventeen per cent of the value of British woollen piece goods production. He delineates a geographically remote, factory-based industry reinvigorated by a fashion fluctuation, carried forward by mid-Victorian expansive conditions, and then caught out by foreign competition, product overspecialization, and organizational weaknesses.

The cloth types created in the 1830s fashion revolution have received extensive scrutiny elsewhere. But to many other aspects of tweedmaking Gulvin's painstaking research gives new definition. For example, the quantitative data for plotting growth are carefully reconstructed. And a chapter on the means of sustaining expansion fully exposes the industry's structure and operation in sections dealing with technology, labor, wool, marketing, and finance.

By the 1890s internal flaws aggravated the difficulties arising from foreign protectionism and rivalry. Gulvin provides a sound account of the latter and clarifies the internal problems, mostly stemming from the commitment to an exclusive market: overpricing, minuscule production runs, neglect of technical education, and the intensely competitive and fragmented industrial structure that put manufacturers at the mercy of cloth merchants. Pursuit of higher standards and new products brought some renewal before 1914.

A final chapter shows that the wages of Scottish woolen workers (except for designers and finishers who did better in tweedmaking) remained on a par with those of their English counterparts. Paternalism and the small scale of production relegated unionism to an ephemeral phenomenon.

Rather short shrift is given to the industry's decline after 1914. To appreciate its zenith we surely need more than a couple of paragraphs on its nadir.

Wary of "technique-based history" (p. 11) the author could nevertheless have strengthened the technological side of his study, for example, by closer investigations of fancy yarns or dyeing, some very critical technical elements. And contrary to pages 102 and 154, ring spinning was never commercially possible for woolen yarns in the nineteenth century. Despite these reservations, the volume is unquestionably the best treatment of its subject.

DAVID J. JEREMY  
Westcliff-on-Sea, Essex,  
England

JOHN SELBY. *Over the Sea to Skye: The Forty-Five*. New York: St. Martin's Press. 1973. Pp. 170. \$8.95.

In this volume John Selby, who has written extensively about military events and personalities of the nineteenth and twentieth centuries, turns his attention to the last full-scale campaign in Great Britain, a subject about which previous commentators have seldom been neutral. This book is evidently not designed for scholars. Printed on slick paper it is beautifully and lavishly illustrated with portraits, battle paintings, maps, and plans. The occasional footnotes are explanatory rather than precise references

to sources, and the bibliography, while helpful, is selective rather than exhaustive. It seems likely that this account is aimed at those of the public who desire to find out "what happened" and are attracted by the feel and look of the book.

Such a purchaser will have many reasons to feel satisfied. One of the virtues of this volume is its nonpartisanship. The actions of both sides are recounted without emotion, excuse, or advocacy. A certain amount of imagination is evident in the decision to start the story with the duke of Cumberland, describing his background and his role in the Battle of Fontenoy, rather than with his more romanticized cousin, Prince Charles Edward, but the book soon settles into a narrative with its focus on the prince and his activities following his embarkation from France. Cumberland reappears only briefly as a threat to the Jacobite army at Derby and, more extensively, as the commander of the victorious forces at Culloden. Selby's emphasis is almost exclusively on the military aspects of the adventure: plans, recruitment, marches, sieges, battles and, finally, escape from Scotland. The author gives a clear and chronological narrative of such events, but he provides no new details or interpretations of them, nor does he deal at length with any other aspects of the affair. No speculations are advanced about such questions as the probable fate of the Jacobite army if it had proceeded toward London (as the prince wished), the responsibility for repeated failures of organization and supply among the rebels, the relative capacities of the officers on either side, or whether there was ever any chance that Charles Edward could achieve his stated aims. Such lack of interpretation is especially serious in a work designed for nonspecialists, since it leaves them without any substantial context for the story and does not suggest to them any of the most interesting questions that can be raised about it.

J. WILSON FERGUSON  
Russell Sage College

A. J. YOUNGSON. *After the Forty-Five: The Economic Impact on the Scottish Highlands*. Edinburgh: Edinburgh University Press; distrib. by Aldine Publishing Company, Chicago. 1973. Pp. xi, 246. \$15.00.

Professor Youngson has written a learned and

thoroughly documented book that covers much of the same ground as Malcolm Gray's *The Highland Economy 1750-1850*, but from a rather different point of view. Gray's groundbreaking and very useful book provided the first modern, detailed account of the economic development of the area after Bonnie Prince Charlie's rebellion; Youngson deals with the period from the perspective of the proposals put forward for the Highlands by the economic planners, notably James Anderson. The theorists believed in the great economic potential of the Highlands; they also subscribed to the view of Sir William Temple who, on the basis of his experience in the Netherlands, believed that a "great multitude of people crowded into small compass of land" is the cause of prosperity. So emigration from the Highlands, which began in a small way in the 1760s and 1770s, and worried contemporaries, was to be checked by the creation of industries, markets, and improved transportation. Youngson then recounts the attempts to develop such businesses as linen and fisheries and to create fishing villages (of which Ullapool was one) and to collect the tax money due the government from distilling, an industry whose growth required no encouragement. There was a brief, delusive prosperity in some parts of the west Highlands from kelp, which ended by the 1820s on account of the end of the war and the reduction in duties on competing sources of alkali. And there was the Caledonian Canal that was supposed to bring about all sorts of economic development, but that instead was "one of these conspicuous white elephants conceived by ambitious and ingenious engineers and enthusiastically brought to birth by misguided politicians" (p. 152). None of these schemes for commercial and industrial development had anything more than very partial success. The result was inevitable: emigration on a scale so large as to amount to depopulation. There were more people in the Highlands in the 1830s and 1840s than there ever have been since. The emigration is usually ascribed to the coming of large-scale sheep raising and the uprooting of tenants by ruthless, tight-fisted landlords, but, as Youngson points out, the clearances "were heralded . . . by fifty years of remorseless and extensive change" (p. 185). By the 1840s "the idea of building up the highland econ-

omy was over," not to be revived until our own time (p. 190). This is an excellent book. The author's style is felicitous and his analysis convincing. The illustrations are well chosen, but do not include a modern large-scale map, which would have been helpful.

MAURICE LEE, JR.  
Rutgers University,  
New Brunswick

T. W. MOODY and J. G. SIMMS, editors. *The Bishopric of Derry and the Irish Society of London, 1602-1705*. Volume 1, 1602-70. (Coimisiún Láimhscríbhinní na hÉireann.) Dublin: Stationery Office for the Irish Manuscripts Commission. 1968. Pp. 430. £6 6s.

The documents in this volume, selected from materials in the Public Record Office of Northern Ireland and the Guildhall Library in London, were compiled for a 1945-48 court case concerning the fishing rights of the Irish Society, the corporation initially founded for the colonization of Londonderry. The dispute originated from rival claims of the Society and the bishop of Derry over fisheries attached to tithes and *erenagh* lands, that is, former monastic and other Church holdings. The complexities of these ancient rights illustrate the tenacity of feudal arrangements. There is much on the temporalities of the see of Derry and on the bishops' relations with their lessees (often landed families) and other tenants as well as with the Society. Among the bishops involved was George Downham, whose survey of the diocese was published by the *Ulster Journal of Archeology* in 1894-95. Charles I, by means of a Star Chamber case, took over the Society's property and granted the disputed lands to Bishop John Bramhall in 1636. Over half of the volume deals with the period 1602-40. In addition to what has been indicated, this section contains information on the building of Londonderry and on rents and farming conditions as well as on schools (or their lack) and the state of the local clergy and churches.

After a few interesting documents on the outbreak of the Irish rebellion in 1641 there is a gap until 1657, when Lord Protector Cromwell, employing the royal "We," issued a new charter (given in full) to Londonderry. This recognized the Society's rights but of course excluded the bishop's. Much of the last quarter of the

volume is concerned with the restoration of the bishop's and other ecclesiastical property. Bramhall's successor, George Wilde, was a Londoner whose efforts to seek support from the London guilds composing the Society met with some success. Among other documents on the diocese after 1660 are two lists of incumbents for 1661 and 1669. The first gives the value of the livings and the names of the "Gentlemen" in each parish; the second lists the patron of each benefice.

The table of contents includes the documents to be published in the second volume (covering 1670-1705). When that appears it will undoubtedly contain an index to both volumes and, one hopes, a glossary of such terms as townland and quarters, both land measurements, that are peculiar to Ireland. The editors seem to have done a careful job; they provide a good, brief introduction but no explanatory footnotes. T. W. Moody is author of the key book, *The Londonderry Plantation* (Belfast, 1939), and J. G. Simms is an outstanding authority on Ulster in the late seventeenth century.

FRANCIS G. JAMES  
Tulane University

DESMOND SEWARD. *Prince of the Renaissance: The Golden Life of François I.* New York: Macmillan Publishing Company. 1973. Pp. 264. \$14.95.

This is a handsome biography. With its numerous illustrations and boldly printed text, it has a coffee-table-book look that is enhanced by an absence of the visual clutter of academic footnotes. That the work should have a format so pleasing to the eye is appropriate, for Mr. Seward is at his best as a historian of appearances. Fashions and castles, ceremonies and processions, diplomatic encounters and the battles of love and war are described in considerable detail, with the chronology of Francis's life serving as a compositional framework for evidence that has been drawn from published journals, memoirs, ambassadors' reports, and chronicles, supplemented by occasional purple passages from the works of writers such as Michelet, Strachey, and Belloc. The result is an entertaining narrative of the major events of the reign, accompanied by word portraits of

Francis and other illustrious persons of the principal courts of Western Europe. It is unfortunate that many of the portraits are reduced to caricatures, such as the one-liner on Juana of Castile, who was "so mad that people said she ran up curtains like a cat."

The central argument of the book, stated with typical exuberance, is that "no ruler since Charlemagne has had a more direct influence upon the civilization of France" than Francis I, whose greatness "lies in his role as supreme patron of the later Renaissance." A great patron Francis certainly was, and the importance of his patronage in the cultural history of Renaissance France has long been recognized. However, Seward's descriptive listings of the *objets d'art* acquired for the king's collections, the building projects undertaken at royal residences, and the literary works produced during the reign do not clarify the extent to which Francis personally understood or was committed to the innovations of mannerists and humanists in his service.

Even if one agrees that royal patronage under Francis I helped to bring about major cultural changes, the notion that the true significance of the reign is found in the spectacle of the king "riding, dancing, or promenading in an unending pageant, escorted by glittering nobles, by famous poets, painters, and savants," is difficult to accept. The reign was indeed one of the most brilliant in the history of France, but an abundance of published sources and secondary sources not used by Seward clearly indicate that the period was also one of fundamental social, economic, and institutional change. Until a history of the reign appears that incorporates the results of recent scholarship in these areas, it is not likely that the study written by Terrasse a quarter century ago will be superseded.

LLEWAIN SCOTT VAN DOREN  
Boston College

RENÉE KOGLER. *Pierre Charron.* (Histoire des idées et critique littéraire, volume 127.) Geneva: Librairie Droz. 1972. Pp. 182.

Pierre Charron was a moral philosopher whose chief work, *De la sagesse* (1601), was an attempt to teach men how to master their passions and achieve a skeptical inner wisdom in the after-

math of the agonizing choices of the French religious wars. Borrowing widely from Montaigne, Du Vair, Lipsius, and Bodin, as well as from Seneca and the stoic philosophers, he produced an awkward, eclectic treatise that was popular in the next generation because he had combined a number of topical intellectual currents into a didactic, systematic argument. Charron, a Catholic priest, divorced his secular moral system from all religious considerations; expressed skepticism about knowledge and rational thought; endorsed the absolute authority of the prince; and offered an antirationalist defense of faith. Despite the ambiguities and contradictions throughout the book its elements were absorbed by a paradoxical diversity of groups: the free thinkers picked up Charron's implied separation of morality from religion; religious reformers, especially Jansenists, liked his skepticism toward the possibility of rationally knowing God. Charron anticipated Pascal's wager and some of the elements of Descartes's systematic doubt. He was admired by Gassendi, Naudé, and La Mothe Le Vayer; Saint Cyran defended his book; Bayle and Rousseau were influenced by it.

Yet despite all this Charron remains a secondary figure, a mere transmitter of ideas. Kogel has written an apology of her subject, designed to rescue him from the charge that he was just a disciple (or even plagiarizer) of Montaigne. On this point she is convincing: Charron borrowed from a variety of sources and his synthesis was his own. But her detailed study, the most thorough in English, tells us little of importance that was not adequately summarized in the works of Sabrié, Adam, Popkin, Church, Busson, and Rice. It tediously analyzes each aspect of Charron's thought in chapters fragmented by far too many quotations in French. Readers will not be convinced that Charron was original enough to merit another whole monograph, or that *Sagesse*, however popular, deserves to be removed very far from the shadow of Bodin and Montaigne; in fact the evidence of its *separate* influence is thin. Thus even in a work devoted to them Charron's ambiguities seem pale when set against the backdrop of other thinkers who were struggling with the implications of secularism. Perhaps by concentrating more on this broader influence

Kogel might have rescued Charron's reputation more effectively.

WILLIAM H. BEIK  
Northern Illinois University

FRANCES ACOMB. *Mallet Du Pan (1749-1800): A Career in Political Journalism*. Durham, N.C.: Duke University Press. 1973. Pp. xii, 304. \$11.75.

The Swiss-born Jacques Mallet Du Pan customarily shares a historical footnote with fellow survivors of the Monarchien group that broke with the patriot majority in the summer of 1789; or else, as a counterrevolutionary publicist and intelligence agent, he is associated with Gentz and Burke. By way of contrast Professor Acomb reaches back two decades prior to 1789 in uncovering Mallet's evolution as a journalist, historian of recent events, and political theorist. She shows that, rather than representing the climax of Mallet's intellectual and professional life, the 1790s served as the denouement. Mallet Du Pan was an antirevolutionary before the Revolution. Formed in the volatile political and social climate of Geneva of the 1760s and nourished upon the fashionable Voltairism of the *Négatifs*, Mallet broke with the philosophes and factional politics in what Acomb calls an assertion of personal autonomy. Hers is a legitimate position to take. Mallet is a puzzling figure, a paradoxical one. Empiricist, humanitarian, and reformer, he desired religious toleration, opposed much corporate privilege, and instinctively supported the underdog. At the same time, however, he could not stomach what he considered the moral hedonism of Voltaire, d'Alembert, and Condorcet, and in the end his antidemocratic bias caused him to turn upon Rousseau. Perhaps the most important influence upon his life and work was an even more lonely figure, that brilliant, erratic bundle of contradictions, Simon Linguet.

Because he was among the first to make a lucrative career in journalism, especially as political editor for Panckoucke's *Mercur de France*, Mallet appears to have little in common with the miserable hacks, pamphleteers, and pornographers who belonged to the counterculture of letters in France during the 1780s. Like them, however, Mallet held the old monarchy in contempt while chafing at insti-

tutionalized restrictions upon liberty of expression. Acomb's study would have profited from exploring Mallet's Paris career in wider perspective. We learn precious little about journalism in the capital. Had she gone more deeply into press-government relations in the early 1780s, particularly with regard to imported books and periodicals, Acomb might not have underestimated the protectionist reasons lying behind Vergennes's hostility toward Mallet's first journals, the Geneva-based ones. She appears uncertain as to the various categories of permissions awarded by the French government to journalists and authors. And from what we know in other instances, her generally positive view of the press lord Panckoucke ought to be nuanced. Nevertheless, this study of Mallet's career is commendable, not the least for its skillful use of archival sources in Geneva and Neuchâtel.

RAYMOND BIRN  
University of Oregon

COLIN LUCAS. *The Structure of the Terror: The Example of Javogues and the Loire*. (Oxford Historical Monographs.) New York: Oxford University Press. 1973. Pp. xv, 411. \$29.00.

Claude Javogues arrived at Saint-Etienne in mid-October 1793 fresh from the republican victory over besieged Lyon, thirty-two miles to the northeast. Like dozens of other representatives sent by the National Convention on missions, he was authorized to command any actions he deemed necessary for the salvation of the beleaguered and strife-torn Republic. He remained in the Loire *département* until early November and returned there in mid-December. By mid-February 1794 he had aroused vociferous local opposition, especially in Roanne, and alienated his colleagues within the Convention, even Collot d'Herbois. He was recalled to Paris under threat of arrest.

The political history of the Loire *département* during Javogues's mission is an illustrative episode in the development of the revolutionary government of Year II. It is fully analyzed in this excellent book, which is based on mastery of diverse and voluminous documentary evidence and an admirable familiarity with the local geography, economy, and social system; the period; and the political ac-

tors. Every chapter is dense with information and illuminated by suggestive insights. It is a demanding book; the more you already know about the topic, the greater your interest will probably be.

Lucas's view is that even the "anarchic" phase of the Terror was, in this *département*, a period of order compared with 1792-93 or 1794-95. His central chapters, two-fifths of the work, show the coherence of the governmental system by surveying the local political institutions: clubs, surveillance committees, the *armée révolutionnaire*, the commissioners moving about on revolutionary business, the ordinary administrative bodies, and the revolutionary tribunal at Feurs. This survey is preceded and followed by chapters on Javogues: first a fine account of his antecedents and political career; then an analysis of his role as representative on a mission, not only as the intermediary between the national government and the province, but also as the main source of energy for revolutionary change in the province. The latter chapter is followed by a long discussion of the terrorists, the local men on whom Javogues had to depend. Lucas produces much evidence for his conclusion that "the Terror was characterized by the transfer of power at the local level towards the lower end of the ruling class of the previous years" (p. 323). Militancy did not originate in conflict between classes, Lucas says; rather, it was the product of a series of political choices, each leading or predisposing to the next. The interpretation is concordant with the idea that revolutionary change was a vast and complex process of social learning.

Lucas's views are convincing. His (like any) perspective has its own limitations. The system of political institutions was not the only structure involved. Lucas has much to say about oppositions between town and countryside, between plains and mountains, between social classes, and between contrasting attitudes or beliefs, but he does tend to treat these as independent variables and to invoke them here and there as explanatory factors. His knowledge of local differences between districts (Saint-Etienne, Montbrison, Boën, Roanne), and his quantitative information, might have been used more methodically, though this would have required sacrificing some of the

valuable concentration on political institutions and personalities.

PHILIP DAWSON  
Brooklyn College,  
City University of New York

PAUL GONNET. *Un grand préfet de la Côte-d'Or sous Louis-Philippe: La correspondance d'Achille Chaper (1831-1840)*. (Analecta Burgundica.) Dijon: Société des Analecta Burgundica. 1970. Pp. 328.

Achille Chaper (1795-1874) was a product of the École Polytechnique, but resigned his commission in 1816 and moved into metallurgy in his native Isère. From 1820-28 he was also mayor of Pinsot, but with the coming of the July Monarchy he began a notable career as a departmental prefect, serving briefly in the Tarn-et-Garonne, then in the Gard, arriving late in 1831 in the Côte-d'Or through the intervention of Casimir Périer to whom he was distantly related by marriage. Chaper would subsequently hold two more posts between 1840 and 1848 and be elected deputy from Côte-d'Or in 1849. His public career ended with the coup d'état of 1851.

These letters reflect his prefectural career in the Côte-d'Or. Chaper left copies of only those letters he considered important. Paul Gonnet has selected 239 letters from the 3,000 that now rest in the Archives départementales de la Côte-d'Or, the documents having originally been part of a larger collection (see R. Avezou, *Inventaire des documents de la collection Chaper du château d'Eybens* [1953]) assembled by Chaper's son Eugène, himself a well-known Orleanist and a member of the parliamentary commission that investigated the Government of National Defense after 1871. The letters are arranged chronologically by topic. Political matters are separated from social and economic issues, and a detailed table of contents offers easy access to such topics as municipal elections; subversive activities; the surveillance of republicans, legitimists, and Bonapartists; relations with the notables; the diocese of Dijon; and the economic crisis of 1832.

In sum this is a well-edited source book, supplied with notes, bibliography, and index, for the specialist on the July Monarchy or the

student of French local government in the nineteenth century.

ROGER L. WILLIAMS  
University of Wyoming

JEAN-ANDRÉ TOURNERIE. *Le Ministère du Travail (Origines et premiers développements)*. (Temps de l'histoire.) Paris: Éditions Cujas. [1971.] Pp. 447. 80 fr.

This book is first the history of an idea; then, the history of an institution embodying a modest part of that idea. Tournier's dissertation traces the idea of a ministry of labor from its appearance in 1848 until Clemenceau's decree establishing the Ministry of Labor and Social Insurance as part of his first cabinet, in 1906, and follows its functioning until 1914.

Tournier has had to rely chiefly on printed sources, especially parliamentary documents and debates and the press, since he turned up few of the manuscript sources he had hoped to find in the ministry's archives. The brevity of the period during which he observes the ministry in action is not compensated for by intimacy of the picture. There is little here, for example, on the role of one of the most remarkable of the *grands commis* of the Republic, Arthur Fontaine, a man appreciated both by employers and by reformist labor leaders, a friend of leading politicians and of great musicians and writers and painters. His work at the Ministry of Labor, said Daniel Halévy, "weighed more on the constitution of French society than did that of vague figures like Sarrien whose names encumber the chronicle of history." It still remains true, as Halévy suggested (*Decadence de la liberté* [1931]), that one needs to know the details of the work of a man like Fontaine to "know how a law was elaborated, drafted, and voted under the Third Republic." The parliamentary sources and the press, valuable as they are, cannot substitute for missing administrative archives and personal correspondence.

Professor Marcel David's introduction praises his student's work, but then deplores its "neutrality of tone" and its failure to realize Professor David's hope that it would "penetrate to the heart of the labor movement." What other tone would have been more appropriate to a study that begins with some of the warm-

est but least realistic proposals for workers' emancipation (the proposed "Ministry of Progress" of 1848) and goes on to describe a government bureau, later a department, navigating between the hopes of workers and social reformers and the fears of employers and reactionary politicians like Méline (whose fears of the ministry's action were at least as unrealistic as the generous hopes of 1848)? The attitudes of labor leaders and of workers of the syndicalist "heroic period" toward various forms of government intervention were complex and often contradictory; this study does not attempt to get at that aspect of "the heart of the labor movement." Of course the ministry of those years did not live up to the hopes of its friends any more than it did to the fears of its enemies. Tournier shows its most energetic efforts in its attempts to implement the abortive pension law of 1910, which the Confédération Générale du Travail and many employers opposed. Meanwhile spectacular government action against strikes alienated the labor movement.

The contrast between the political democracy of the Third Republic and its backwardness in labor legislation and industrial relations is, once more, apparent in Tournier's study. A single example: the Chamber of Deputies year after year begrudged the ministry and added a mere two more inspectors; in 1914 imperial Germany had 572 labor inspectors, while republican France had only 144.

VAL R. LORWIN  
*University of Oregon*

W. H. C. SMITH. *Napoleon III*. New York: St. Martin's Press. 1973. Pp. 296. \$14.95.

The aim of Professor Smith's book is, according to the jacket, to "rehabilitate a major European figure who until now has not received a true measure of historical justice." The Second Empire, he says, "appears to tap a particularly bilious duct in historians." Ever since Victor Hugo's original hatchet job historians have been content to follow the Hugo line and to "perpetuate a mythology" about Napoleon III.

Smith's enterprise, it seems to me, rests on a faulty premise. Although the author has read extensively in the secondary literature and has dipped into the archives he has chosen for some

odd reason to ignore many of the modern historical works that have portrayed what Alan Spitzer calls "the good Napoleon III." Albert Guérard's literate and impassioned defense of the emperor might never have been written; neither it, nor Guériot's detailed biography, nor Bury's judicious synthesis is even listed in the bibliography.

But even if the profession had been thirsting for a fresh and sympathetic reappraisal of a misunderstood statesman this would not be the book to fill the need. Its merits, in my judgment, are heavily outweighed by its faults. The writing is often careless, the language ambiguous, the syntax awkward. Far too often, dubious assertions are advanced as fact with little or no supporting evidence. More than once the author appears to contradict himself. On some important and complex issues (e.g., the class basis of Napoleon's support, or the nature of the liberal reforms of 1870) the argument is more confusing than enlightening. Some episodes are quickly passed over without adequate explanation; others are spun out at excessive length. This imbalance is made more obvious by Smith's interest in diplomatic history, which leads him into secondary byways and thickets that clutter the story. On top of all this Smith has been badly served by his publisher. A careful editor might have corrected many infelicities and ambiguities; a good proof-reader would surely have caught the innumerable typographical errors.

Perhaps this review proves at least one of Smith's points: that books on the Second Empire "tap a particularly bilious duct in historians." Or perhaps any subject treated in this manner would start the bile flowing.

GORDON WRIGHT  
*Stanford University*

BERNARD LE CLÈRE and VINCENT WRIGHT. *Les préfets du Second Empire*. (Cahiers de la Fondation nationale des sciences politiques, 187.) Paris: Armand Colin. 1973. Pp. 411. 107 fr.

This study of the 220 prefects who served Louis Napoleon during the Second Empire will interest specialists in the political and administrative history of that regime. Wright and Le Clère have joined their respective talents in



a work characterized by thorough research, erudite description, and clear topical organization.

A brief preliminary section discusses the impact of the coup d'état and repression (1851-52) on prefectural political functions and personnel. The second section reviews the subsequent history of prefectural power, arguing that administrative and political limits on the prefects were far more significant than critics have admitted. The third section presents evidence on the social and geographical origins, educational backgrounds, early careers, and political antecedents of the prefects. Here the authors suggest that administrative ability rather than wealth, family prestige, or political affiliation was the major criterion for recruitment and promotion within the prefectural corps, especially in the 1860s. They relate the trend toward "professionalization" and "bureaucratization" to that larger movement of "depoliticization" that reshaped prefectural functions as the regime drew to a close. Two further sections present disparate materials on prefectural life styles, administrative routines, incomes and expenditures, retirement benefits, and job insecurity, while a final section describes the fate of individual prefects once the Second Empire collapsed. Extensive appendixes and a fine bibliography complete the book.

The author's major thesis concerning the growing professionalism and declining political authority of the prefects is surrounded by so many biographical details that the reader is left more with an encyclopedic impression than with a persuasively developed argument. Their minute attention to the imperial prefects also lacks any broader comparative perspective on bureaucratic recruitment patterns, promotional practices, and functional roles. For example, if the social background of imperial prefects had been compared with that of all the subprefects, or with that of prefects in previous or subsequent regimes, the reader would have a basis for evaluating the significance of noble status and family wealth as particularistic criteria of appointment and advancement. Similarly comparisons with other European bureaucracies of the period would have established a standard for examining the relationship between educational performance and bureau-

cratic recruitment during the Second Empire. In their concern to rehabilitate the reputation of Louis Napoleon's prefects the authors may have underestimated the importance of social snobbery, oligarchical influence, and political favoritism within the prefectural corps. Only comparative analysis can provide a satisfactory answer to this problem.

TED MARGADANT  
University of California,  
Davis

PIERRE MIQUEL. *La paix de Versailles et l'opinion publique française*. (Nouvelle bibliothèque scientifique.) Paris: Flammarion, Éditeur. 1972. Pp. 610.

Miquel analyzes French opinion during the negotiation of the peace treaty of 1919. At the start of talks in January the French people had high expectations. On the one hand pacifists and socialists extolled President Wilson and his proposed international organization. On the other hand most Frenchmen were convinced that the treaty would provide for massive German reparations and for concrete and permanent guarantees of French security against future German aggression. News of the negotiations gradually destroyed both illusions. With the completion of the treaty in May it was clear that the French had lost the peace.

The revelation is how little the French protested. Maréchal Foch, in spite of his prestige, was unable to arouse opinion against Clemenceau's abandonment of the Rhine as the permanent line of French defense. The parliamentary debates over ratification were lackadaisical and were largely ignored by the press. The treaty was not an issue in the parliamentary elections of November. Miquel offers several explanations: preoccupation with domestic problems, the secrecy of the negotiations and the government's censorship and manipulation of the press, faith in the intransigent patriotism of Clemenceau, the reassuring despair of the Germans over the provisions. At the time of ratification the French were both unaware of the full extent of France's defeat at the peace table and resigned to France's inability to impose its views on its more powerful allies. The growth in 1919 of this attitude of resignation,

Miquel suggests, shaped the next two decades of French history.

But Miquel is unable to specify the real impact of the peace of Versailles on French public opinion. First, he can have no direct access to the attitudes of most French people. He focuses his research on the public's primary source of information, reading and classifying the articles in fifty-one Parisian and eighteen provincial daily newspapers, as well as in fifty-two weeklies and monthlies. But, as he admits, even this formidable accomplishment does not bring him to the level of popular *mentalités*. Second, the time span of his study is too limited. His detailed discussion ends with the completion of negotiations. Even if press coverage of the ratification proceedings was minimal and parliamentary candidates ignored the treaty it would seem impossible to appreciate the treaty's impact without analyzing what was said and not said at the conclusion of the whole affair. Furthermore a full appraisal would consider responses to later events such as the American rejection of the treaty and the French occupation of the Ruhr. Miquel's study is an invaluable first step.

The book is skillfully organized, but is long and repetitious. It is designed for specialists.

PAUL C. SORUM

University of North Carolina,  
Chapel Hill

E. A. KRAVCHENKO. *Narodnyi front vo Frantsii, 1934-1938* [The Popular Front in France, 1934-1938]. (Akademiia Nauk SSSR, Institut Vseobshchei Istorii.) Moscow: Izdatel'stvo "Nauka." 1972. Pp. 294.

During the past twenty years Soviet historians have devoted considerable attention to the period of the mid-1930s when the USSR was temporarily aligned with liberal and socialist elements in the West against the common enemy of fascism. Most of this Soviet literature has treated the more dramatic episode of the Spanish Civil War, but Western analysts like Max Beloff have long argued that the key element in Soviet calculations was the triumph of the broadest possible "anti-Fascist" bloc in France. Madame Kravchenko's unusually candid remark that "the civil war and intervention in Spain complicated the activity of the Popular Front," as well as her stress on French Commu-

nist efforts to include Catholics and other non-socialist elements in a French Front imply, if they do not wholly concede, the validity of this interpretation. Her quotation of Pietro Nenni's condemnation of Trotskyite agitation for a worker-peasant government in France is entirely in line with the position of putting limited objectives ahead of revolutionary millennial goals. Fidelity to this interpretation tends to conflict, however, with the author's avowed aim of using the Popular Front as a model for contemporary action: "In our day, too, the Popular Front retains its attractive power for the international worker and Communist movement." Collaboration "from above" with official social-democratic parties (Kravchenko briefly but bluntly rejects Stalin's earlier "unfounded" vilification of "Social Fascists") may fit both approaches. The alternative interpretation, however, emphasizes violent class conflict (as in the French uprising of May 1968, which Kravchenko explicitly cites) even at the expense of alienating middle-class elements.

The preceding paragraph suggests that the principal value of Kravchenko's work is its revelation of contemporary Soviet interpretations. This is true, I believe, because the author virtually confines her treatment to narration based on contemporary newspapers, memoirs, and the few published French documents. While she utilizes some major new economic histories (e.g., Alfred Sauvy's) she makes little effort to re-examine the impact of economic developments on social groups, voting patterns, or latent group influence in French politics. She does not even cite old semi-Marxist analyses of this kind like Augustin Hamon's, to say nothing of reinterpreting the wealth of data that recent French political scientists and sociologists have provided. This retreat from Marxist sociological analysis to rather old-fashioned narrative description is, of course, characteristic of Soviet historiography. With certain exceptions on Middle Eastern subjects straight chronological narration predominates especially in Soviet treatments of recent developments abroad. In Kravchenko's case the limitation is compounded by her failure to take into account some important interpretations presented by non-Communist writers (notably Franz Borkenau and A. Tasca [Rossi]) although she does use some strongly anti-Communist

sources. More surprising is her failure to cite Ilya Ehrenburg's recent extensive eyewitness accounts, despite the fact that she cites one of his minor *Izvestia* articles from the 1930s.

JOHN A. ARMSTRONG  
University of Wisconsin,  
Madison

JOHN E. DREIFORT. *Yvon Delbos at the Quai d'Orsay: French Foreign Policy during the Popular Front, 1936-1938*. Lawrence: University Press of Kansas. 1973. Pp. xi, 273. \$10.00.

This account of Popular Front diplomacy makes clear that Yvon Delbos, foreign minister from 1936 to 1938, was a man of very limited vision, thrust into prominence during the political turmoil of the Blum ministry. It is very likely that he did represent a large segment of French opinion on international affairs since he continued in office under Chautemps. He had two basic diplomatic principles: a devotion to the concept of collective security and a determination to bring about the closest possible relations with Britain.

According to the author these two principles led Delbos into serious diplomatic errors. In his desire for collective security he mistakenly attempted to recreate a viable alliance with the Little Entente, which gained nothing for France. More serious, according to Dreifort, was Delbos's failure to respond to Russia's diplomatic overtures. This would have seemed a logical policy for France to pursue, but, as Dreifort explains, the French general staff greatly underestimated Russia's military capabilities. Also Delbos's anti-Communist feelings were intensified by the role Stalin was playing in Spain, by the wave of Communist-led strikes in France, and even by the attacks on him by *L'Humanité*, which Delbos believed were instigated by the Soviet Union. In addition the British were cool toward such an alliance.

The fact that France followed Britain's lead in international affairs was again apparent when the Spanish Civil War began. Both Delbos and Blum wanted to intervene, but because of pressure from the British they decided against it. Dreifort introduces new documentary evidence for this interpretation, quoting an unpublished note from the British am-

bassador to France that made it emphatically clear that England was against intervention.

Delbos did make one attempt at an independent French policy by organizing the Nyon Conference at which the signatory powers agreed to patrol the Mediterranean to force Italy and Germany to comply with the non-intervention agreement. The author maintains that, contrary to most interpretations, France did take the diplomatic initiative in this instance and was not merely following in the wake of British policy. Even after Nyon had clearly failed, however, Delbos continued with his policy of strict nonintervention, and France relapsed into her role of diplomatic subordination to Britain.

Dreifort declares that France gave up her freedom of action in exchange for an assured position as Britain's junior partner. He also states that historians have tended to overlook Delbos's genuine efforts to pursue an independent policy. But perhaps this point has been overstressed because even the Nyon Agreement was undertaken with the approval and support of the British, and it was at Britain's insistence that Italy was made a party to the agreement.

This book does not substantially change the depressing picture of appeasement policy in the 1930s. The author has made good use, however, of newly available British Foreign Office papers. He has also conducted personal interviews with colleagues of Delbos and has drawn upon the resources of the Association les Amis d'Yvon Delbos, which collects and publishes memoirs about the foreign minister. In sum it is a well-documented, scholarly study and a welcome addition to the diplomatic history of the interwar years.

HELENA F. LEWIS  
Appalachian State University

JULES JEANNENEY. *Journal politique, septembre 1939-juillet 1942*. Edited by JEAN-NOËL JEANNENEY. Paris: Librairie Armand Colin. 1972. Pp. xix, 514. 65 fr.

The *Journal politique* of Jules Jeanneney (1864-1957), president of the French Senate, is an indispensable primary source for the history of the last days of the Third Republic and the Vichy regime. Jeanneney, a highly re-

spected and well-informed parliamentarian, recorded his observations and conversations with political leaders from September 1, 1939, through July 9, 1942, except for a few lapses. The *Journal* has been edited with meticulous care and objectivity by his grandson, initially as a doctoral thesis (third cycle). The work includes a perceptive introduction, voluminous annotations, an extensive index, and seven appendixes containing a short biography of Jeanneney, his notes defending the prerogatives of the Senate and his own actions, his recommendations to General de Gaulle in the summer of 1942 on the future constitution of France, and his deposition before the High Court of Justice in 1945. A bibliography of works cited in the footnotes would have been useful.

Much of the story is familiar, but the journal provides new evidence, penetrating and often highly critical comment on the politicians and generals, and a portrait of Jeanneney himself, a bourgeois lawyer from Haute-Saône, who served forty years in parliament, but refused to accept any ministerial post except that of under-secretary of state in Clemenceau's cabinet during the First World War. Jeanneney's judgments were generally sound although he was mistaken in his first appraisal of Pétain and only belatedly recognized de Gaulle's potentialities. He hated the Germans, opposed the Armistice, and favored moving the government to Algeria. He criticized the Center and Right for not defending the Republic, but was himself attacked for lack of leadership, praise of Pétain, abstention in the crucial vote on the grant of constitutional powers to the Marshal, and failure to recognize opposition deputies. Jeanneney held that Pétain was the symbol of national unity, that a presiding officer should abstain, and that the Marshal's supporters drowned out the opposition. His poor health, legalism, horror of civil war, and lack of aggressiveness prevented him from assuming leadership, but he bitterly attacked the Vichy government and tried to defend Mandel and Jewish senators. As minister of state in de Gaulle's provisional government in 1944-45 he made constructive proposals concerning constitutional procedures.

EVELYN ACOMB WALKER

State University of New York,  
College at New Paltz

HERBERT TINT. *French Foreign Policy since the Second World War*. New York: St. Martin's Press. 1972. Pp. xi, 273. \$12.95.

A history of French foreign policy since the Second World War must be to some extent an exercise in imagination. The government archives remain closed. Statesmen rarely keep their papers and even more rarely grant access to them. Memoirs of participants are more often than not *ex parte* arguments with greater literary than historical merit. Books purporting to reveal the "secrets" of state mix the true with the false in proportions impossible to determine. And newspapers and periodicals disclose less about the actual process of policy formation in France than in any other major Western country. Under such circumstances the most one can expect from a book on this subject is a plausible collection of the miscellaneous facts available, ordered with good judgment and given coherence by the use of structural categories drawn from political science.

Within these modest limits Tint contributes a highly serviceable primer, the first in a series of Foreign Policy Studies to be issued by St. Martin's Press. Tint's work is not as comprehensive as previous entries into the field by Alfred Grosser and Guy de Carmoy, but is markedly less ethnocentric. He never loses sight of the fact that France has become a middle-sized power with essentially regional concerns, whose pretensions to a wider role in world politics are increasingly anachronistic and peripheral to its vital interests.

This perception logically impels Tint to focus on France's relations with Germany, its other Common Market partners, and the Soviet Union. The Indochinese and Algerian wars receive relatively cursory attention. This is unfortunate because France's efforts to play at great-power diplomacy elsewhere are more comprehensible when viewed against a backdrop of defeat and humiliation in these wars and the national trauma occasioned by the failure to save much, besides cultural prestige, from the process of decolonization.

Tint is particularly good, on the other hand, in showing how it rankled the French to be dependent on American economic assistance for a full decade after World War II and how pent-up resentment fueled their subsequent determination to extrude the United States from

a leadership position in Western Europe. Without adding anything new Tint presents a lucid summary of the development of West European integration. He attributes periodic obstruction by de Gaulle and his predecessors primarily to old-fashioned nationalism and a desire to ensure that France dominate United Europe.

STEPHEN A. SCHUKER  
Harvard University

MIGUEL ARTOLA. *La burguesía revolucionaria (1808-1869)*. (Alianza Universidad. Historia de España Alfaguara, vol. 5.) Madrid: Alianza Editorial Alfaguara. 1973. Pp. 434.

Miguel Artola's book belongs to a new series on the history of Spain, of which he is the general editor. The series aims to give heaviest coverage to the recent past—an innovation for Spanish historians—and to emphasize social and economic structures, following the examples of Jaime Vicens Vives and the *Annales*.

This is Artola's finest book, perceptive, balanced, written in the tempered tone of a mature scholar. Many sections bring new clarity to the confusing reigns of Ferdinand VII and Isabel II, particularly to the latter, on which Artola has not previously written. At the level of political history his analysis of the issues in the Carlist War and the conflict between the *Moderados* and *Progresistas*—seen as a struggle over the organization of the state and the means of political control—are excellent. Artola also provides his own interpretation of economic developments. His discussion of the revolution in transportation is especially good. Against Jordi Nadal and Gabriel Tortella he asserts that Spanish legislation did little to retard economic growth; against Vicens Vives and Nicolás Sánchez Albornoz he argues that the Revolution of 1868 was not produced by an economic crisis; and countering common opinion he claims that *desamortización* did not alter the rural structure radically or lead to extensive expansion in agriculture. Spain's comparative backwardness, he argues, came out of its physical and social structure and, measured in absolute terms, its progress was great. Throughout, his reasoning is convincing.

Like those of the Langer series the book is built around a theme: the accession of the revolutionary bourgeoisie to power. Even the

chapter on culture sees it as a bourgeois product. (Culture is broadly conceived and includes the new urban life, travel, and summer vacations.) It is a puzzling theme, for although Artola believes in the existence of a bourgeois class he never describes it in detail, and the sketchy lines he provides do not call to mind the usual picture. The most powerful sector of the new ruling class, he says, given the circumstances, was inevitably agricultural and incorporated the old aristocracy. The riddle becomes clear when one realizes that for Artola the bourgeois nature of the period is revealed not by the characteristics of the dominant class, but by its legal, economic, and constitutional system, which was classically liberal. The new regime was bourgeois because it replaced the older society of orders with a "classist society organized on the doctrines of liberty, equality, and property," and opposed freedom of association (for workers) and the welfare state. Unfortunately his emphasis on the term "bourgeois," heavy with implications associated with more commercial and industrialized countries, suggests the presence of a stronger middle class than Spain had and clouds the perceptiveness of his interpretation.

On the whole, however, this is a very good book. Artola has given his collaborators a hard act to follow. If the rest of the series measures up, it will become the standard survey of Spanish history, replacing, or at least complementing, Vicens Vives's *Historia de España y América*.

RICHARD HERR  
University of California,  
Berkeley

CLARA E. LIDA. *Anarquismo y revolución en la España del XIX*. [Madrid:] Siglo Veintiuno de España Editores. 1972. Pp. 334.

JOSEP TERMES. *Anarquismo y sindicalismo en España: La Primera Internacional, 1864-1881*. (Colección Horas de España.) Barcelona: Ediciones Ariel. 1972. Pp. 670. 750 ptas.

Historians have studied the First International in Spain, 1868-74, using almost exclusively either the prism of ideology or that of the international anarchist movement directed by Michael Bakunin. Working independently of one another, Professors Clara Lida of the State

University of New York (Stony Brook) and Josep Termes of the Autonomous University of Barcelona have brought these events into focus as an on-going problem in Spanish history. They analyze the process by which the ideology and tactics of anarchism became the official program of the Spanish Federation, which was founded by vote of the first national workers' congress in 1870. Neither purports to write a comprehensive account. Termes studies the way anarchism was used to mobilize the Catalan industrial labor force. Concerned with anarchism and revolution, Lida examines the Republican revolutionary movement. Both trace the origins of the First International to Spain's political and labor agitation before 1868 and then move forward to the subsequent organization of the federation.

The Spanish Federation was the public organization, but its motive force was the *Alianza*, a semiclandestine leadership group inspired by Bakunin. Termes's delineation of the *Alianza*'s role, particularly in the clandestine era after 1874, is an important contribution; he has skillfully integrated the material from the Max Nettlau archives published by Renée Lamberet to complement the classical account of Anselmo Lorenzo. But much more work is needed in foreign archives, particularly in Geneva, to show the precise degree and manner that *Aliancistas* collaborated with Bakunin's international brotherhood.

The solidly new aspect of these works is the close analysis of the division within the ranks of Spanish anarchism: on the one hand, the Catalan labor leaders who needed legality in order to be able to organize a mass workers' movement and thus, in turn, to be able to resist factory owners (the dimension analyzed by Termes), on the other, the Andalusian peasantry who adopted terrorist tactics and clandestine action groups because they constituted the most viable weapons for an impoverished agricultural proletariat residing in agro-towns (the concern of Lida).

The way in which officials of the Spanish Federation assimilated and implemented the program of the First International is charted in greatest detail by Termes who has studied the minutes and directives of the federal councils and commissions, the editorials of the Bar-

celona organ (*La Federación*), and the proceedings of the three labor congresses. But he has also been at pains to describe the reactions of lower echelon officials in Barcelona, and those of workers themselves. In this context he analyzes the ideas that reached workers through popular political ballads and broadsheets and includes an appendix of this source material, invaluable for all historians concerned with what workers really wanted and believed. Although he surveys the strikes called by member unions of the federation during these years, Termes himself points to the need for further study of labor conflict.

Reacting against the interpretation of Andalusian peasant insurrections as originating with the anarchism of the First International, or as messianic in inspiration, Lida considers as precedent the armed insurrections of Republican clandestine conspirators, which she argues incorporated the ideas of Utopian socialism. Her evidence indicates these ideas entered Spain through Andalusian (not Catalan) newspapers. But to prove that they were a political force she must show that peasants knew about and reacted to the ideas. Another contribution is the data from diplomatic and police files in Paris and in Washington showing contacts between Andalusian Republicans and European movements for social and political justice. Finally she offers a new interpretation of the Black Hand criminal trials of 1883, as being both an epilogue to the First International and a protest of desperate Andalusian peasants against the Catalan-oriented tactics of the federation's national leadership. Two difficulties are the nature of the evidence on workers' activities (always suspect as worker, police, and politician accuse one another of fabricating evidence for plots and counterplots) and the tendency to describe isolated examples in the light of new evidence rather than to provide a continuous narrative that would link mid-nineteenth-century Republicans with anarchists. But Lida, like Termes, has provided invaluable new documentation and analysis for the events of the First International as the product of socioeconomic and political realities in Spain itself.

JOAN CONNELLY ULLMAN  
University of Washington

RAYMOND CARR, editor. *The Republic and the Civil War in Spain*. (Problems in Focus Series.) [New York:] St. Martin's Press. 1971. Pp. x, 275. \$10.00.

PIERRE BROUÉ and EMILE TÉMIME. *The Revolution and the Civil War in Spain*. Translated by TONY WHITE. Cambridge, Mass.: MIT Press. 1972. Pp. 590. \$12.50.

The contrast between Raymond Carr's *The Republic and the Civil War in Spain* and Pierre Broué and Emile Témime's *The Revolution and the Civil War in Spain* is summed up in the single word that differentiates each title. The contributors to the Carr volume cover a lot of ground, but most of their articles revolve around questions of responsibility for destroying the Second Republic, while Broué and Témime focus on the Left and Right revolutions and the diplomacy they necessitated.

First published in French in 1961 Broué and Témime's book is, in my opinion, the best single volume on the Spanish Civil War. It is divided into two discreet parts. Broué deals with wartime peasant and proletarian attempts to crush Republican capitalism replacing it with collectivism. Had the revolution within the Civil War been allowed to proceed, he argues, the popular forces would not have been demoralized as they were after May 1937, and the Loyalist Republican government might have defeated the Nationalists. His thesis is intended to refute claims that only Stalinist communism, allied with the agricultural and industrial petty bourgeoisie in the Republican sector, was capable of winning the war.

Partial supporters and extreme critics of this view are represented in Carr. Hugh Thomas attempts to show how varied and, in some cases, successful agrarian communes were. As a passionate defender of the anarchists and the anti-Stalinist POUM, Burnett Bolloten forcefully laments that, in the name of defeating fascism, the Communist party squelched the revolution. Ramón Salas Larrazábal, concerned with questions of legitimacy, claims that the Left's revolution—not the Fascists or Insurgents—destroyed the republic. And Raymond Carr intimates that what little chance the republic had to succeed was reduced by the anarchists' spontaneous revolution.

Emile Témime takes up the issue of the Right revolution carried on by the Nationalists and the problem of diplomacy. Témime was among the first liberal historians to argue that Francisco Franco was not a Fascist but a skilled authoritarian politician, who not only built a victorious army but co-opted the Falange and welded the antagonistic forces of the Right into a new traditionalist state able to provide social services. From quite an opposite point of view Ricardo de la Cierva portrays the Nationalists as a popular movement. He asserts that public donation paid for the Italian planes necessary for Nationalist victory and that 80 per cent of the northern Insurgent army were eager volunteers not conscripts.

Témime, de la Cierva, Carr, and Robert Whealey all devote space to the international ramifications of the war although Carr supports Gabriel Jackson (the leading American Civil War scholar absent from Carr's collection) in stressing that too much attention has been paid to foreign intervention and not enough to domestic questions. Carr refutes the shibboleth that the Spanish Civil War was the first act of World War II; he says its international importance has been overestimated. The others dissent, and Whealey is quite explicit when he writes: "It can be argued that forceful Anglo-French intervention on the side of the Republic in July 1936 might have saved liberalism" (p. 233).

De la Cierva claims that both sides received equal foreign aid. Possibly the most interesting statement in either volume is his assertion that "the unlimited supply of fuel oil and lubricants on credit from oil companies in the southern United States, and the pressure of Catholic opinion there which kept the embargo on arms for the Republic had perhaps as important an effect on the course of the Civil War as other more highly publicised contributions" (p. 206).

Several of Carr's historians think the republic's salvation lay in a middle road not taken because the Left prevented it. Edward Malefakis conjectures that there might have been a centrist political coalition uniting groups from Prieto Socialists to the Gil Robles' Christian Democratic C.E.D.A., which he defends against charges of Fascist tendencies. He is supported by Richard Robinson who ex-

onerates the Right from any blame in polarizing the republic "because it was the revolutionary left which brought about the failure of the legalist C.E.D.A. as the party of the right-wing masses" (p. 74). And Stanley Payne implies that Manuel Azaña and the Republicans were responsible for their own doom because they collaborated with the Socialists during the republic and attacked the army.

Needless to say Franco's army, the militias, and foreign armed forces receive a great deal of attention in the pieces by Témime, Payne, Larrazábal, Whealey, and de la Cierva. Taken together—although they disagree on many points—they show that the army and the Church were the victors on the Nationalist side. Unfortunately the political activities of the Church are considered only in passing in either volume. But old platitudes are put to rest. Both books succeed in showing that the complexity of political groups on the Right matched the more widely recognized divisions on the Left.

TEMMA KAPLAN  
*University of California,  
Los Angeles*

H. V. LIVERMORE. *Portugal: A Short History*. (Short Histories of Europe, 3.) [Edinburgh:] Edinburgh University Press; distrib. by Aldine Publishing Company, Chicago. 1973. Pp. 213, 4 maps. \$9.00.

RICHARD HERR. *Spain*. (The Modern Nations in Historical Perspective.) Englewood Cliffs, N.J.: Prentice-Hall. 1971. Pp. x, 306. \$7.95.

STANLEY G. PAYNE. *A History of Spain and Portugal*. In two volumes. [Madison:] University of Wisconsin Press. 1973. Pp. xvi, 349; xiii, 351-712. \$10.00 each.

It is perhaps not too homely to compare the problem of writing a national history with those of packing for a long trip. Should the author try to take nearly everything, somehow compressing it into the bulging suitcase, or should he cut away the unnecessary parts of his historical garments, covering only the essentials? Perhaps it would be better to pick only the relevant items, and with some daring combination of tuxedo, bathing suit, and sheepskin coat brave all weathers and conditions.

The authors of these three books have

packed their baggage in different ways, and each of their books emerges as a kind of design compromise between narrative continuity, topical clarity, and conceptual insight. Livermore chooses to organize his book around narrative, considering it his task to "narrate the main events of political history with some account of social organization." Moreover he makes this political history complete and deals with Portuguese history from the remotest times to the end of the Salazar regime. Since Livermore has little elbow room (197 pages) the result is a densely packed political narrative interlarded with brief sections on economic and social matters. He gives about one-third of his space to the period up to 1385. The time from the advent of the house of Avis to the rise of the Braganza kings gets about 25 per cent of the pages. The balance of the book brings us up to 1964, with heavy emphasis on the nineteenth century. The focus of the narrative is naturally on metropolitan Portugal and particularly Lisbon. Brazil and the Far East get attention only in passing as their histories bear on that of the Peninsula.

Almost every sentence Livermore writes has one or more items of information to convey. The author's prose is clear if uninspired, and, if the reader is moved by his own interest in Portuguese history, he will find a selection of interesting data. But reader and author both have to pay a price for the decision to organize the book around a political narrative. To my mind Livermore's book is insufficiently conceptualized. He is reluctant to subject his facts to the control of ideas or to make general propositions. The result is not only difficult reading but frequent uncertainty about what the author himself thinks about the facts. Even when he brings together various themes, such as economic revival, Brazilian trade, and intellectual innovation during the ministry of Pombal, somehow the interaction of various factors (the sense of process) does not emerge. Nor is there anything daring or innovative about Livermore's arrangement of the data. The dust jacket of the book provides a capsule review: "[This] book may not modify prevailing views but it will certainly better inform them." In short, then, Livermore's work is a brief factual summary, essentially correct and remarkable for its completeness within the space allowed,



but eschewing attempts at profound penetration or interpretation.

In contrast to Livermore, Herr's *Spain* is highly selective and conceptualized. Herr commences *in medias res* with the Franco government's execution of Julian Grimau, an alleged war criminal and Communist subversive, in 1963. From this he moves to a *tour d'horizon* of the state of Spain twenty-five years after the end of the Civil War. Only then does he move back to the historical roots of Spain. Daringly the author brings us from the Romans to the eighteenth century in just twenty-two pages. He does this by discussing the philosophical interpretations of Spanish history advanced by Unamuno, Ortega, Americo Castro, Sanchez Albornoz, and Vicens Vives. Following their lead he tries to define the Spanish character in terms of historical experience. With the making of Spain disposed of the author turns to the origins of contemporary Spain, which he sees as beginning with the eighteenth century, especially with the reign of Charles III. Herr then proceeds in a more leisurely fashion, using 35 per cent of his pages on the period up to the end of the Primo de Rivera dictatorship. On the period since 1930 Herr becomes more detailed and thorough, devoting over half his book to the Second Republic, the Civil War, and the subsequent Franco regime.

This treatment gives Herr's book cogency and relevance to contemporary concerns as well as a focus that will interest most readers. The author's interpretative device is an interesting elaboration of the "two Spains" thesis. Briefly, he sees the split between enlightened and traditional Spain as paralleled by another gulf between the landless peasantry and the great latifundia. This separation, however, was bridged by the common allegiance of landlord and peasant to Catholic tradition. This transcendent fact rallied the peasantry to traditional politics well into the twentieth century. A third dichotomy is the regional tension between the center of Spain, Castile, and the periphery, most notably Catalonia. This tension has endured to the present.

Like Livermore, Herr has to pay a price for his choice of approach. I cannot suppress a certain unease at the rapidity with which the medieval period is passed over. Many historians think of it as the decisive period of Spanish

history. Are the ideas of Castro, Sanchez Albornoz, etc., suggestive and fascinating as they are, adequate to explain the period and complexity of events? Nevertheless Herr's work is thoughtful, penetrating, and written in a lively style that attracts the attention.

In contrast to Livermore's and Herr's strictly national histories Payne takes the large task of dealing with both Iberian nations. This involves not only a larger study but the additional problems of balance and a kind of implicit contract to make comparisons. To be sure the author has more space with two volumes and 676 pages at his disposal. He, like Herr, seems to regard the eighteenth century as the beginning of the modern period for he divides his work almost equally between the years since 1700 (vol. 2) and all the previous eras of Iberian history. Portugal receives about 16 per cent of the pages, alternating with the more extensive treatment of Spain. The author deals with each country as they passed through parallel stages, and the transition from one to the other is smoothly managed.

Payne conceived his work as both a textbook and an interpretative account and thus committed himself to a compromise, which he nicely achieves, between the requirement of narrative continuity and the demands of explanation. The work is organized essentially around concepts, but with sufficient narrative to make us aware of process without overwhelming us. Moreover the work is careful with facts, and its conclusions are ably drawn. Payne is more cautious in interpretation than Herr. He does, however, advance the highly suggestive idea that Spain should not be compared to Britain, France, and Holland, but rather to the nations of southern and eastern Europe (notably Poland) whose historical experience and social conditions were more similar to Spain's. Some very useful and interesting perspectives flow from these comparisons.

All three works have indexes, useful maps, and charts, while Livermore's has, in addition, some interesting photographs, though it lacks any bibliography or references. On the other hand Herr and Payne have useful and quite complete bibliographies.

On balance the Payne and Herr books must be accounted successful. They carry out their authors' intentions, and they would be useful

as texts or simply as well-done national histories. Herr is perhaps more daring in interpretation, but Payne's judiciousness and broad coverage are attractive. Livermore's work has to be considered as less successful, largely on account of its old-fashioned focus on political narrative and the author's reluctance to conceptualize.

GEORGE M. ADDY  
Brigham Young University

C. B. WELS, editor. *Bescheiden betreffende de buitenlandse politiek van Nederland* [Documents on Foreign Policy of the Netherlands]. First period, 1848-1870. Volume 1, 1848. (Rijks geschiedkundige publicatiën, Major Series, 139.) The Hague: Martinus Nijhoff. 1972. Pp. xxxi, 714.

This massive collection of documents is part of a government-sponsored project begun two decades ago and now nearing completion, the publication of the archives of the Ministry of Foreign Affairs from 1848 through 1919. As is the case with previously issued volumes this one is ably edited (Wels is a specialist in mid-nineteenth-century Dutch foreign affairs). It includes notably complete indexes, extensive footnotes, a valuable bibliography, and a fine introductory essay. The bulk of the documents (many of which are in French, the diplomatic language of the day) are the reports coming into the Hague, and the replies to them. Arranged chronologically, they add up to an interesting picture of the "year of revolutions" as it appeared from the vantage point of a small but important state.

For the Netherlands 1848 was the year when William II (a brother-in-law of Nicholas I of Russia) accelerated his cautious consideration of Liberal demands (led by J. R. Thorbecke) and granted a constitutional revision that for practical purposes meant a parliamentary regime. The wider events of 1848 doubtless influenced this action. For the Ministry of Foreign Affairs the year was a difficult one. Apart from the ministry's natural aristocratic distaste for the revolutions it soon became clear that specific Dutch interests were involved. Both the Hague and Brussels feared an adventurous French foreign policy, perhaps involving French troops in support of Belgian republicans. In the face of this possibility Dutch-Belgian an-

tagonisms (the treaties finalizing the Belgian separation were only a decade earlier) softened considerably. Of even more direct concern was the problem of Limburg: as a result of the 1839 treaties, it was, curiously, both a Dutch province and a member of the Germanic Confederation, thus owing a dual allegiance. Its representatives to the Frankfort Assembly supported the separation of Limburg from the Netherlands. Hence events in Frankfort and Berlin became a major concern as the year wore on.

Dispatches from Vienna, St. Petersburg, London, Madrid, and elsewhere round out the collection and include passing references to hundreds of players in the historical drama, ranging from Metternich to Lola Montes to Lewis Cass (Zachery Taylor was the preferred presidential candidate). In summary, these documents will interest not only historians of the Netherlands, but also those concerned with 1848, especially those dealing with the Frankfort Assembly.

DIRK W. JELLEMA  
Calvin College

GUNILLA INGMAR. *Monopol på nyheter: Ekonomiska och politiska aspekter på svenska och internationella nyhetsbyråers verksamhet 1870-1919* [Monopoly in News: The Economic and Political Aspects of the Operation of Swedish and International News Agencies 1870-1919]. (Scandinavian University Books. Studia Historica Upsaliensia, 52.) [Stockholm:] Esselte Studium. 1973. Pp. xiv, 240.

International agreements between European press associations sought to reduce competition and assure profits. The first pact of 1859 proved too limited and a second of 1870 among Reuters, Havas, and Wolff (British, French, and German) framed a cartel arrangement that lasted through World War I. Even with renewals the arrangement withstood political pressures from governments and adverse national opinion, proving that profits counted for more than national interest. The press associations divided the world into spheres of operation, just as the political powers carved out zones of influence.

The Swedish press association Telegrambyrå, a private venture with Foreign Office support, received a portion of its international news via

Ritzau in Copenhagen. Because Sweden and other northern countries fell within Wolff's sphere, during World War I another press association, Nordiska Presscentralen, was created to disperse Allied news. Ingmar's description of the conflict over control of Telegrambyrå and its competitors demonstrates the subordination of the Swedish press to both the Germans and monopoly control by the major news services. The full implications of the major news bureaus influence on governments and international relations is, however, somewhat lost, both through neglect of works such as those of Oren Hale and E. Malcolm Carroll and through concentration on financial aspects and intrigues by individuals trying to maintain their grasp. This latter point is especially damaging to the detailed study of Telegrambyrå, where a content analysis of news would easily have proven the point of the thesis. On the whole it appears that governments influence news gathering and distribution negligibly, as when Wolff refused to do the German government's bidding or where the same government permitted use of Reuters and Havas services in World War I.

Although too brief to permit a full detailed study of the international press associations or Swedish Telegrambyrå this effort contributes knowledge on the subject. Its summaries (often with unnecessary duplication), publication of texts of agreements, and bibliography on a specialized topic are a contribution worthy of note for the historian interested in the press.

RAYMOND E. LINDGREN  
California State University,  
Long Beach

SEPPO MYLLYNIEMI. *Die Neuordnung der baltischen Länder, 1941-1944: Zum nationalsozialistischen Inhalt der deutschen Besatzungspolitik.* (Dissertationes Historicae 2; Historiallisia Tutkimuksia 90.) Helsinki: Suomen Historiallinen Seura. 1973. Pp. 308.

At a time when students of Nazi Germany are increasingly emphasizing the major personalities at the center of power a young Finnish historian Seppo Myllyniemi, has given us an institutionally-oriented examination of rule at the periphery—German wartime occupation policy in Estonia, Latvia, and Lithuania. Based on a wide variety of archival sources Myllyniemi's detailed work largely substantiates

the major themes of the Baltic chapter of Alexander Dallin's monumental *German Rule in Russia*.

The Nazis' long-range goal of incorporating the Baltic area into the Reich clashed with and ultimately superseded the immediate task of winning the war in the East. Hence, after lengthy debate the Germans refused to grant the minimum popular demand for semi-independent protectorate status for the three former states, made only token restitution of private property earlier nationalized by the Russians, and waited until military defeat was staring them in the face before authorizing sizable native armed forces in Latvia and Estonia.

This impractical rigidity was grounded in Nazi racial theory. The Baltic area fell into a middle category in Hitler's New Europe roughly parallel to Bohemia and Moravia: a site for future German colonization where the indigenous population contained large segments that were potentially Germanizable (*eindeutschungsfähig*). Persistent attempts to convince the lucky ones of their Nordicness, however, usually ran afoul of national pride, while severe colonial exploitation eroded the initial enthusiasm for the Germans as liberators from Soviet oppression.

Haphazard policy execution compounded the problems caused by contradictory goals. Irreverent Berliners quickly dubbed Alfred Rosenberg's East Ministry the "*Chaostministerium*," and the *Reichskommissar* for the Baltic, Hinrich Lohse, presented a particularly vivid picture of fanatical ineptitude. Moreover, Rosenberg's losing battles with Himmler, Göring, and Koch further confused German Baltic policy; the helpless ideologue was still concocting administrative reforms when the Red Army was at the doorstep. A careful reading of this book will surely disabuse one of any lingering belief in the alleged efficiency of dictatorships.

The author is especially effective in tracing several tragicomic ideas of little men in the party bureaucracy. For example, the Estonians were judged to be racially superior to the Latvians, who in turn were supposedly superior to the Lithuanians. Unfortunately the Estonian language, a Finno-Ugric tongue, did not correspond to the Nazi racial-linguistic schema,

so officials were cautioned against bringing up this touchy subject.

A weakness of this book is the failure to discuss thoroughly the effect of German policy on the daily lives of the Baltic natives. Though Myllyniemi's dry descriptions of bureaucratic affairs often make for difficult reading his study nonetheless is a valuable contribution to an understanding of Nazi occupation policy.

MICHAEL H. HALTZEL  
Hamilton College

CARL HAASE. *Ernst Brandes, 1758-1810*. Volume 1. (Veröffentlichungen der Historischen Kommission für Niedersachsen und Bremen, 32. Niedersächsische Biographien, 4.) Hildesheim: August Lax Verlagsbuchhandlung. 1973. Pp. xvi, 443. DM 48.

This volume by Carl Haase is obviously a labor of love. Based on an exhaustive study of all of Brandes's published writings and reviews as well as on unpublished archival material in the Westphalian archives, this book discusses the total literary achievement of this interesting exponent of enlightened conservatism during his formative and early adult years. After discussing Brandes's early years within the framework of local Westphalian history, Haase summarizes Brandes's writings through paraphrase and comment and relates them topically to the issues and debates current in Germany at the end of the *ancien régime* and during the first years of the French Revolution.

The first hundred pages of this often tedious study present Brandes's biography until 1791 against the backdrop of institutions and personalities that have either been neglected altogether or have received cursory mention in previous studies of the period. I found this part of Haase's book the most interesting, as it refers to such fascinating tidbits as the association of Ernst Brandes's father, Georg, with Moses Mendelssohn, the friendship of the Brandes family with Heine and Forester, the academic politics in Göttingen, and the bureaucracy and social structure in Hannover during the late eighteenth century. Based on an intimate knowledge of many of the printed and unpublished sources, this part of the study suggests among other things a "republic of letters" that crossed political lines. It is unfortunate that Haase leaves too much to the

imagination here, a failing conspicuously absent from the remainder of the book.

The rest of this study is concerned with topically presenting Brandes's views on a variety of issues ranging from the theater, the English constitution, secret societies, women, the relationship between the nobility and the middle classes, the role of the lower classes, the science of history, social life in Hannover, and contemporary morals to the French Revolution. It is the last of these that has received the most attention from scholars—rightfully so, in my opinion, as Brandes was not only one of the first German writers to approach the study of this momentous event critically but also followed the course of events with attention and judicious insight. Haase's own concluding chapter on Brandes and the French Revolution is interesting, if not fascinating. Unfortunately many of the chapters that precede seem to be there only to support Haase's contention that Brandes remained consistent throughout his career in his politics, his Anglomania and in his prudent avoidance of controversy with such politically dangerous arch-conservatives as Johann Zimmermann. Haase consequently drags his reader through a lengthy presentation of Brandes's views of the prerevolutionary period where any humor that might offset the banality of Brandes's arguments is lamentably absent. Indeed, if it were only for the chapters on the theater, history, women, and so on, one might well wonder whether it would not have been kinder to disregard the plea of the late Klaus Epstein for a study of Brandes's achievement and leave Brandes in the partial obscurity that his thought on these subjects merited. This impression is unfortunately exacerbated by Haase's frequent attempt to present Brandes's views as profound and even relevant to our times.

The bibliography is excellent and, unlike the bulk of the book, valuable for any serious student of late eighteenth-century German intellectual history.

FRANZ G. NAUEN  
University of California,  
San Diego

WILHELM LIEBKNECHT. *Briefwechsel mit deutschen Sozialdemokraten*. Volume 1, 1862-1878. Edited, with an introduction, by GEORG ECKERT.

(Quellen und Untersuchungen zur Geschichte der deutschen und österreichischen Arbeiterbewegung. New Series, number 4.) Assen: Van Gorcum & Comp. 1973. Pp. lii, 908. 116 gls.

This superbly edited collection is only the first installment of a projected multivolume edition of Wilhelm Liebknecht's correspondence with socialists and other radicals from 1862 to his death in 1900. Liebknecht played a key role in the Social Democratic movement, not only within Germany but also internationally, and one naturally expects that his extensive exchange of letters will be an exceptionally valuable source for the history of European socialism. The expectation, however, is not completely fulfilled in this first volume. One disappointment is that only 103 of the 534 letters in the main body of the text are by Liebknecht. As a result it is seldom possible to get a sense of thematic continuity through a sustained exchange between Liebknecht and another person. The lacunae are not the fault of the editor, Georg Eckert, who carried on a thorough search for missing Liebknecht letters and had the cooperation not only of the International Institute for Social History in Amsterdam, which is sponsoring the publication, but also of the Institutes of Marxism-Leninism in East Berlin and Moscow. Eckert has been able to compensate to some extent for the gaps by the use of related documents and explanatory footnotes. But the nature of the available material made it inevitable that the volume would have a certain diffuse and fragmented character.

These letters have something to say on nearly every issue related to the early socialist movement, but it is also amply clear that Liebknecht's correspondents were concerned predominantly with practical matters of organization and day-to-day tactics. Broad questions of policy or socialist theory come up only occasionally. The fact is that Liebknecht was far less immersed in theoretical problems than his pamphlets and speeches often lead people to assume. This point was already evident in his correspondence with Marx and Engels, also edited by Georg Eckert (1963). The practical orientation, however, does not diminish the value of this documentation for historical purposes. On the contrary, for these letters impart a mood of personal immediacy and contain a

great wealth of detail on a variety of topics, including conflicts between Lassalleans and Eisenachers, problems of early socialist journalism, relations with the First International, tensions within the Austrian labor movement, socialist attitudes on the Franco-Prussian War, the trial of Liebknecht, Bebel, and Hepner for high treason in 1872, and the merger of the two socialist parties in 1875. One also finds here a rewarding source for information on a number of lesser-known personalities; for example, the letters by Robert Schweichel, the nearly forgotten author of socialist belles-lettres, give some interesting hints on how early socialists approached the arts. When this project is completed—there are at least three volumes to come—it will certainly be one of the major sources for the history of German and European socialism.

VERNON L. LIDTKE

*Johns Hopkins University*

HENRY ASHBY TURNER, JR. *Faschismus und Kapitalismus in Deutschland: Studien zum Verhältnis zwischen Nationalsozialismus und Wirtschaft*. Göttingen: Vandenhoeck & Ruprecht. 1972. Pp. 185. DM 18.

This little volume contains the German translation of Professor Turner's recent articles, all but one of which originally appeared in American historical journals, including the *AHR*. It is most appropriate that the author's conclusions become known to a larger audience, for they explode some myths.

While the essays were designed for separate publication and therefore do not have a consistent thesis that ties them together as a book, the reader detects as a general theme the relations between big business and right-wing politics in Weimar Germany; the last essay analyzes fascism as an antimodern phenomenon. More specifically, the first four articles attempt to answer the question, "Did German big business support Adolf Hitler's climb to power?" After all, such prominent tycoons as Emil Kirdorf and Fritz Thyssen were closely linked with Hitler's movement several years before he seized power. Turner convincingly demonstrates that Hitler's party received very substantial financial support from business, but only after, not before, Hitler became chancellor. A secret brochure by Hitler,

which is contained in the second essay of this volume, shows that he had succeeded in gaining the support of Kirdorf as early as 1927. As the third essay in this collection reveals, however, the relations between the "Bismarck of the coal industry" and the Nazi party were characterized by Kirdorf's admiration for Hitler but only a short-lived membership in the party, from which he resigned in a huff in 1928. Both Kirdorf and Thyssen, whose ghost-written *I Paid Hitler* (1941) is critically analyzed for factual accuracy in the fourth essay, lent, for the most part, just their names and reputations to the Nazi movement before 1933 rather than the alleged vast sums of money mentioned by numerous historical works. Similarly, the *Ruhrlade*, the secret cabinet of the leaders of the largest coal and iron firms who regularly met after 1928 to deal with problems of common concern, made only very modest monetary contributions to the Nazis before 1933. The analysis of this subject is followed by the concluding essay that characterizes National Socialism as "a utopian form of anti-modernism" and raises but does not answer the question as to whether Italian fascism and other fascisms can be similarly perceived.

These essays give answers to several of the problems raised, but they should also prove a strong stimulus for further research. Since the author made only minor editorial changes in the essays, one wonders if a substantive introduction would not have enhanced the unity and sharpened the perspective of this well-translated volume.

GEORGE P. BLUM  
Raymond College,  
University of the Pacific

WERNER MASER. *Hitler: Legend, Myth & Reality*. Translated from the German by PETER and BETTY ROSS. New York: Harper and Row. 1973. Pp. viii, 433. \$12.50.

Over the past twenty-odd years Werner Maser has collected more bits and pieces of "Hitleriana" than anyone alive today. This clearly makes him the world's leading "Hitler buff," though not necessarily "Germany's foremost authority on Adolf Hitler," as his American publishers claim. He has used this information, some significant, some irrelevant, in a number

of repetitious books, of which this one is the first to appear in English. It differs in many parts from the German original, *Adolf Hitler: Legende, Mythos, Wirklichkeit* (1971), which is more detailed and about a hundred pages longer. There is no indication who made these changes. They do not seriously affect the general tenor of the book, however, and they make it more readable.

Except for the opening chapters dealing with Hitler's early life and the last one on his activities in World War II, Maser's book is not so much a biography as a series of essays on various aspects of Hitler's career and personality. Their purpose is to correct whatever "legends" have grown up around the Führer. It should be noted that some of these legends have already been challenged by earlier writers and others have never been taken very seriously.

On Hitler's family background Maser discounts the rumor that his grandfather was Jewish, and he adduces further evidence of inbreeding among his forebears. Hitler's childhood and youth were far less dismal than the Führer describes in *Mein Kampf*. As artist and architect, Hitler, according to Maser, had far more talent than he is usually credited with. His record as a soldier in World War I was "comradely, level-headed and uncommonly brave" (p. 83). A long discussion of Hitler's intellectual background emphasizes the influence of some hitherto neglected figures, notably the biologist Wilhelm Bölsche, and describes Hitler as a voracious reader with a "sceptical, imaginative, speculative and ceaselessly fertile" mind (p. 191). Hitler's sex life, Maser states, was normal, and he denies that Hitler had syphilis. Much attention is given to Hitler's illnesses. New evidence confirms, though fails to explain satisfactorily, Hitler's rapid physical decline, starting in 1942. (The subject of the Führer's health deserves further study by more qualified specialists.) It also provides the theme for the last chapter dealing with Hitler's role as military leader. The one point that has caused most debate in Germany is Maser's assertion that the remains on which the Russians based their famous autopsy report—Lev Bezymenski, *The Death of Adolf Hitler* (1968)—were not Hitler's.

Maser's book, as these few samples show, covers a lot of interesting ground. The reader,

however, who hopes to gain from it a better understanding of the Hitler phenomenon comes away disappointed. There is no attempt made to fit the new evidence into the traditional Hitler image. Without some such synthesis a reader unfamiliar with the subject might gain the impression that Hitler "wasn't really so bad," that he was a normal person of wide-ranging artistic and intellectual gifts who suffered from innumerable illnesses for which he was given the wrong medications, which then made him do horrible things. To create such an impression, of course, was far from the author's mind. But one wonders if, in concentrating on a number of minor "legends," Maser is not, unintentionally, helping to restore the major "myth" that haunted Germany for twelve terrible years.

HANS W. GATZKE  
Yale University

ANTHONY NICHOLLS and ERICH MATTHIAS, editors. *German Democracy and the Triumph of Hitler: Essays in Recent German History*. (St. Antony's College, Oxford, Publications, number 3.) New York: St. Martin's Press. 1971. Pp. 271. \$12.95.

HORST VON MALTITZ. *The Evolution of Hitler's Germany: The Ideology, the Personality, the Movement*. New York: McGraw-Hill Book Company. 1973. Pp. xiv, 479. \$12.95.

The essays assembled by Nicholls and Matthias in *German Democracy and the Triumph of Hitler* are all thoughtful and suggestive. Indeed some are cogent and compelling. These papers, largely the work of young historians, were originally presented in seminars at St. Antony's College, Oxford, and the University of Mannheim. Although diverse and wide-ranging in topic and technique, most of the essays portray the political pathology of Weimar Germany. Several authors convincingly restate or reinforce familiar theses concerning the syndromes of unimaginative German liberalism and socialism. Other essayists suggest new sources, signs, and symptoms of early Nazism. In "Hitler and the Bavarian Background to National Socialism" Anthony Nicholls finds Hitler's Bavarian experience so "crucial and seminal" that he conjectures that "had Hitler tried to establish himself in any other German capital in 1919 it is almost inconceivable that he could have

achieved the same results" (p. 101). Such tantalizing, if unprovable, speculation is fortunately accompanied by fascinating and closely reasoned arguments about the Bavarian roots of Hitler's foreign and internal policy plans.

This anthology also includes essays on the impact of the Versailles Treaty and of Article 48 on Weimar developments, as well as studies of Hitler's ideas about the Western powers and of the legacy of 1918 for National Socialism. Especially well documented is Robin Lenman's article on "Julius Streicher and the Origins of the NSDAP in Nuremberg, 1918-1923." Careful archival research shapes this detailed narrative of internal Nazi party strife on the eve of the Munich putsch. Jill McIntyre's stimulating essay, which also relies on extensive primary sources, separates Nazi rhetoric from the reality of the German woman's participation in professional life during the 1930s. She concludes that despite the Third Reich's initial ideological antagonism toward professional women, "the net result of the decade was one favourable to women in the professional occupations" (pp. 212-13).

One comes away from this book refreshed by the various brief explorations of novel approaches and new evidence, along with the clarification of prevailing theses. Yet one regrets the lack of a substantial, interpretive introductory essay that would attempt to assess the larger meanings and broader implications of the diverse articles in this volume. Despite the disparity in essay topics, common themes and conflicting or overlapping interpretations might have been compared, analyzed, or synthesized.

A Herculean but disappointing attempt at synthesis characterizes Horst von Maltitz's *The Evolution of Hitler's Germany*. The book promises much but produces little that scholars will find new or fresh. Intended for a popular audience, two-thirds of this massive volume examines Nazi ideology. Except for original research in Hitler's writings, the author essentially summarizes or excerpts secondary sources. Maltitz, a lawyer by profession, has read widely and buttresses his arguments with frequent references to leading historians and psychologists. The main merits of this work lie in the author's clear, journalistic writing style (supplemented by trenchant

quotes) and his willingness to take seriously the significance and sources of Nazi ideology.

Disconcerting, however, is Maltitz's disproportionate stress on "Jewish destructiveness" in the Weimar era: "The intellectual German Jews were almost exclusively critics rather than builders, and this at a time when the tender flower of the Weimar Republic desperately needed whatever constructive care and help it could get from any source" (p. 152). Maltitz concludes that "paradoxically it is the absence of a liberal Jewish intelligentsia which may contribute to the stability of the German democracy today, in contrast to the Weimar Republic" (p. 457). Other shortcomings of the book include its uneven, episodic, and disjointed organization and character. While claiming that Nazi ideology "centered around" *Lebensraum* rather than anti-Semitism (pp. 180-81), Maltitz devotes ten pages to the former and over one hundred pages to the latter. The limitations of the author's impressionistic intellectual history are evident in his chapters on ideology, which are too often superficial summaries rather than probing analyses.

Moreover, Maltitz fails to place Nazi ideology in the larger European framework of fascism. Potentially misleading is his claim that "the great bulk of the ideology was uniquely German and had no counterpart elsewhere" (p. 271). Unfortunately the author totally overlooks Ernst Nolte's seminal study, *Three Faces of Fascism* (1963, 1965). These criticisms should not, however, detract from the often perceptive discussion of Hitler's personality, including his "religiousness." Occasionally Maltitz's suggestions and speculations invite interest in areas open to continuing scholarly treatment.

RUTH ZERNER

Herbert H. Lehman College,  
City University of New York

LUDWIG VOLK. *Das Reichskonkordat vom 20. Juli 1933: Von den Ansätzen in der Weimarer Republik bis zur Ratifizierung am 10. September 1933.* (Veröffentlichungen der Kommission für Zeitgeschichte bei der Katholischen Akademie in Bayern. Series B: Forschungen, volume 5.) Mainz: Mathias-Grünwald Verlag. 1972. Pp. xxvii, 266. DM 48.

In July 1933 Hitler's Germany signed a concordat with the Vatican that gave prestige to

the new Nazi regime and offered concessions to the Holy See that no Weimar government had given. The reasons of both parties entering into this agreement have been debated since its inception. Volk has now presented the background of this treaty based upon documentation drawn from both Church and state archives in order to show the motives of the participants and how they interacted upon one another. The volume is another in the series published by the Kommission für Zeitgeschichte of the Catholic Academy in Bavaria, which has already published documentary collections about the concordat.

After summarizing the historical debate about the concordat, Volk describes the attempts to reach a Church-state agreement during the Weimar era and the negotiations for the Bavarian and Prussian concordats, and he explains the importance of the school question in German politics for German-Vatican relations. Volk concludes that technical obstacles and political opposition to a German-Vatican treaty during the Weimar period made the possibility of its realization very unfavorable during that time.

In 1933 the Nazis made the first suggestion of once more taking up the question of a concordat. Volk clearly shows the role of participants such as Franz von Papen, Ludwig Kaas, and Eugenio Pacelli in the various negotiating stages as well as the attitude and influence of the German hierarchy upon both parties. Taking the reader through all the negotiations up to and including the signing and ratification of the treaty, the author contends that the Nazis' primary goal in initiating the talks was to induce the Vatican to depoliticize the clergy, but when the Center party dissolved itself in 1933, which the Nazis had not expected at that moment, only then did the secondary motive for a concordat, that of gaining prestige for the regime, gain in importance. Volk says that the Vatican, on the other hand, fully appreciated the evil in nazism but had no other choice than to sign the concordat to preserve some safeguards for their schools and social organizations that the Nazis were sure to abolish. With an internationally legal framework in which to work, Rome hoped to fight back, since Hitler's guarantees within Germany had proved doubtful at best.



Volk's work is thorough, based on detailed research, with many quotations in the footnotes, including some from the all but inaccessible Vatican Archives of the period. One final point might be added. Despite Volk's description of the Vatican's intentions, one still might like a further discussion about whether the Vatican, or any other power for that matter, saw the complete moral and diplomatic consequences of signing a treaty with Hitler.

STEWART A. STEHLIN  
New York University

WILLIAM CARR. *Arms, Autarky and Aggression: A Study in German Foreign Policy, 1933-1939.* (Foundations of Modern History Series) New York: W. W. Norton and Company. 1973. Pp. vi, 136. \$7.00.

Studies of Nazi foreign policy seem to follow clearly recognizable patterns. Until the mid-sixties they tended to focus either on Germany's relations with the other major European powers or on the narrative account of the main and familiar way stations on the road to war, from the Anglo-German Naval Agreement through Hitler's march into Prague. More recently the internal rivalries between National Socialist party organizations and the *Auswärtiges Amt* have received monographic attention, as has the role of Nazi ideology in the conduct of Hitler's foreign policy. Carr, the author of two previous books on nineteenth-century German history, is not only aware of these trends but has based his slim volume rather heavily on secondary works that exemplify them. For his discussion of Hitler's ideology Carr relies very much on the German edition of Eberhard Jäckel's *Hitlers Weltanschauung* (1969), while Carr's description of the diplomatic developments between the wars seems to owe much to Gerhard L. Weinberg's *The Foreign Policy of Hitler's Germany* (1970). This procedure might be quite legitimate in itself, considering that the title of the study carries the promise of examining Hitler's foreign policy in the light of Nazi economic policy and rearmament. That promise, however, remains largely unfulfilled.

Carr's basic argument and thesis is as logical and simple as it sounds convincing. Given Hitler's ideology or *Weltanschauung*, with its twin goals of racial purity and territorial expansion and his belief that war was not only in-

evitable but salutary, it does not seem surprising that he should have deduced the need for German rearmament from his ideological premises. Since even the Führer could not overlook the fact that in the twentieth century an effective army needs an industrial and economic base of considerable strength and independence from foreign interference, the connection between ideology, autarky, and rearmament for aggression seems to establish itself almost self-evidently. Unfortunately the author never presents an account that demonstrates in convincing historical detail his logically cogent thesis, nor does he achieve an integration of Hitler's foreign policy with his economic and military policies.

In part this is because of Carr's attempt to juggle too many balls at once and to do so in a space too narrowly confined. Thus he not only tries to integrate narratively the stories of rearmament and the drive for economic autarky with the development of foreign policy; he also endeavors to overcome what he sees as the traditional "primacy of foreign policy" (p. 1) approach by emphasizing the interplay and mutual dependence of external or foreign policy with internal or military and economic factors. The weakness of this book also stems in part from the author's indecision about his audience. The bulk of the material recounted is so well known that its presentation can profit only the beginning student. However, the assumptions and conclusions of at least some of the secondary materials used in this study are by no means universally accepted; their presentation and discussion would thus seem to be most fruitfully directed at the specialist. In sum, this brief book falls between too many stools to live up to its title or to the promise and potential of its thesis. It is hoped that another attempt will be made to demonstrate the validity of the core propositions of this work, but that it be done on the basis of primary sources and within a scope adequate to the magnitude of the task.

HERBERT A. ARNOLD  
Wesleyan University

ERNA LESKY and ADAM WANDRUSZKA, editors.  
*Gerard van Swieten und seine Zeit: Internationales Symposium veranstaltet von der Universität Wien im Institut für Geschichte der*

*Medizin*, 8.-10. Mai 1972. (Studien zur Geschichte der Universität Wien. number 8.) Vienna: Verlag Hermann Böhlaus Nachf. 1973. Pp. 194. DM 54.

This volume originated from a symposium held to commemorate the two hundredth anniversary of Van Swieten's death. The symposium focused on his significance for the cultural and scientific life of the Habsburg monarchy during the Enlightenment. Of the eleven papers, four deal with "his times" more than with Van Swieten himself and a fifth concerns his son, Gottfried. For publication the editors have added a significant section of documents and an extensive bibliography.

As Erna Lesky indicates, we lack a thorough study of Van Swieten, in part because we lack sources. This collection illustrates the painstaking process of reconstruction that consequently confronts historians. It is not surprising that we do not obtain a coherent accounting of his life and work, but a consistent view of Van Swieten as a reformer emerges. Lesky provides the critical perspective: that everything Van Swieten did in his public life had roots in his deep sense of social accountability, which in turn derived from the social sense of the Leiden Catholic community. Van Swieten was forty-five when he moved to Vienna, and he did not readily leave his homeland; he was not then rejecting his past. G. A. Lindeboom aptly remarks that the Leiden period is the *andante* to the symphony of his career. Our knowledge of those early years is still sparse, although his correspondence with Antonio Nunes Ribeiro Sanches gives us crucial insights into the beginning of Van Swieten's career in Vienna.

Van Swieten's medical reforms (Christian Probst), his educational reforms (Kalman Benda), and his views on censorship (Grete Klingenstein) reflect the essential unity of his career. To be sure, Lesky and Klingenstein stress that Van Swieten was no Jansenist, while Lindeboom, more judiciously, admits some Jansenist influence on his youth. And Klingenstein sees Van Swieten more distinctly as a man of the Enlightenment than as the Catholic Leidener. But the images remain congruent: as reformer, Van Swieten had a vision of the world even if the definition of that vision and its relationship to the particulars of his own life continue to elude us.

Ultimately this volume serves best to underline and clarify aspects of our understanding of Van Swieten rather than to provide us with striking new insights or a balanced introduction to his life and times.

WILLIAM J. MCGILL

Washington and Jefferson College

ADRIAN LYTTTELTON. *The Seizure of Power: Fascism in Italy, 1919-1929*. New York: Charles Scribner's Sons. 1973. Pp. 544. \$17.50.

NINO VALERI. *Tradizione liberale e fascismo*. (Quaderni di storia.) Florence: Felice Le Monnier. 1972. Pp. viii, 228. L. 2,800.

Lyttelton's impressive book invites comparison with Karl Dietrich Bracher's *Die nationalsozialistische Machtergreifung* (1962) and the first half of Bracher's *The German Dictatorship* (1970), which summarizes his views about the Nazi seizure and consolidation of power. According to Bracher "the National Socialist takeover and the organization of the Third Reich essentially was the social revolution of the professionally handicapped or unsuccessful petty bourgeois with military training or tendencies, who harbored great resentments against the establishments of both the Left and the Right. Hitler was their perfect embodiment" (*The German Dictatorship*, p. 276). Lyttelton comes to essentially the same kind of conclusion concerning Fascist Italy and Mussolini. But whereas Bracher tries to explain Hitler, Lyttelton's frequent labeling of Mussolini's behavior as "pragmatism" or "realism" begs the question of the Duce's ideas and motivations. On the other hand Lyttelton is more thorough than Bracher in analyzing those forces that the Fascist dictators had to placate or eliminate, particularly big business, the other political parties, and nonconformists within their own party. The Nazi and Fascist regimes created police states, but, as Lyttelton rightly notes, "in Germany, the police administration fell into the hands of an ideologically inspired elite, originating in the ranks of the party. In Italy, it remained under the control of trained bureaucrats. . . . The police, under Bocchini's efficient management, largely absorbed the repressive functions of the party and the squads" (p. 297).

The main differences between Bracher's and Lyttelton's accounts reflect the slower and

more halting pace of the seizure of power in Italy. Whereas in Germany the party dissidents were crushed and the rest of society was brought under Nazi control within a year and a half after Hitler became head of the government, it took Mussolini six and a half years to consolidate his regime. Nevertheless the descriptions of the regime's economic policies and the fate of the Fascist unions in Lyttelton's chapter on "The Fascist Economy" are not convincingly related to the political struggle. The same is true for most of the material in the chapter on "Ideology and Culture." The chapter on "Propaganda and Education," particularly the section on control of the press, is certainly important for Lyttelton's main theme, but the concluding chapter, "The Regime," is sketchy on the negotiations leading to the Lateran Accords with the Vatican, which presumably justify the 1929 terminal date of the book.

Despite its occasional *longueurs* and allusiveness Lyttelton's book is the best account in any language of the Fascist seizure of power and a major contribution to historical scholarship by any standards. The section on the crisis in the party and the Militia, which followed the Matteotti crisis, is excellent. And Lyttelton gives the most convincing explanation available for the survival of Roberto Farinacci (in contrast to Ernst Röhm in Nazi Germany) as a gadfly of the regime. He also provides new insights concerning the alliances of rival Fascist factions with or against rival banking groups, especially the Giolittian Banca Commerciale. Although all his facts are well documented some of his speculations are based on partisan secondary sources. But Lyttelton's greatest achievement is the way he weaves together the complex interplay of conflicting political groups, the economic situation, and Mussolini's concern about foreign opinion.

Valeri's book deals with the historiography of fascism. Unlike Costanzo Casucci, Renzo De Felice, and A. William Salomone in their anthologies, Valeri incorporates the main ideas of diverse critics into his own analysis. Starting with H. Stuart Hughes, Federico Chabod, and Christopher Seton-Watson, Valeri reviews a number of works of the 1960s that try to link fascism with elements of Italian national character and behavior. In chapter 3 he discusses the published letters, notebooks, and memoirs

of some of the leading Italian politicians and critics of the period immediately following the First World War. Chapters 4 and 5 are devoted to various interpretations of the European character of fascism and the totalitarian ingredient in contemporary civilization. In chapter 6 Valeri discusses polemics and critical works about the last years of Italian fascism. He also reviews certain popular books—Luigi Barzini's *The Italians* (1964) and Antonio Pellicani's *Il filo nero* (1968)—which try to give "authoritative" explanations that professional historians are unable to provide concerning the "whys" of fascism for ordinary people who are conditioned to expect instant analysis from the mass media. But unlike De Felice in his *Le interpretazioni del fascismo* (1969) Valeri completely neglects the contributions of political sociologists to our understanding of the nature of fascism.

In chapter 7, "The Problem of Responsibility," Valeri argues that a true understanding of history must be distinguished from polemical interpretations of what happened in the past in terms of present political, practical, or ideological concerns. He willingly accepts the label "bourgeois liberal" for this view and criticizes some of its most eminent older spokesmen, such as Croce and Meinecke, for deviating from it in saying that fascism was caused by gangs of adventurers taking advantage of an extraordinary and temporary crisis situation. Valeri insists on "the objective impossibility of isolating, in that chaotic situation, one fact or group of facts and attitudes and making it the sole or dominant explanation of the tragedy" (p. 220). Like Ernst Nolte, Valeri believes that fascism was a basic phenomenon in European history in the period after 1919.

Valeri's book is the swansong of a liberal historian who has come to believe that conditions rather than men determine the course of history. Valeri is undoubtedly correct in warning us against seeking the one true explanation. But surely the professional historian must try to explain how the conditions that make things happen came about in the first place. Lyttelton, a much younger liberal, does this successfully by emphasizing the role of men as agents of political forces.

EDWARD R. TANNENBAUM  
New York University

GIORGIO BOCCA. *Palmito Togliatti*. Bari: Editori Laterza. 1973. Pp. 753.

He was cold, cutting, sometimes cruel, a bit of a snob, suspicious of zeal, alert to the imperatives of power and the prerogatives of rank, an authoritarian, a dangerous enemy, and an unreliable friend. His comrades called him "the Torinese professor" and found him hard to like. Celebrated for his double-breasted suits, his shrewdness, and his quiet, courteous, almost pedantic manner he cut a strange figure as the leader of the largest revolutionary party in the West. His mastery of maneuver and his tactical flexibility—what the Italians call in a splendid and untranslatable word "*spreguidicatezza*"—called to mind men of state like Machiavelli, Cavour, or Giolitti rather than idealists like Garibaldi and Mazzini, whose vision of what *might be* often obstructed their ability to see what *was*. Like so many of his countrymen, Togliatti prided himself on his ability to read reality rather than to change it. And yet few men shaped Italian postwar politics more than he did—perhaps only the Agnelli and the Popes.

Giorgio Bocca has told his story on the basis of published sources and a large number of interviews with those who were close to Togliatti at various times: his family, his friends, his lieutenants, his rivals, and his victims. The result is a fascinating book. Bocca knows how to sift the evidence, search out the facts, and shatter the legends created by party hacks and hagiographers without losing sight of Togliatti's considerable achievement. He judges and sometimes he judges harshly, but his judgments have the ring of truth and the virtue of compassion. It is naive, Bocca says, to think that Togliatti was not a convinced Stalinist. Indeed he rose to power in the first place because he was willing to implement Stalinist policies that other Italian Communist leaders rejected as mistaken or absurd. Bocca's chapter on Stalin's Comintern, his best, evokes the sinister atmosphere of the Hotel Lux during the era of the Purges and explains why even the best of men fell prey to the fascination of Stalin's power, demonstrating in sad detail that if Togliatti survived it was because his dominating characteristics were caution rather than courage, prudence rather than the pride that comes from un-

yielding integrity. Nor, Bocca goes on to warn us, should we suppose that Togliatti had undergone a sudden conversion to the values of pluralist democracy when he returned to Italy in 1944 and shocked many of his supporters by calling for a government of national union. He simply acknowledged that given Italy's situation there was no other way to power than through collaboration with other antifascist and sometimes recently fascist elements. Finally, Bocca shows that while it is true that Togliatti had a great respect for humanistic culture and an attitude of condescension toward those who did not possess it he never viewed it as an end in itself, but only as a means for reshaping Italian social reality. When it served his purpose he did not hesitate to bend the truth, suppress an embarrassing fact, or rewrite history. Togliatti's later liberalism was forced upon him by events.

Bocca is quick to speculate when documents are missing or when his informants have omitted important facts. He may have made mistakes and sinned through an excess of demystifying zeal. But whatever the errors of detail, he has written in the spirit of Togliatti himself who advised his comrades when evaluating Stalin to seek to understand the circumstances in which he had operated. "Believe me," he once said, "in great part what he did could not have been done differently." Bocca comes to a similar conclusion about Togliatti and views the PCI as a progressive force in postwar Italian life. Because of it, he argues, Italian culture is livelier, Italian democracy is healthier, and Italian capitalism is more humane. It is hard to disagree.

ROBERT WOHL  
University of California,  
Los Angeles

STEPHEN GAZI. *A History of Croatia*. New York: Philosophical Library. 1973. Pp. xv, 362, 6 maps. \$11.95.

This is a simple narrative of the political and military history of Croatia to the outbreak of World War II. A concluding chapter summarizes events since then in ten pages. Another chapter, essentially a catalog of names and titles, sums up Croatian contributions to literature and the arts. It is discouraging to find that

historical writing that is so antiquated in approach and so lame in execution can still find a publisher. Most of this book is merely a chronological recital, often a detailed listing, of dynastic changes, military campaigns, diplomatic alliances, territorial adjustments, and electoral results. A bit of superficial analysis is occasionally given in the concluding paragraphs of a chapter. The complicated multinational context of Croatian history is left confusing. The author's writing style is dull and studded with trite and outmoded usages (such as "viz."). The author has his own highly personal way of spelling non-Croatian names (for example, Kramarz, Husit Wars, Olumec, Sobiesky, Safaryk), and this is compounded by the almost complete absence of necessary diacritical marks throughout the book. In his introduction Gazi lists a number of published works on Croatian history that he has consulted. No additional bibliography or notes are included. Reference matter consists of a two-page chronological outline and six pages of crude maps. The only positive thing to be said of this work is that its author is not a Croatian chauvinist. Although his preferences for Catholics, Croats, and the peasant leader Radić are quite evident, they, like the traditional anti-Serb and anti-Magyar biases, are kept in reasonable check. A feeble vanity publication such as this contributes absolutely nothing to the pressing needs of American scholars and teachers involved with East Central European history.

JOSEPH FREDERICK ZACEK  
State University of New York,  
Albany

F. L. CARSTEN. *Revolution in Central Europe, 1918-1919*. Berkeley and Los Angeles: University of California Press. 1972. Pp. 360. \$11.95.

In many ways this work illustrates the historian's craft at its best. A talented and established scholar has addressed himself to a familiar period and subject, found new sources and explored them with the thoroughness of the most earnest Ph.D. candidate, and reinterpreted the events of the time. His stage is Germany, the German provinces of Austria, and to a lesser extent Bohemia and Hungary; the time period is approximately the year that followed the

defeat and dissolution of the armies and governments of the Central Powers. In a broad sense the drama is one of many social forces in contention with each other in a chaotic situation, but the author throws his spotlight on the activities of the soldiers' and workers' councils, which sprang up in so many cities and towns. As primary sources he has consulted the municipal records of Berlin, Vienna, and scores of provincial centers, supplementing them with memoirs and other material, with the result that he can show us, province by province, just what kind of revolution, counter-revolution, or nonrevolution was taking place.

Two striking conclusions about the councils come through. One is the degree to which they played a moderate and constructive role. Far from being revolutionary soviets they did a great deal to hold the fabric of society together, helping to maintain security, to distribute available food supplies equitably, to look after social welfare, and to enable local governments to function. The second notable point is that the councils were limited in their potential by the moods, traditions, and continuing political patterns of their respective localities. In Austria, bearing the imprint of the left wing of Social Democracy, they had no firm roots outside Vienna and Lower Austria, and even there they lacked the support of the Social Democratic leadership that was essential to their exercise of real political power. In Berlin and elsewhere in Germany they had a major influence on the composition of new governments, but unfortunately, in the author's view, they had little staying power because of their own internal divisions, the distrust of the middle class and the trade unions, and the attacks of militants on the extreme left and right. Carsten gives us a new interpretation of Kurt Eisner and his attempt to carry out a revolution in Bavaria based on strengthened workers', peasants', and soldiers' councils, and of the Councils Republic, which was proclaimed after his murder and soon fell into the hands of the Communists.

In his story and his conclusions the author interprets the events of 1919 as a series of lost opportunities for basic social change, with the result that democracy did not find a secure home in Central Europe. Thus the revolution "failed." Here the thesis runs into problems of definition, and, as he says, many may disagree.

But there is no denying that his is a fresh insight ably presented and backed by solid research.

JOHN C. CAMPBELL  
*Council on Foreign Relations*

PETER F. SUGAR, editor. *Native Fascism in the Successor States, 1918-1945*. Santa Barbara, Calif.: ABC-CLIO, 1971. Pp. 166. Cloth \$9.50, paper \$4.50.

This collective work owes its importance primarily to the fact that it is one of the very few treatments of East European history that attempts to be comparative. The authors were asked "to investigate what features of the fascism manifested in Eastern Europe differed from those observable elsewhere and what common characteristics these local manifestations had that could be tied, at least to some extent, to their common Habsburg inheritance" (p. 147). The fascist movement in each of six countries, furthermore, was subjected to analysis twice, once by a citizen of the country concerned and a second time by an American specialist. (Five of the Americans, however, were born in Eastern Europe.) The papers were first presented to a conference held in Seattle and organized by the University of Washington in 1966.

Yet the juxtaposition of authorities from two continents did not produce much in the way of contrast or controversy. (Nor is there much direct comparison within the area, not to speak of matching East European with fascist movements elsewhere.) Rather, the principal difference in interpretation that emerges cuts across the lines of citizenship and divides the authors into those who think fascism was not a very important force in their countries and those who think it was.

Thus, on the one hand, R. John Rath of Rice University asks whether it is fair to indict seven million Austrians for not resisting the might of the Third Reich when the Western great powers stood idly by, while Jan Havránek of Charles University argues that neither Hlinka's nor Henlein's party was "truly fascistic" until after 1939 (p. 52). On the other hand, George Barany (Denver) speaks of a disease that "must have penetrated all strata of Hungarian society fairly deeply by the time of the Second World War" (p. 77), and Stephen Fischer-

Galati (Colorado) portrays the Iron Guard as the dominant force in Romanian politics in the 1930s, explaining that the Guardist vote in the election of 1937 was in fact significantly greater than that officially reported.

This division is in part confounded with another, which is the line between countries (or peoples) that could expect assistance from the Third Reich in the realization of state interests or of ethnic ambitions (the Hungarians or the Croats) and those who could not, for example, the Poles and the Czechs. Indeed, the first fascist-like parties arose among the German populations of Bohemia in the 1890s as a popular reaction to the advance of Czech nationalism. By the 1920s fascism appeared to be a single movement with Bohemian, Austrian, and German branches. Those of our authors who regard fascism as of little importance are also inclined to view it as Nazi-imposed or Nazi-supported, while those who think to the contrary see it as a nativist phenomenon with a life of its own.

But in part, at least, the division reflects a definitional problem, reference to which presents itself in almost every chapter. The editor asserts in his conclusion that the authors have achieved a tacit definition of fascism, one related to the totalitarian conception of Hannah Arendt, whereas the author of the introduction, Lyman H. Legters, maintains that "the fullest value of these studies can only be extracted by relating them to the as yet only incipient effort to refine the concept of fascism" (p. 9). I agree with Legters, and I would go so far as to say that the unexpected outcropping of the definitional problem is one of the more important results of this application of the comparative approach.

*Native Fascism* is also useful because it provides convenient summaries, often based on sources in generally inaccessible languages, and because it illuminates some problems common to the Habsburg successor states.

R. V. BURKS  
*Wayne State University*

VICTOR S. MAMATEY and RADOMÍR LUŽA, editors. *A History of the Czechoslovak Republic, 1918-1948*. Princeton: Princeton University Press, 1973. Pp. xi, 534. \$22.50.

Few small countries in the twentieth century

have played a more important part in world affairs than Czechoslovakia, at one time the world's eighth industrial power and the only parliamentary democracy in Central and Eastern Europe. Continuing efforts by Czechs and Slovaks to build a democratic society have commanded worldwide attention, especially in 1918 and again in 1938, 1948, and 1968 as domestic conflict in Czechoslovakia led to an international crisis. To help inform a renewed Western interest in Czechoslovakia, a team of fourteen American and European scholars has taken advantage of new evidence and a greater perspective in time to produce a balanced and objective history of the Czechoslovak Republic in a European context. This work, ably edited by Victor S. Mamatey and Radomír Luža, and the equally recent study by Věra Olivová, *The Doomed Democracy: Czechoslovakia in a Disrupted Europe 1918-1938* (1972), are the most comprehensive and informed surveys of modern Czechoslovakia to date in English. Of these two works, the former is the more detailed and better documented and alone covers the critical and still controversial decade from Munich to February 1948.

Each topical chapter in *A History of the Czechoslovak Republic* is written by a specialist, and the work as a whole is characterized by a remarkably uniform excellence in style and scholarship. Every author has utilized available archival sources and contemporary Czechoslovak as well as Western scholarship. Two chapters by Victor S. Mamatey comprehensively survey the founding of the Republic and "the development of Czechoslovak democracy from 1920 to 1938." Václav Beneš presents the most thorough and balanced account in English of the postwar consolidation of the Republic during two years of domestic and international crisis. Piotr Wandycz cogently and critically evaluates all facets of Czechoslovak foreign policy from 1918 to 1938 and very fairly assesses its principal architect, Eduard Beneš. Keith Eubank reviews the Munich crisis from beginning to end with emphasis upon its international as opposed to its domestic aspects. J. W. Bruegel's sympathetic and objective discussion of "the Germans in pre-war Czechoslovakia" and Zora P. Pryor's thorough survey of "Czechoslovak economic development in the inter-war period" complete the series of seven articles on

the first twenty years of Czechoslovak independence.

Six chapters cover the period of Nazi occupation from 1939 to 1945. Jörg K. Hoensch presents the most analytical and objective survey of the quasi-independent Slovak Republic yet to appear in English. Theodor Prochazka concentrates upon the short-lived and heretofore largely neglected Second Czechoslovak Republic from Munich until its abolition by the Nazis. Balanced essays by Gotthold Rhode, Edward Taborsky, Radomír Luža, and Anna Josko examine respectively the Protectorate of Bohemia and Moravia, the Czechoslovak government in exile, and the Czech and Slovak resistance movements.

Political and economic problems in the post-war era and the expulsion of the Germans are surveyed in three chapters by Radomír Luža, Jan Michal, and Ludvík Němec. A concise epilogue by Victor S. Mamatey reviews the principal themes of the book and helps place each chapter in perspective. Readers wishing to undertake further study of modern Czechoslovakia will appreciate the forty-two page annotated bibliography by Radomír Luža; his judgments are fair and to the point, excepting several instances in which he has too negatively evaluated recent publications by Czech and Slovak historians.

*A History of the Czechoslovak Republic* is enhanced by a detailed index and is characterized throughout by clear prose, ample documentation, and very few factual or typographical errors. In general, its authors discuss domestic politics, foreign affairs, and economic development more thoroughly than other topics and no more than sketchily examine arts and letters, education, religion, and the institutional and class structure of Czechoslovak society. A short review of their fine work cannot adequately comment upon each chapter. Suffice it to say in conclusion that every scholar interested in modern Europe will find their book an invaluable reference work and a stimulating and informative narrative.

BRUCE M. GARVER  
Yale University

J. W. BRUEGEL. *Czechoslovakia before Munich: The German Minority Problem and British Ap-*

*peasement Policy*. New York: Cambridge University Press. 1973. Pp. xiii, 334. \$14.95.

The bulk of Mr. Bruegel's book deals with the Munich crisis of 1938 and its immediate antecedents. The book also contains chapters on the formative years of Czechoslovakia and treats in relatively great detail Czechoslovak-Reich relations from 1918 to the rise of Hitler as well as the participation of German parties in Prague coalition governments. Bruegel notes that the Weimar Republic upheld no territorial claims against Czechoslovakia and that its diplomats even encouraged Sudeten-German politicians to cooperate with the Czechs. In the fact that from 1925 to 1938 it was indeed possible for Germans to be partners in Czechoslovak governments Bruegel sees proof that Germans were gradually abandoning their original negative position toward the state and that the German problem was being solved. In strong terms the author blames Britain's statesmen and diplomats for allowing Hitler to destroy Czechoslovakia and he attributes their actions to their stupidity, dishonesty, and callousness.

It is indeed true that for a considerable time a clear majority of the German electorate supported activist parties. By the spring of 1938, however, an equally clear majority voted for the Sudeten-German party, which was not loyal to the republic. Activists left the government, and their parties, with the exception of the Social Democrats (who also lost votes), were decimated. It is also true, of course, and Mr. Bruegel notes this, that even in 1938 about a third of the German votes were cast for German opponents of the Nazis. Does it mean, however, that they were also cast, as he alleges, in favor of the Prague regime? The answer has to be in the negative. Those Germans who recognized Czechoslovakia as a political fact did not extend their recognition to the conception of Czechoslovakia as a national state of the Czechs (and one in which Germans were a minority), a position Mr. Bruegel rightly attributes to the country's "powerful and generally chauvinistic bureaucracy." It is impossible to attribute the rise of the pro-Hitler element in Czechoslovakia solely to the intrigues or attraction of Hitler's Reich and to ignore the fact that German activism in Czechoslovakia failed to win those concessions that all Germans considered necessary.

It is even more wrong to ignore the fact that British policy makers considered the system under which the Czechoslovaks were a "nation of the state" and Germans a national minority untenable in the long run, Hitler or no Hitler. The earlier German version of this book (*Tschechen und Deutsche, 1918-1939* [1967]) devoted a great deal more attention to domestic affairs and thus made it easier to understand not only the complexity of Czechoslovakia's internal situation but also its influence on the policies of foreign powers, such as Britain. Hitler, whose plans regarding Czechoslovakia were not limited to territorial correction, but included eventual elimination of the Czech people from Central Europe, was able to take advantage of Sudeten-German grievances, whose theoretical validity even Prague recognized. The Sudeten Germans were willing in 1938 to identify their cause with the goals and policies of Hitler, thus making it impossible to preserve a reformed Czechoslovakia. In 1945 they in turn were treated as a part of the Nazi legacy, and their national existence in Czechoslovakia was terminated together with that of the Third Reich.

ROMAN SZPORLUK  
University of Michigan,  
Ann Arbor

I. I. POP. *Chekhoslovatsko-ungerskie otnosheniia (1935-1939)* [Czechoslovak-Hungarian Relations (1935-1939)]. (Akademiia Nauk SSSR, Institut Slavianovedeniia i Balkanistiki.) Moscow: Izdatel'stvo "Nauka." 1972. Pp. 245.

This is an interesting and valuable contribution to the history of Czechoslovak-Hungarian relations during 1935-39. The book is based mainly on published documents and secondary sources; however, some Czech archival materials are used, but outside of the TASS collection and some manuscripts no Soviet archival documents are utilized. There is no bibliography, and some important Hungarian, Czech, and Polish sources were not consulted. In addition to the skillful use of documents found in the *Diplomáciai iratok Magyarország külpolitikájához 1936-1945*, I. I. Pop should have also consulted *Magyarország és a második világháború. Titkos diplomáciai okmányok a háború előzményeihez és történetéhez* (1959), which contains some of Milos Kozma's papers relating



to the Carpathian Ukraine—or at least he might have utilized its Russian translations: *Vengriia i vtoraiia mirovaia voina; sekretnye diplomaticheskie dokumenty iz istorii kanuna i perioda voiny* (1962). Apparently among the Czech archival material he has missed "*Dokumenty z jednání o územních změnách Československé republiky, 19.IX.—30.IX. 1938.*" Its third volume of fifty-seven pages covers the Czech negotiations with Hungary. Polish publications are better utilized, but such important works as Stanisławska's *Polska a Monachium* (1967) and Lipski's *Diplomat in Berlin* (1968) should not have been overlooked. Furthermore a greater use of Western documents also would have been beneficial.

The most interesting and valuable parts of the book are those discussing the Hodža Plan, whereby it is shown that since 1936 Prague periodically tried to stabilize the Danubian region through closer economic and political relations with Hungary and Austria. Due largely, however, to the inflexible position of Budapest and the Hungarian insistence on territorial changes at the expense of Czechoslovakia all Czech attempts to improve relations with Hungary were without success. Failing to find strong supporters abroad for revisionist policies Budapest found itself isolated, and in the end Hungary was left totally dependent upon the favors of the Third Reich.

Specialists will find little that is really new here, but the book is a valuable contribution to historiography—and indeed it represents a new gender in Soviet historical writing! Naturally its presentation falls within the general Marxist-Leninist framework, but it is refreshingly void of the usual abundance of ideological jargon. The diplomatic entanglements are generally presented with a measure of objectivity, and the clarity of presentation and style are most captivating. Thus in a final analysis both specialists and students will find the book rewarding.

C. M. NOWAK  
Bridgewater State College

WALTER R. ROBERTS. *Tito, Mihailović and the Allies, 1941-1945*. New Brunswick: Rutgers University Press. 1973. Pp. xv, 406. \$15.00.

The author, an associate director of the United

States Information Agency, has made an outstanding contribution to historical scholarship. His carefully researched book is an objective work on a complex and controversial subject and represents a turning point in the historiography of Balkan resistance movements during World War II. It depicts a Yugoslavia overwhelmed by Nazi military might, partitioned, occupied, and soon the scene of a bitter civil war between two resistance movements. The author does a superb job of summing up the major developments and how they were influenced by the decisions of the major allies.

Roberts shows how the Big Three acted in support of what they conceived to be their short-run interests (winning the war) and against their long-term goals (ideologically preferred political systems). For Churchill (and Roosevelt through Churchill's urging), Tito's Communist-led partisans were killing the most Germans and therefore should be supported. Stalin, on the other hand, believed that Yugoslavia could help most by avoiding civil war, urging Tito to assist Mihailović and his *chetniks*, who were the first to raise the flag of resistance. Only when it was too late did Churchill express concern about the political implications of his earlier decisions.

Perhaps the book's greatest service is the light it throws on the leaders of the two resistance movements. Both wanted the Allies to win and assumed that they would. Tito's main concern was that he be in power at the end. Mihailović, on the other hand, wanted to prevent that from happening. He would have preferred to follow Western Allied instructions to European resistance movements to lie low until the signal of Allied landings. But Tito's actions forced him to fight against an imposed political solution, hoping that when the war ended the people would have an opportunity to determine their own destiny.

Roberts explodes the myth that Mihailović was a traitor and Tito a patriot. Each regarded the other as his principal enemy, and three of Tito's top associates (including Milovan Djilas) even put their position in writing during a negotiating session with German representatives. The Germans were happy to see Tito and Mihailović fight each other, but they viewed both as enemies of the Third Reich and offered the same reward for their capture.

Specialists may wish that the author had given more space to certain subjects, and they may find minor faults, but they cannot deny that this is a first-rate book on a subject that has produced so much bad history over the past twenty-five years.

ALEX N. DRAGNICH  
Vanderbilt University

IU. V. BROMLEI *et al.*, editors. *Slaviane i Rossiia: K 70-letiiu so dnia rozhdeniia S. A. Nikitina* [The Slavs and Russia: For the 70th Birthday of S. A. Nikitin]. (Akademiia Nauk SSSR, Institut Slavianovedeniia i Balkanistiki.) Moscow: Izdatel'stvo "Nauka." 1972. Pp. 304.

*Slaviane i Rossiia* is a *Festschrift* presented to the distinguished scholar of Russo-Balkan relations, S. A. Nikitin, on his seventieth birthday. If the *Festschrift* is generally a reviewer's nightmare the book under review is particularly so. It contains thirty-four items, some rather fragmentary, some quite substantial, on topics ranging from feudal rents in Dalmatian Croatia in the thirteenth and fourteenth centuries to the crisis over Trieste at the end of the Second World War. Only Professor Nikitin himself, perhaps, has the range to adequately criticize all the essays in *Slaviane i Rossiia*; I will confine myself to commenting on a few that seemed in one way or another noteworthy.

One of more interesting essays dealing with the earlier centuries of Russian and Balkan history is L. V. Cherepnin's discussion of grants of immunity and privilege among Russian and south Slavic princes in the fourteenth and fifteenth centuries. Cherepnin's most interesting point, which he merely poses and does not develop, pertains to the relationship between these grants and subsequent legal norms. The degree to which privileges and immunities subsequently acquire a more general legal application—or fail to do so—provides an excellent device for the analysis of the power relations between the prince and local aristocracies over time. It is a pity that Cherepnin did not discuss either the potential uses or the practical difficulties of this kind of analysis in more detail. In Russia, for instance, the number of codified legal sources for the period in question is so small that it would seem to make the kind of analysis that Cherepnin suggests very difficult.

The heart of *Slaviane i Rossiia*, as of Niki-

tin's published work, is the relations between Russia and the emerging Balkan nations in the nineteenth century; eighteen of the thirty-four contributions deal with some aspect of this massive topic. One of the best is a solid, judicious and well-grounded piece by I. S. Dostian, dealing with the attempts of Serbian nationalists to obtain Russian assistance in the founding of a Serbian state in the first years of the nineteenth century. K. L. Strukova's "On the Character of the Development of the European Provinces of Turkey in the First Three Quarters of the Nineteenth Century" contains valuable evidence of the backwardness of Macedonia. E. M. Shatokhina's essay on the socioeconomic condition of the Bulgarian peasantry in the 1860s is based on information accumulated by Russian diplomats in Turkey and now located in the Archive of the Foreign Policy of Russia (AVPR).

Perhaps the most substantial essay in *Slaviane i Rossiia*, although not surprising in its conclusions, is N. M. Druzhinin's "The Agrarian Reform of 1866 and Its Consequences." Using the statistical survey of state properties (1858) and a government compendium of 1888, surveying the activity of the Ministry of State Properties over the preceding fifty years, Druzhinin shows that the bulk of the former state peasants were no better able to deal with their chronic shortage of land and heavy financial exactions than were the former landlord's peasants, despite the modest advantage over the latter that they had enjoyed at the time of the reform of 1866.

Several of the essays in this volume deal with the mutual relations between Russian radicals and Balkan revolutionaries. The majority are informative, but several suffer from excessive abstraction and schematism, as does V. D. Konobeev's treatment of the agrarian program of the Bulgarian radical, Vasil Levski. Konobeev is fond of such deadening sentences as: "Having correctly defined the final, objective goals of the peasant struggle, Levski selected the only true path to their resolution—the popular (*narodnyi*) path, that is, the bourgeois-democratic revolution, which must resolve the task of the social liberation of the peasantry."

If Konobeev's Marxism is abstract and conventional, A. S. Beilis's treatment of American missionaries in Bulgaria ("From the History of Early American Penetration in Bulgaria") is

angry and confused to the point of utter incoherence. Beilis accuses American missionaries in general of "ideological aggression," but does not define the term further and goes on to assert that American missionary activities in Bulgaria (1850-80) merely cloaked "intelligence and espionage activities." Needless to say this broad claim is not supported by any real evidence. What really appears to enrage Konobeev is that American missionaries (in his view) were systematically trying to undermine Russian influence in Bulgaria. Although he certainly exaggerates the directedness and consistency of American efforts he is probably right. But any consistent Marxist should applaud their efforts; surely in the middle of the nineteenth century the ideas emanating from Robert College and American missionaries were more "progressive" than those espoused by the Russian government.

*Slaviane i Rossiia*, then, is a grab bag. There are a number of excellent articles. There is considerable information on a variety of historical points of Russian and Balkan history. But *Slaviane i Rossiia*, like most *Festschriften*, should be dipped into judiciously, rather than read cover to cover.

ABBOTT GLEASON  
Brown University

A. A. ZIMIN. *Rossiia na poroge novogo vremeni (Ocherki politicheskoi istorii Rossii pervoi treti XVI v.)* [Russia on the Threshold of the New Age: Essays on the Political History of Russia in the First Third of the 16th Century]. Moscow: Izdatel'stvo "Mysl'" 1972. Pp. 451.

This latest work by one of the leading historians in the USSR is a survey of the reign of Vasili III (1505-33). In recent years Soviet historians such as Zimin, Lur'e, Kazakova, and Klibanov have written on specific aspects of Vasili's reign, their major efforts being studies of the dynastic crisis prior to Vasili's accession, the question of the secularization of monastic estates, and heretical and rationalistic movements in the Church. These topics, in addition to discussions of internal politics and a heavy stress on foreign policy, constitute the bulk of Zimin's book.

Because Vasili ruled during the early adolescence of the Muscovite Empire historians might expect to find in this work chapters on

class structure, economic development, and the growth of the administrative apparatus. Yet except for a number of pages on the economy these subjects are not adequately treated. The fault, however, is not entirely Zimin's, for the historical data of the period (chronicles and publicists' literature) tells us little of these matters. We have, therefore, a very unbalanced account of Vasili's reign, one in which we do not even find a true biographical sketch of the ruler.

Zimin's work is based on a close study of the primary sources. But aside from some minor points that he disputes with other historians his overall interpretation is conventional: Vasili's efforts at territorial expansion and concentration of political authority in the monarchy represented a continuation of his father's policies. One novel point that Zimin should have dwelt upon is his argument that Vasili seriously contemplated leaving the throne to a Christianized Tatar prince and later to the man who married that prince's daughter. Considering Ivan IV's brief abdication to a Tatar prince in the 1570s one might speculate on Muscovy's image of the Chingisid dynasty in the sixteenth century. In terms of legitimacy it may well have been considered equal to the Riurikid. But Zimin rarely speculates on such a scale. In all his book is the best survey we can expect, considering the imbalance in the available data. Yet we shall remain frustrated in not knowing what actually went on in Moscow in the early sixteenth century.

THOMAS ESPER  
Case Western Reserve University

EFFIE AMBLER. *Russian Journalism and Politics, 1861-1881: The Career of Aleksei S. Suvorin*. Detroit: Wayne State University Press. 1972. Pp. 239. \$12.50.

B. I. ESIN. *Russkaia dorevoliutsionnaia gazeta, 1702-1917 gg.: Kratkii ocherk* [Russian Prerevolutionary Newspapers, 1702-1917: A Brief Essay]. Moscow: Izdatel'stvo Moskovskogo Universiteta. 1971. Pp. 86.

B. P. BALUEV. *Politicheskaia reaktiia 80-kh godov XIX veka i russkaia zhurnalistika* [The Political Reaction of the 1880s and Russian Journalism]. Moscow: Izdatel'stvo Moskovskogo Universiteta. 1971. Pp. 313.

While the peculiarities of prerevolutionary Rus-

sian journalism have inevitably received some attention outside the Soviet Union from specialists in literature and the history of ideas the role of the press in the political life of tsarist Russia has been given undeservedly little attention. Where it has been taken into account judgments about the significance of particular periodicals and journalists have all too often been taken at secondhand from tendentious treatments by Russians (for whom, both before and after 1917, it has been impossible to discuss this topic without a *parti pris*) or from a variety of unsatisfactory Western sources.

A welcome contribution to the inchoate Western effort in this field is Professor Ambler's substantial and lively study of A. S. Suvorin, a publicist and literary entrepreneur whose career spanned the period from the Great Reforms of the 1860s to the eve of the First World War and whose notably profitable abandonment of a moderate and eclectic liberalism in favor of the firmer ground of chauvinism and conservatism made him a byword for journalistic venality. It is the course of Suvorin's political development that forms the framework of Professor Ambler's study: the period under consideration is the last twenty years of the reign of Alexander II, during which many of Suvorin's compatriots described a similar path, from the civic euphoria of the early 1860s to the indifferentism and reaction of the early 1880s. The conflict between this tendency and the persistent oppositional traditions of the intelligentsia is nicely handled by Professor Ambler, with a valuable emphasis on the role that the organs of the periodical press filled as a surrogate for normal political parties. Unfortunately Suvorin is not an entirely satisfactory point of focus for the examination of these questions, for reasons that Professor Ambler herself acknowledges: first, because we have uncommonly little material with which to investigate the private thoughts behind Suvorin's publicistic persona (his published *Diary* concentrates on a period beyond the limits of Professor Ambler's work and is in any case, as recent Soviet scholarship indicates, textually suspect), and more importantly, because his political views were too miscellaneous and contingent to offer much beyond a case study in the art of adaptability. Suvorin's drift to the right gathered speed notably after he

acquired his own newspaper in 1876, and one suspects that his was the example that moved the then chief of the censorship to remark that he was glad to grant permission for known dissidents to open new periodicals, since while they might "play the liberal on someone else's paper, with their own they have to look out for number one."

Nonetheless Professor Ambler has done an excellent job of describing the political and intellectual currents that created the milieu in which Suvorin worked and the dynamics of the relationship between the press, the government, and the tiny educated minority that regarded itself as the representative of "public opinion." Equally valuable is her repeated reference to moments in the material development of the Russian press, a banal detail often ignored in Western treatments, but indispensable for giving a sense of proportion to any discussion of the politics of Russian journalism. The only serious reservation to suggest itself concerns matters of translation. Professor Ambler tends to use excessively literal translations in contexts that are clear enough to the Russian specialist, but may confuse historians from other disciplines who ought not to be put off by such an instructive book. Thus, for instance, the repeated use of the word "accusatory" to describe authors and works from the vein of Russian social criticism that combines both exposé and remonstrance is too terse. Equally odd-sounding is the frequent use of "idea-less" to translate the word used by all hues of the Russian left to condemn any writing not informed with high-minded civic principles. These (and numerous other) dictionary translations are, however, tolerable faults in an extremely useful book.

B. I. Esin's short study of the development of the newspaper press in Russia is (as, indeed, are all his works) an example of the best in Soviet history of journalism and should be read by every scholar working on the political and intellectual history of Imperial Russia. In the hands of a historian of Esin's caliber the tools of Marxism are extremely effective for the exposition of problems that must be considered if a work in this field is to have more than an anecdotal or a narrowly technical value: the development of newspapers in any country is inextricably bound up with questions

of economic and social changes and their political consequences, and the narrow focus that Soviet historians of journalism bring to bear on these questions supplies a solid base from which scholars free of ideological imperatives may range with much greater variety and penetration, as has Professor Ambler. Another instructive point that arises from a consecutive reading of these two books is the reminder that the curious notions that many Russians have about the purpose of journalism have changed little since Suvorin's day. Despite Esin's sobriety, thoroughness, and reliability in matters of fact it is always evident that he cannot begin to sympathize with a system of journalism in which diversity, competition, and spontaneity are values of the first importance.

This may be said even more emphatically of B. P. Baluev, whose study of the press in the reign of Alexander III is a model of the grotesque vulgarization of history that can be produced by writers whose use of Marxism is arrested at the level of political agitation: Baluev's work is rubbish both as history and as polemic. Only the narrow specialist would enjoy a detailed exposition of his errors of fact and the nonchalant violence that he works upon his sources. Suffice it to say that the book can only be read as a sort of historical mirror-writing. Even as polemic it is ridiculous, written as though bands of Cadets were about to leap out of every Moscow sidestreet, laying about them with sheaves of statistics, quill pens, and other frightful weapons. One must hope that Baluev will quickly find his true *métier* as a writer of *feuilletons* for a party newspaper in one of the Soviet Union's duller provinces. Fortunately this work is an anachronism; most Soviet historians of journalism are doing excellent work, and the field of Russian studies will be notably enriched if more Western scholars follow Professor Ambler in using their techniques as the starting point for a fresh approach to the history of tsarist Russia.

DENNIS M. O'FLAHERTY  
St. Antony's College,  
Oxford

D. L. BESKROVNYI. *Russkaia armia i flot v XIX veka: Voennno-ekonomicheskii potentsial Rossii* [The Russian Army and Navy in the 19th Century: Russia's Military-Economic Potential].

(Akademiia Nauk SSSR, Institut Istorii SSSR.) Moscow: Izdatel'stvo "Nauka." 1973. Pp. 615.

In this substantial volume the author, one of the leading Soviet military historians, seeks to show the problems of Russian economic and military potential and how its defenses developed from a war machine based on serfdom and a backward industrial and transportation system to one with universal manhood service, a largely modern navy, an extensive railway network, and powerful industry. Earlier Russia had to make extensive changes in its war machine to cope with the French, such as introducing divisions, army corps, and a more rational command. But serfdom, which restricted military manpower and drew its officers solely from the nobility, prevented any real modernization of the military and naval services. Beskrovnyi approaches his subject in a series of essays, each of which deals with a separate aspect of the problem expressed in abundant and detailed statistical tables.

Because of the backwardness of the social system Russia was at a grave disadvantage in the Crimean War, which compelled sweeping reforms: in railways, modernization of weapons, military education, and the introduction in 1874 of universal military service. Industry, much of which was state-owned, had to be greatly expanded to provide the steel and breech-loading rifles and artillery, the smokeless powder, and the armored vessels and torpedo boats to keep pace with the rest of the world. Russian science, called on to provide technical assistance, proved able to meet the demands placed upon it. All this, however, had important consequences, for as the non-noble classes supplied more and more of the officers they took an active role in the revolutionary movement, which continued to grow. Moreover, the enlisted men, given a modicum of education, were vulnerable to propaganda, which in part was introduced into their midst by conscripted workers. Thus, in bringing the masses into the armed services, the autocracy was admitting a Trojan horse that was to bring about its ruin.

This volume is by no means easy reading, partly because of its organization. It is, however, so convincing in its mass of detailed archival evidence that it is a documentary

exposé that no one who deals with Russian defense matters in the nineteenth century can afford to overlook.

JOHN SHELTON CURTISS  
Duke University

KHADZHI MURAT IBRAGIMBEILI. *Kavkaz v Krymskoi voine, 1853-1856 gg., i mezhdunarodnye otnosheniia* [The Caucasus in the Crimean War, 1853-1856, and International Relations]. (Akademiia Nauk SSSR, Institut Vostokovedeniia. Azerbaidzhanskii Gosudarstvennyi Universitet im. S. M. Kirova.) Moscow: Izdatel'stvo "Nauka," Glavniia Redaktsiia Vostochnoi Literatury. 1971. Pp. 402.

A growing number of historical monographs have been appearing from the universities of the non-Slavic republics of the USSR. This one, from the Azerbaijani State University, exemplifies the problems that non-Slavic scholars face in the Soviet Union and the contributions to scholarship that they can nevertheless provide.

The author, an Azeri, is well acquainted not only with the usual literature, Russian and Western, on the Crimean War, but also archival materials from the Caucasian republics and published Turkish sources and accounts. With his unusual linguistic abilities the author is able to direct our attention to an important yet neglected part of the Crimean conflict, the military front between Russia and Ottoman Turkey in the Caucasus. Beginning with a long and relatively complete historiographical analysis of Russian-Ottoman relations in the Caucasus, which I think is the most valuable section of the book, Ibragimbeili proceeds to discuss, year by year, the events on the Caucasian front. More importantly the author includes discussions of local Caucasian political and military movements in his analysis of broader questions of international relations. In so doing Mr. Ibragimbeili presents an interesting revision of traditional Soviet historiography on Shamil and his rebellion against tsarist authority. Contradicting the usual view of Shamil as a reactionary Muslim fanatic he characterizes Shamil's movement as one for "national liberation" and relates it to Ottoman-Persian-Russian competition for hegemony in the Caucasus.

It is clear to the reader that Mr. Ibragimbeili feels uncomfortable with this position. One

notices, for example, the author's need to quote extensively from Marx, Engels, and Lenin to a much greater degree than a Russian historian would do today. In addition some of his most interesting ideas are so buried in traditional and rhetorical verbiage that only a careful reading can bring them to light.

Nevertheless this monograph is a welcome sign of major changes occurring in the Soviet historical profession, particularly in those branches in the smaller republics. For historians of the Crimean War it is a very important addition.

ALAN W. FISHER  
Michigan State University

B. G. LITVAK. *Russkaia derevnia v reforme 1861 goda: Chernozemnyi tsentr, 1861-1895 gg.* [Russian Villages during the 1861 Reform: The Black-Soil Center, 1861-1895]. (Akademiia Nauk SSSR, Institut Istorii SSSR.) Moscow: Izdatel'stvo "Nauka." 1972. Pp. 422.

The process of serf liberation in Russia involved two documents. The first was a contract (*ustavnaia gramota*) identifying the land allotments the ex-serfs were to receive and establishing the amount of rent they had to pay. The second was the "redemption act" (*vykupnyi akt*) by which the villages actually received their allotments in ownership and undertook to pay the government for them. The contracts were drawn up and signed in the early 1860s. Some of the acts were completed at about the same time, but most took longer. When in the 1880s the government ordered the last of them to be completed about one-fifth of the serfs had still not taken over their allotments. Litvak tells us that these documents—the contracts and acts—are the best source for studying the evolution of peasant landholding in nineteenth-century Russia. He ought to know; he has been working with them for almost twenty years. This book presents an exhaustive analysis of them in six *gubernias* of the central black-earth region of European Russia: Voronezh, Kursk, Orel, Riazan, Tambov, and Tula.

Litvak is the first scholar to make such an analysis on a large scale. It leads him to conclusions that differ with many hitherto prevailing views. He says, for example, that the increase in the number of household serfs on *barshchina* (*corvée*) estates during the early nine-

teenth century did not signify the rise of gentry luxury. Most household serfs were skilled laborers and the increase in their number probably reflected a growing spirit of economic enterprise. Further Litvak points out that *barshchina* estate owners were not taking away land from their serfs in the decades prior to the liberation. They did not even take as much land from them in the course of the liberation as did the *obrok* (money rent) estate owners. As for the *obrok* estates, rates of payment rose during the early nineteenth century, but serf income rose faster. Generally speaking Litvak concludes that gentry exploitation of the peasants before and after the liberation was not a matter of crushing or oppressing them, but of maintaining them in a condition that would allow them to work effectively.

Litvak is more cautious with his statistics than any historian of serfdom I have read. He qualifies most of his generalizations with whole catalogs of local exceptions. He does not insist on what his statistics prove so much as what they do not indicate. Litvak notes, for example, that gentry indebtedness in the 1850s was much higher on prosperous estates involved in market relations than on smaller, more primitive economies. However parasitic the serfowners may have been their debt did not reflect their insolvency or the economic unworkability of serfdom.

There are questions Litvak ignores. He takes no great interest in the fate of the gentry during the liberation; only in the ex-serfs. Moreover he leaves out any consideration of peasant income in the decades after the liberation. The questions he does ask, however, he answers convincingly despite the usual admixture of leninical exegesis. His scholarship is in the best Russian tradition.

GEORGE YANEY  
University of Maryland,  
College Park

N. A. TROITSKII. "*Narodnaia Volia*" *pered tsarskim sudom, 1880-1891 gg.* ["The People's Will" before the Tsarist Courts, 1880-1891]. [Saratov:] Izdatel'stov Saratovskogo Universiteta. 1971. Pp. 238.

The 1880s marked an extended pause in the tempo of the Russian revolutionary movement. The populist faith in a peasant uprising had been shattered, society lay quiescent under re-

doubled government repression, and Marxism was still in its "gestational period." One force alone actively opposed the government, the dwindling and increasingly disorganized party of the "People's Will" (*Narodnaia Volia*). Troitskii's statistics show that members of *Narodnaia Volia* were defendants in all but 32 of the 103 political trials held in Russia between the middle of 1880 and the beginning of 1891. The leaders of *Narodnaia Volia* sought to turn these trials to the advantage of the party and by programmatic statement, agitation, or defiant gesture, made their day in court their last revolutionary act. Drawing on material from a variety of published and archival sources Troitskii reveals the party's program and ethic through its struggle with an increasingly baleful and arbitrary judicature.

What is most interesting about this book is Troitskii's assessment of the role that *Narodnaia Volia* played in the history of the Russian revolutionary movement. He ascribes to the party a greater stature and significance than recent Soviet scholarship has allowed. For Troitskii *Narodnaia Volia* is not a last bankrupt manifestation of populism, but an integral stage of revolutionary development marking a transition from populism to the new modes of opposition that will develop in the 1890s. Dealing with the period 1879-82 Troitskii specifically challenges S. S. Volk's contention that the tactic of political terror was a "fatal mistake" and argues persuasively that within the specific historical context of those years terror was not only unavoidable but correct. Troitskii's treatment of *Narodnaia Volia* during its years of disintegration after 1882 is equally provocative. He emphasizes those aspects of the party's organization and tactics that tend to link it as a precursor to the later development of Russian Marxism.

DONALD SENESE  
University of Victoria

IA. S. KHONIGSMAN. *Pronyknennia inozemnoho kapitalu v ekonomiku Zakhidnoi Ukraïny v epokhu imperializmu (do 1918 r.)* [Penetration of the Economy of Western Ukraine by Foreign Capital in the Era of Imperialism (till 1918)]. Lvov: Vydavnytstvo L'vivskoho Universytetu. 1971. Pp. 253.

Novel in this study of economic history of the northeastern corner of the Austro-Hungarian

Empire in the period between 1890 and 1918 is the author's statistical appraisal, partly based on local archives, of a relatively high level of development of industries, banking, and especially of the joint-stock enterprises and cartels, in comparison not so much with the rest of Austria-Hungary as with the rest of the Ukraine in the Russian Empire. In the existing historiography on the capitalist economy in the Ukraine, to which this monograph contributes, predominant so far has been the view that the Austrian Ukraine was an extremely backward, overpopulated, agrarian region, with only a scattering of significant industries, such as petroleum and timber. Jacob Samuel Khonigsman shows now, however, that by 1912 industry finally came to produce slightly more of the local GNP than did agriculture, so that at least structurally the region was not much less developed than the rest of the Ukraine. There was indeed a very belated but genuine industrial revolution in eastern Galicia, Bukovina, and Trans-Carpathia, and decisively instrumental in it was the Rothschildian, German, Austrian, and some other multinational West European capital, particularly financial. Khonigsman estimates that in 1912 all foreign (i.e., in his definition, nonlocal) capital in west Ukrainian industries (excluding railroads, banks, and trade) amounted to 596.7 million crowns, or 73 per cent of total local working capital. This is a large figure if one compares it with about 550 million rubles of foreign capital spread throughout the whole of the Russian Ukraine (one crown was equal to 0.39 rubles). Following Lenin's definition Khonigsman views western Ukraine as an "internal colony" of Austria-Hungary (why not of the whole Western Europe?). One may still wish that his thesis concerning economic exploitation be better evidenced statistically, particularly through estimates of the west Ukrainian balances of trade and payments for longer periods of time. Had Khonigsman researched customs and banking archives in Vienna this task could probably be a feasible one. Also in a monograph dealing with Austria a German language summary would have been more useful for the international reader than only the Russian one.

VSEVOLOD HOLUBNYCHY

*Hunter College,*

*City University of New York*

M. P. SANAKOEV. *Deiatel'nost' krest'ianskogo pozemel'nogo banka v Gruzii, 1906-1917 gg.* [The Activity of the Peasant Land Bank in Georgia, 1906-1917]. (Akademiia Nauk Gruzinskoi SSR, Iugo-Osetinskii Nauchno-Issledovatel'skii Institut.) Tbilisi: Izdatel'stvo "Metsniereba." 1971. Pp. 109.

This little volume examines the reasons for the establishment of the Peasant Land Bank in Georgia and its activities. The bank was set up in 1883, but extended to Georgia only in December 1905. The bank's main purpose was to facilitate the purchase of land by peasants. In practice, Sanakoev argues, the bank served the interests of the nobles by providing a mechanism for selling their land advantageously and provided the state a medium for implementing policies. As the period of its Georgian activity coincided largely with the years of the Stolypin land reforms it worked to encourage personal landowning at the expense of communal holdings.

The heart of the book is a study of the activities of the bank, replete with statistical tables. These examine the amount of land bought and sold, amounts paid, loan terms, and debt arrears. The second half of the book is made up of short essays on special topics about the role of the bank in the implementation of Imperial policies, especially the Stolypin reforms; the settling of Russian peasants in the area; the liquidation of German landholdings in World War I; and a brief but tendentious account of the period of Georgian independence. The data, though valuable, are difficult to use because the author constantly switches from Georgian to Transcaucasian or other regional bases for his figures. Moreover the book fails to elaborate on the social impact of the bank's activities. A glimpse is provided here and there, as in a chart showing shifts in landowning by nationality, but these features quickly slip past. Not only is the human factor absent, but the author does not even consider what role the Revolution of 1905 might have had on the decision to establish the bank. The broader perspective is sadly lacking.

While the proportions and aims of the book are modest it is useful to have such studies as a balance to the Great Russian orientation of most work concerning agriculture and the peasants, even if the author fails to make com-



parisons. It will be of interest to scholars studying a variety of topics: the peasantry, the Caucasus area, the nationality question in Imperial Russia, the Stolypin reforms, to name only some of the most obvious.

REX A. WADE  
University of Hawaii

N. VALENTINOV (VOL'SKII). *Novaia Ekonomicheskaiia Politika i krizis partii posle smerti Lenina: Gody raboty v usikh vo vremia NEP vospominaniia* [The New Economic Policy and the Party Crisis after the Death of Lenin: Reminiscences of My Work at the VSNKh during the NEP]. Edited by J. BUNYAN and V. BUTENKO. With an introduction by BERTRAM WOLFE. (Hoover Institution Foreign Language Publications.) Stanford: Hoover Institution Press, Stanford University. 1971. Pp. xix, 256. \$9.50.

V. P. DMITRENKO. *Torgovaia politika Sovetskogo gosudarstva posle perekhoda k nepu, 1921-1924 gg.* [The Trade Policy of the Soviet State after the Transition to NEP, 1921-1924]. (Akademiia Nauk SSSR, Institut Istorii SSSR.) Moscow: Izdatel'stvo "Nauka." 1971. Pp. 269.

Dmitrenko's monograph on the commercial policy of the Soviet government from 1921 to 1924 offers little new on the subject, though he fills in an occasional detail. He is most interesting when describing the breakdown of Bolshevik resistance to the replacement of the state allocation of goods by a system of regular trade, which struck many Bolsheviks as an abandonment of the cause of the Revolution. But the author's obsession with the role of Lenin is debilitating and detracts from the value of the book, as does his distortion of the views of such people as Bukharin and at least one reference to Soviet archives for material readily available in print.

Valentinov's book is a different proposition since for the most part it is archival material in its own right. In the decades before his death in 1968 Valentinov devoted himself to reminiscences. In 1953 he published his memories of *Encounters with Lenin* (translated in 1968), relating to the time of the party schism. The book gave probably the only recognizably human portrait of Lenin that we possess. The present volume is devoted to portraits of other postrevolutionary luminaries, especially Dzerzhinskii, as head of the Soviet industrial or-

ganization, and Piatakov, his antagonistic lieutenant.

Valentinov was one of a number of non-Bolshevik intellectuals who, between 1923 and 1925, agreed to serve the Soviet state because they thought the regime had divested itself of the radical utopianism of the civil war period. They were also moved by the thought that no ready alternative was available if they wished to serve the interests of their country. Valentinov and a group of friends formed the "League of Observers," which shared a general perspective and pooled information and gossip that came its way from government and party officials with whom its members associated. In this volume Valentinov recreated from memory the activities and findings of the League. His memory was indeed colossal and, when it can be checked, accurate. Problems arise when corroborative information is lacking or when he gets so carried away by a concrete detail that the reader is hard put to know how important the detail may be. (Admittedly, if, as Valentinov reports, a special secret commission was formed in the Central Committee of the Communist party to investigate whether Lenin died of syphilis, perhaps the history of the Soviet higher circles really should be written in terms of small-town gossip-mongering.)

As a non-Bolshevik specialist, in his case an economic journalist, Valentinov fell in love with Dzerzhinskii when the latter moved from the Cheka to the VSNKh. By 1923 Dzerzhinskii had become a fanatical moderate (expressing his opposition to the industrialization schemes of the left wing with temperamental fanaticism) and treasured non-Bolshevik specialists who could perform. By the same token Valentinov's villains were the left opposition—Preobrazhenskii, Trotsky, Piatakov—in whom he saw too much civil war radicalism. His relations with Piatakov sound very odd since Piatakov seems to have made him something of a confidant, a role that Valentinov accepted.

In his book Valentinov argues openly, as he had before privately, that the New Economic Policy (NEP) provoked a tremendous opposition within the Communist party, that Lenin had to exercise all his authority to prevail, and that even after 1921 important segments of the party remained hostile to the principle of the NEP. Implicitly, Dmitrenko makes the same

case. But the evidence remains puzzling. If anyone had protested to NEP in 1921 the protest was denied in 1926-27 in public party gatherings. And had there been any shred of evidence showing that friends of Trotsky and Zinoviev objected in 1921 to NEP it would certainly have made big news in 1926. That it never did suggests the need to keep a degree of skepticism in accepting the thesis of widespread hostility to NEP at its inception among Communist leaders.

Altogether Valentinov's work is a lively, interesting description of personalities, which supplies a missing element when considering the 1920s. His description of the inner workings of the Special Conference on the Reproduction of Basic Capital (OSVOK) is particularly enlightening. The editors and the Hoover Institution are to be thanked.

DANIEL MULHOLLAND  
Tufts University

#### NEAR EAST

ROBIN E. WATERFIELD. *Christians in Persia: Assyrians, Armenians, Roman Catholics, and Protestants*. New York: Barnes and Noble. 1973. Pp. 192. \$11.75.

Students of Iranian civilization have almost invariably disregarded the fact that Iran has always been a plural society composed of several distinct religious and cultural personalities. One reason for this oversight has been the tendency for Iranian cultural identity to be co-opted by the dominant religious creed of any given period. Thus, under the Sassanids, to be an Iranian meant to be a Zoroastrian; under the Safawids and the Qajars, to be an Iranian meant to be Shi'a Muslim.

For the historian the problem of defining the ongoing relationship between majority and minority communities is peculiarly baffling, whether the relationship is defined in terms of day-to-day social and economic intercourse or in terms of the more subtle symbiotic processes of acculturation. Even during the nineteenth century, a period when data relating to the minorities are most abundant, it is difficult to determine what impact, if any, the existence of Armenian and Nestorian Christian, Jewish, and Zoroastrian communities exercised upon

the Muslim majority. Of these minorities, the Armenians must have made a substantial, if unobtrusive, contribution to the molding of Iranian civilization, especially in the period when the Armenian kingdom constituted a formidable buffer protecting the northwestern marches of Arsacid and Sassanid Iran. Was there an Armenian element within the Iranian architectural tradition? Did the survival of Christian minorities during the early centuries of Arab domination contribute to the luxuriant growth of sectarian movements on the Iranian plateau? What was the effect upon the ethnic composition of Islamic Iran of a slave trade in Armenian and Georgian Christians, especially women, who were still being brought annually into Iran down to the first half of the nineteenth century? There can hardly have been a prominent family in Qajar Iran that did not somewhere have Christian Armenian or Georgian forebears.

To pose these questions is to indicate the limitations of Robin Waterfield's *Christians in Persia*, a most readable account that concentrates on the rather modest activities of nineteenth- and twentieth-century Catholic and Protestant missionaries in a country that shelters two of the most ancient Christian communities in the world, communities whose cultural traditions are hardly mentioned here. This is essentially an account of missionary activity, mainly from a missionary point of view. It is sometimes rather naive, occasionally inaccurate with regard to the broader currents of Iranian history, but it also contains much interesting information that, unfortunately, is unannotated. It does not take cognizance of the way in which Iranian nationalism, like other Asian nationalisms, regarded the missionaries as part of the larger intrusion of European imperialism, but it rightly records the devotion of a handful of men and women, "narrow, earnest men, but chiefly earnest," who made a substantial contribution to the educational and medical history of modern Iran, a contribution all too often overlooked.

This is not a definitive history of Christianity or even of European missionary activity in Iran, but most specialists in recent Iranian history will probably want to refer to it. The author may have overextended himself in taking on a subject of such dimensions. Pos-

sibly he should now narrow his range and attempt a detailed account of the activities of the Church Missionary Society in Iran from 1869. The subject deserves a chronicler, and Mr. Waterfield seems well qualified for the role.

GAVIN R. G. HAMBLBY  
Yale University

JOSEPH J. MALONE. *The Arab Lands of Western Asia*. (The Modern Nations in Historical Perspective Series.) Englewood Cliffs, N.J.: Prentice-Hall. 1973. Pp. x, 269. Cloth \$8.95, paper \$4.95.

The Modern Nations in Historical Perspective series is designed to summarize "the chief historical trends and influences that have contributed to each nation's present-day character, problems, and behavior." Such a task is indeed difficult in a region in which more than one state is of questionable viability, and the concept of the nation-state itself does not flow in every case from the historical tradition.

Joseph J. Malone has made the best of the charge in *The Arab Lands of Western Asia*. Here are lucid accounts of the independence years of Lebanon, Syria, Iraq, Jordan, and the various polities of the Arabian Peninsula. There runs throughout a theme of diversity derived from unique historical roots, an emphasis that brings forth details of local history seldom included in surveys of this sort.

Yet this book has serious problems that render its overall utility questionable. In most cases the treatment of the preindependence years of the polities discussed is so compressed that events are referred to, rather than described. Hence its readership becomes in effect restricted to those already acquainted with Middle East history.

As the title suggests, not only is Israel omitted but also Egypt and North Africa. Denied more than half the Arabic-speaking world by previous volumes in the series, Professor Malone has found it necessary to depreciate the force of the Arab nationalist movement, however chimeric its goal may be. Egypt, the principal protagonist of that ideal in the Nasser years, is thus portrayed as a factor external to the region, despite its constant intrusion into the pages of the volume. Such an interpretation only serves to em-

phasize the artificiality of the concept of "the Arab lands of western Asia."

The focus of the series on the contemporary nation "in historical perspective" creates anomalies with which Professor Malone must wrestle. Lebanon, Syria, and Iraq as national entities with historic roots have a certain validity, but to put Jordan on the same plane and link it with Nabataea and the trans-Jordanian Biblical kingdoms implies a nationality, although the author does not specifically claim it. Turning to the Arabian Peninsula, Professor Malone faces a new difficulty with the concept he must manipulate. It is an area really quite separate from that which he has already discussed in separate chapters, one with a common history of insularity and Islamic traditionalism. It thus becomes a single chapter, nearly half the book. Yet the concept of the nation so dominates the structure of the volume that separate sections must be devoted, for example, to the two Yemens. Only when the Persian Gulf region is reached is the principle sacrificed to common sense, for the sheikdoms are hardly nations by any definition.

Finally, the state-by-state categorization has been extended to the index with ludicrous results: such stalwart pan-Arabicists as Michel Aflaq and Sati' al-Husri may be found only under the rubric "political leaders" for Syria and Iraq respectively.

HERBERT L. BODMAN, JR.  
University of North Carolina,  
Chapel Hill

ISAIAH FRIEDMAN. *The Question of Palestine, 1914-1918: British-Jewish-Arab Relations*. New York: Schocken Books. 1973. Pp. xiii, 433. \$12.00.

Making use of previously unavailable British cabinet and Foreign Office papers at the Public Record Office, Isaiah Friedman has set himself an ambitious project: to unravel the tangled skein of pronouncements, exchanges, and deals involving the British government in the Middle East in World War I. In successive chapters he deals with various aspects of the personalities, events, expectations, and circumstances behind the Balfour Declaration, the Husain-

McMahon correspondence, and the Sykes-Picot Agreement.

The book has great merit. It is a detailed and revealing record with copious notes. Indeed, the notes are perhaps the most valuable contribution, for the reader now has a ready index to the newly available materials. Moreover, in attempting to deal with the whole range of issues, Mr. Friedman usefully juxtaposes the choices, dilemmas, and options so that each illumines the others. In the past, most authors have not even attempted this.

The book, however, fails to meet its objective in three important, even crucial ways. First, it does not set the stages or even the main stage upon which the British statesmen had to operate, nor does it suggest the circumstances in which the decisions were made. The First World War was vast and hard-fought beyond the worst dreams of those in power when it began; yet one senses a sort of denatured, even ethereal, approach to events and decisions, an approach that was far from reality. Second, an order is imposed upon events and decisions, and it significantly distorts the turmoil of the war and the *ad hoc* nature of the decision-making process of all governments and especially of the British government of that period. Third, so neatly does he make the Balfour Declaration, the Sykes-Picot Agreement, and the Husain-McMahon correspondence fit together that there is no disparity to worry about and consequently no "problem." Too bad the British statesmen did not have Mr. Friedman's book to satisfy their worries. Lord Curzon flatly contradicted Mr. Friedman's thesis on December 5, 1918; and Mr. Balfour on August 11, 1919, warned his cabinet colleagues that "so far as Palestine is concerned, the Powers have made no statement of fact which is not admittedly wrong, and no declaration of policy which, at least in letter, they have not always intended to violate." Earl Grey expressed similar if more guarded thoughts in the House of Lords years later. Of these worries and doubts Mr. Friedman takes no note. In consequence one comes inevitably, I believe, to the conclusion that what he has prepared is a legal brief rather than a historical analysis. Unfortunately, as Mr. Friedman's approach to his subject shows, he is wrong in saying that "today it [the

Palestine question] has no more than an academic value . . . [while] in the past the keenest brains in Middle East politics were pitted against each other to prove their respective cases, only to demonstrate the gulf that separates them." But this study is another step toward the writing of a history. Taken together with other recent steps, notably Doreen Ingram's *Palestine Papers, 1917-1922* (London, 1972), this work offers the hope that another fifty years may not pass before we can look forward to a balanced, synthetic analysis.

WILLIAM R. POLK

University of Chicago

#### AFRICA

C. C. STEWART, with E. K. STEWART. *Islam and Social Order in Mauritania: A Case Study from the Nineteenth Century*. (Oxford Studies in African Affairs.) New York: Oxford University Press. 1973. Pp. xviii, 204. \$14.50.

When my *Mauritania: A Survey of a New African Nation* was published (1967), I noted in the introduction that my objective—to write a history of that country—was thwarted by the lack of historical analyses of segmentary societies there, and I called on other scholars to fill that gap systematically. *Islam and Social Order in Mauritania*, an adaptation of C. C. Stewart's Ph.D. dissertation, is an excellent contribution to that proposed, indispensable, bricklaying scheme.

The particular concern of the book is the career of Sheik Sidiyya al-Kabir (1775-1868), a venerated Mauritanian Qadiri mystic and jurist who, by the mid-nineteenth century—a century of intense Islamic activity in West Africa—was a major figure in religious, political, and economic affairs on the right bank of the Senegal River.

A presupposition carried over into West African history from Middle Eastern writing is that of the polarity of *figh* and *Tasawwuf* in Islam and that this culture is almost by definition an urban phenomenon. Prior to this century, however, this polarity was a negligible one in West Africa, and most highly respected jurists, like Sidiyya al-Kabir, also enjoyed very large followings as mystics of a Sufi brotherhood. This study painstakingly attempts to

explain this deviation from the norm. It also seeks to answer, in the context of nineteenth-century Mauritanian Gebla, such questions as, What were the roots of a possible connection between legal reforms and mysticism in the literature or social structure of influential Muslim communities in West Africa? What was the precise motive of the relationship between these themes of legal reforms and mysticism? How can one explain the presence of the Qadiriyya brotherhood in the greater part of the states and communities established by West African Islamic reformers? And what role did the Kunta scholars of Timbuktu play in the dissemination of that tariqa?

The authors were not only fortunate in their efforts to gain some historical perspective on the period because of a rich literary tradition in nineteenth-century Mauritania, but also because of the judicious analysis of these sources and the invaluable assistance they sought from such Mauritanian scholars as Moktar ould Hamidoun, Mohammad ould Mouloud ould Daddah, and others. I also heard a lot of positive comments about C. C. Stewart from a wide variety of Mauritanian intellectuals while visiting there last summer. But the authors evidence some lacunae in their understanding of Moorish society—at least this is suggested by the very skimpy section, "The Peopling of Mauritania," which does not do justice to the very rich social patterns of that country. There is, for instance, a lack of distinction made between the nobility of Arab origin and that of Berber origin. Furthermore, the terms *zenagha* and *lahma* are not necessarily interchangeable.

Elsewhere the authors refute the contention that the war of Shurr Budda, a watershed in the history of Shingit, referred to in my book as "Mauritania's Thirty Years War," was a conflict in which the Berber autochthonous rose up in a final stand against the Hassani intruders, but they do not offer any other alternative reason.

Be that as it may, the Stewarts drew many conclusions about the practice of Islam in the Mauritanian Gebla and also raised many questions that may be taken up by other researchers; among the former is that by the late 1860s the most profound changes taking place there were not in response to an external (French)

power, but rather the result of the emergence of an individual, Sheik Sidiyya al-Kabir, and a people, the Awlad Ibiri, who bridged traditional hassani and zawiya divisions in Moorish segmentary society. On the whole, this excellent book is a welcomed rare addition to the history of West African Islam from its most important source—Mauritania.

ALFRED G. GERTEINY

University of Bridgeport

JOHN DE ST. JORRE. *The Brothers' War: Biafra and Nigeria*. Boston: Houghton Mifflin Company. 1972. Pp. 437. \$10.00.

ALAN FEINSTEIN. *African Revolutionary: The Life and Times of Nigeria's Aminu Kano*. [New York:] Quadrangle/The New York Times Book Company. 1973. Pp. xvi, 299. \$9.95.

R. A. ADELEYE. *Power and Diplomacy in Northern Nigeria 1804-1906: The Sokoto Caliphate and Its Enemies*. (Ibadan History Series.) [New York:] Humanities Press. 1971. Pp. xvi, 387. \$10.00.

*The Brothers' War* is not the first and certainly will not be the last attempt to put the events of the Nigerian-Biafran conflict into an adequate perspective. Scanty coverage of the war by U.S. news media makes the effort welcome. Earlier publications have been mainly documents with annotations or involved persons' interpretations. John de St. Jorre's account is first-rate journalism; he covered the war, visiting both sides, after experience in war theaters in Southeast Asia, and now he has drawn together his own and other reports, official documents, background materials, and reflections and has produced a very readable account. He professes to be impartial, and no doubt he intends to be, but there seems to be a preference on his part for the principal of federalism rather than secession and the fact that the secessionists lost tends to give a certain coloring to his presentation that may make a close reader question somewhat the fullness of this impartiality. The personalities of the opposing leaders in de St. Jorre's pages tend to forecast in the manner of a Greek drama the ultimate conclusion: Ojukwu is egotistic; Gowon has humility; the former tends toward autocratic decisions, the latter is consultative.

Beyond the history of the military events

and the ideological forces behind them *The Brothers' War* also reveals how the political structure of Nigeria has changed because of this experience. Gowan, finding himself in charge after the second coup (in which he had not participated), had a very uncertain basis of power. He demonstrated a shrewd political sense and an ability to win adherents. Being from a Middle Belt minority people he had empathy with the minorities in all regions who aspired to escape the domination of the major ethnic groups. The creation of twelve states in place of the old three regions strengthened his position during the war and has consequences for the future political complexion of the country.

Aminu Kano benefited from this move by Gowan. The idea was as much his as it was the commander's. Aminu had sought greater autonomy of the various ethnic groups of the north, the Middle Belt, and elsewhere, but his party was in a shambles on the eve of the civil war. Intimidation and violence had shattered the political machine he had built. The first coup had eliminated the Sardauna, Aminu Kano's opponent, and no new leader of the conservative forces emerged.

Aminu Kano is in the tradition of the mujāddidūn, reformers of Islamic society who have so effectively altered the course of western Sudanic history. The prototype in Nigeria is 'Uthmān dan Fodiye, whose reforms, backed with military force, created the Sokoto caliphate. Adeleye recounts the first century of the Sokoto presence: establishment of the caliphate, its constitution, internal rivalries, relations with other African polities, intrusion of Europeans, and British conquest. This story of massive dimensions is competently told; that this could be done is a mark of the maturity of historical research in Nigeria for Adeleye draws upon the works of other historians who have published in the last decade. About a decade after Adeleye leaves off his account Aminu Kano was born into the society that resulted from the adjustment of the caliphate to British rule.

The revolution that Aminu worked for was not just the removal of the British, but also of the arbitrary power of the emirs, the heirs of dan Fodiye and his followers who had become as oppressive as the authorities their predecessors had overthrown. This revolution has now

been accomplished to a great extent, but not in the way Aminu would have wished or could have foreseen. This modern reformer is unlike the founder of the caliphate in that he advocated nonviolent means. Dissatisfied majors set things off on the road to war by an uncompleted coup, and before it was over the emirs had been reduced in power and more contained in institutionalized restraints. While he was not personally the means of accomplishing his reforms, and is not by any means satisfied with how things are, he is a man whose life has been a forceful influence on his people. Aminu's life story exudes hope; the failure of the caliphate to attain and maintain its highest ideals and the bitterness of civil war reek of the tragic sense of life. That the wars are now in the past and the humanist reformer is still on the scene to make further contributions, and that he has countrymen of comparable outlook, bodes well for an emergence from military government to full political participation again—and on a basis that can avoid the divisive antagonism that has cost them so heavily.

These three books—only one by a professional historian—are not particularly related except that they are about Nigeria and published about the same time, but taken together, while they leave out much, they tell a large part of the modern history of one of the most important nations in Africa.

DANIEL F. MCCALL  
Boston University

#### ASIA AND THE EAST

JOHN W. DARDESS. *Conquerors and Confucians: Aspects of Political Change in Late Yüan China*. (Studies in Oriental Culture, number 9.) New York: Columbia University Press. 1973. Pp. 245. \$12.50.

This book looks at the history of the Yuan dynasty from the viewpoint of the occupying power, the Mongols, while most works have taken the Chinese viewpoint. Thus, much attention is given to loyalties to family and tribe and the problems that the Mongols faced when they ruled over the Chinese. Professor Dardess states that from 1260 on, the Mongol rulers became "Confucianized," meaning they accepted the principles of filial piety and ances-

tral loyalty that could be integrated into the Mongol value system, without accepting Sinification, which could have involved loss of Mongol language, ancestry, and caste.

The book starts with the anti-Confucian intermezzo of Bayan between 1328 and 1340 and his racist pro-Mongol policy, equating Confucianism with Chinese nationality. This policy worked against the supranational factional alignments that had dominated the earlier part of Mongol rule; it did not succeed because there was opposition against it from the side of the Mongols themselves. The period following Bayan's rule, from 1344 to the end of the dynasty in 1367, marked the end of a truly Mongolian regime in China and the Mongolian steppes, and the Mongol dynasty became a Chinese dynasty in which the 3 per cent Mongols by law demanded a quota of 50 per cent of all qualified degree holders and were sucked into the Chinese factional struggles. The Confucians were split between modernists (Wang An-shih group) and conservatives (Szu-ma Kuang group). Dardess thinks that the conservatives did not constitute a faction, but were tied together by individual moral exertion, not by self-interest (p. 164). They first consulted the local gentry and got their cooperation before they decided to propose action; their opponents were for determined, planned action. But if we hear that the conservatives were accused of venality (p. 166), we may suspect that they defended local interests, for instance, in their failure to check coastal piracy. So we may doubt whether they were kept in control by strong "institutional loyalties" and not by clique ties, especially as dismissal from office meant execution or death within a year.

Dardess shows that the breakdown of Mongol rule after 1344 was basically a consequence of the breakup of the federation of Mongol tribes and not a consequence of general decadence. When rebellions began to break out within the tribes, although Tohto could at first crush them, he was soon forced to give up hopes of decisive military action against rebels, resulting in the eventual loss of south and southeastern China. Regional interests of individual Mongol leaders and their tribal associates began to thrive and could no longer be controlled by Confucian ideology. The final victory of the rebels and the end of Mongol rule

was, thus, not so much due to a national upheaval against the foreign rulers, but due to a breakup of tribal loyalties within a federation of tribes that graded the tribes according to their situation at the creation of the Mongol federation. Tribal federations of this type are always in danger when the leader of the federation is weak, and the last Mongol emperors, indeed, were weak.

Dardess's book, well based upon Chinese, Mongol, and other documentary sources, will change many of our views concerning this period of central Asian or Chinese history. Some problems, in my opinion, still have to be discussed. For instance, because the Mongols officially sponsored Lamaism and Buddhism, to what degree was the Confucianism of the Mongols acceptable to Chinese? Does not the Mongol stress upon ethnicity indicate their ideological orientation? While we see that the conquerors leaned toward some form of Confucianism, or toward Confucians, we would like to see why they also appealed to currents in China that were opposed to Confucianism. Perhaps we also should know more about the background of the economic policy of the period between 1328 and 1367, which followed a period of strong financial and fiscal interest of Mongols in China. But the main value of this book is to take a solid look at the Mongol dynasty from the viewpoint of the conquerors and rulers. Much of what the author has said here can be used for a deeper understanding of other dynasties of nomadic conquest.

WOLFRAM EBERHARD  
University of California,  
Berkeley

PHILIP A. KUHN. *Rebellion and Its Enemies in Late Imperial China: Militarization and Social Structure, 1796-1864*. (Harvard East Asian Series 49.) Cambridge, Mass.: Harvard University Press. 1970. Pp. 254. \$8.50.

This work is a brilliant analytical study of the process of militarization that accompanied the breakdown of imperial control in the countryside during the late Ch'ing period. The first part of the study traces the historical development of various forms of militarization in traditional China, the uniquenesses of early Ch'ing forms, and finally the emergence of the gentry-

led *t'uan-lien* militia system in south and central China during the White Lotus Rebellion and its aftermath. This material is woven around the author's definition of militarization—a process by which men are severed, in varying degrees, from their roles in civilian life while bearing military responsibilities (pp. 13–15). In a superbly reasoned analysis the author demonstrates that bureaucratic divisions, such as *pao-chia* and *li-chia*, were interfused with “natural” organizational units in local society and thus easily molded into a cohesive hierarchy of *t'uan-lien* units.

The second part of the study deals with the Taiping Rebellion and the way in which the *t'uan-lien* system became the cornerstone of imperial defense. The author carefully analyzes the strengths of the personal armies that were built on the orthodox fabric of gentry dominated *t'uan-lien* units and contrasts them with heterodox alternatives, such as the Taipings. He concludes that the expansion of the *t'uan-lien* system during the Taiping Rebellion resulted in direct gentry control of subprefectural government in the postrebellion period, which, to some extent, was to shape political developments in twentieth-century China.

The contributions of this study are enormous. In addition to binding the Taiping period to the beginning of Ch'ing decline with the tough thematic threads of local militarization, the author has put historians on the scent of a whole range of problems regarding local control. Was the process of militarization a phenomenon unique to south and central China, or was it being generated throughout China by conditions of imperial decline? How important were gentry-led militia in controlling piracy on the China coast prior to the Opium War period? Was direct gentry domination at the local level always a function of the militarization process? How does one account for situations where heterodox forms of militarization seem to have persisted or were never completely enveloped by gentry organization? Does the direction of the militia movement in the early nineteenth century bear the unique imprint of the statecraft reformers, many of whom were Hunanese in origin? One hovers between the search for historical uniqueness on the one hand and generalization on the other. The

answers lie, as the author suggests, in further research into late Ch'ing local history.

Viewing the study more broadly, this book is a pivotal interpretive work that directs the scholar's attention to the internal dynamics of late Ch'ing history and away from the somewhat Europocentric concern with China's responses to external stimuli on the China coast. The author has sensitized late Ch'ing historians both to Peking perceptions of the nineteenth-century historical moment and also to hinterland responses to those problems that were, in Chinese eyes, the great historical issues of the times.

This work is a rigorous and creative piece of scholarship and unquestionably one of the most significant works to appear on late Ch'ing history.

JANE KATE LEONARD  
Kean College of New Jersey

JEAN CHESNEAUX. *Peasant Revolts in China, 1840–1949*. Translated by C. A. CURWEN. (Library of World Civilization.) [New York:] W. W. Norton and Company. 1973. Pp. 180. \$7.95.

Most peasant movements are almost by definition local, sporadic, and fragmented. To assess their role in the history of China between 1840 and 1949 in 166 liberally illustrated pages is in a sense already a tour de force. This little book does more, however. For the nonspecialist it is a compact and clear introduction to the subject, while the specialist will find it interesting and useful as a general interpretation by one of the more eminent scholars in the field. The book is well written, and here credit should be given not only to Professor Chesneaux's original text, but also to the translator, who is himself a scholar in the same field.

The basic concern of the book is stated on page 20: “What historical role did peasant movements play in the evolution of imperial China through the ages? What function did they have in the dynamics of Chinese history?” The first four chapters deal mainly with peasant revolts in the nineteenth century, with an excellent introductory chapter that places these revolts against the premodern rebel tradition. The last four chapters deal with peasant movements in the twentieth century, but focus mainly on that part of the peasant movement



led by the Chinese Communist party, so that in effect the book shifts at midpoint from a study of peasant movements as a whole to a discussion of only that part of it which "accomplished the agrarian revolution." This is unfortunate because, as Chesneaux himself points out on page 83, probably more peasants were involved in "archaic forms of struggle" during 1911-49 than were involved in their "modern counterparts." The second half of the book is really a study of the peasant role in the Chinese revolution, and that is rather less than the title of the book promises.

The book also bears some traces of having been hastily done; there are various inconsistencies of some importance. For example, on page 62 Chesneaux refers to the Elder Brother Society's occupation of Sian in the far northwest in 1911, and five pages later categorically states that the society operated only in the Yangtze Valley. Similarly, there are inconsistencies (see pp. 65 and 72) in his assessment of the effects of Western imperialist pressure on peasant activity and in his evaluation of how "radical" the Peasant Associations of the 1920s were (pp. 90-95). In spite of these and some minor factual errors, this is an insightful and useful little book that should be particularly welcome to those whose field is not China but who would like, perhaps for teaching purposes, an introduction to some aspects of the Chinese historical experience.

ELLA S. LAFFEY  
McGill University

PAUL RICHARD BOHR. *Famine in China and the Missionary: Timothy Richard as Relief Administrator and Advocate of National Reform, 1876-1884*. (Harvard East Asian Monographs, 48.) [Cambridge, Mass.: East Asian Research Center, Harvard University; distrib. by Harvard University Press, Cambridge, Mass. 1972. Pp. xviii, 283. \$4.50.

Timothy Richard's empathy with the Chinese and his efforts on behalf of the reform and modernization of China make him one of the more attractive figures in the Western crusade to Christianize China. They also made him one of the more influential, but atypical of the nineteenth-century Protestant missionaries. Paul Bohr argues that Richard's experiences in the north China famine of 1876 to 1879 were the

source of his increasing concern with social, political, and economic reform; indeed, after his relief efforts in Shantung and Shansi, Richard turned to publicity among the literati to encourage far-reaching institutional change as the core of his evangelistic activities. Bohr says, furthermore, that the famine origins of Richard's reform program led to an emphasis on the welfare of the populace that distinguished his proposals from those of officials advocating reform because of military, economic, and ideological threats to the Middle Kingdom. Despite mission criticism of Richard's secularism, Richard found no conflict, for he believed that the advantage of Western civilization "over Chinese civilization was due to the fact that it sought to discover the workings of God in Nature, and to apply the laws of Nature for the service of Mankind" (p. 146). Richard never doubted that the Christianization of China was essential to the modernization of China even as it had been essential to the progress of the West; sympathetic as he was toward the Chinese people, his goal was to revolutionize and to destroy much of the Chinese heritage.

The scope of Bohr's work is limited, and it will alter no current interpretations of nineteenth-century China or the reform movements. Since he ends his study in 1884, Bohr does not examine Richard's influence on Liang Ch'i-chao, K'ang Yu-wei, and other reformers. Bohr summarizes rather than analyzes or assesses Richard's reform proposals. Richard's initial definition of his mission was a broad one, and his relief work seems to me to have contributed specificity to his reform program rather than to have been its origin.

Bohr, nevertheless, provides useful information and insights. His details on the devastating effect of the famine and the inadequacy of government aid vividly illustrate the reality of negative checks on population and the limitations on relief before modern technology. The Chinese response both during and after the famine stressed relief and rehabilitation in a static economy, and Bohr quite rightly contrasts this with Richard's postfamine search for means of prevention and reform. Even so, the magnitude of financial contributions by gentry and merchant as revealed by Bohr is impressive. However cumbersome and belated the government efforts at relief and however much they

were motivated by fear of rebellion, they dwarfed the efforts of Westerners. What set Richard apart as a relief administrator was his effort to develop a program ensuring equity and efficiency in distributing relief to individual sufferers. His unusual tact in working out such a program in cooperation with Chinese officials who were suspicious of his motives and who tolerated extensive inequities and profiteering help explain his later influence as reform advocate.

JESSIE G. LUTZ  
Douglass College,  
Rutgers University

HAO CHANG. *Liang Ch'i-ch'ao and Intellectual Transition in China, 1890-1907*. (Harvard East Asian Series 64.) Cambridge, Mass.: Harvard University Press, 1971. Pp. 342. \$11.00.

Despite the sizable existing literature on Liang Ch'i-ch'ao, Dr. Chang's volume is a welcome addition, for it is a forthright exposition and analysis of Liang's thought during the most creative period of his life. Throughout the book the author carefully adheres to the substance of Liang's thinking and strives to pinpoint its meaning. His explorations of the antecedents and polarizations of Liang's ideas are unencumbered by tendentious hypotheses or contrived points of view. Dr. Chang's style of writing is serious and at times ponderous, but his interpretations are marked by accuracy and authenticity.

Dr. Chang's theme is that Liang Ch'i-ch'ao, whose writings were read by every educated Chinese at the turn of the century, opened the door to a new age by preaching the inadequacy of the old Confucian order, the need for building a modern state, the urgency of national cohesion and dynamic growth, and other related messages. This theme is, of course, well known to students of Chinese history. Dr. Chang's contribution lies in giving depth to the theme by a systematic and searching analysis of Liang's writings. Especially effective are the chapters on "The Foundation of Liang's Reformist Thought," "The New Citizen," and "The New Citizen and Private Morality," in which the concept of *ch'ün* ("the group"), the ideal of citizenship, and the importance of public morality, among other matters, are dis-

cussed with such fidelity of insight that few readers can fail to see why Liang's ideas were intimately linked to the rise of modern China.

With the advent of revolution and communism Liang's writings ceased to be popular reading. Yet his ideal of the "new citizen" contained elements of enduring validity that continued to inspire leaders after him, including the Communists. This persisting impact is a matter of considerable significance for understanding the Sinification of Marxism in present-day China. In Dr. Chang's study one can easily trace the genesis of such Sinification. Thus Liang's discourses on the imperatives of civic virtue, altruism and service, on the moral vice of selfishness, on the need for struggle and progress and for activism against fatalism, on the collective rights of the people, and above all, on the importance of existential decision, motivational commitment, and self-rectification as techniques for character cultivation—all these precepts dealt with extensively in this volume are among the spiritual roots of Mao Tse-tung's programs for national reconstruction. However unconsciously, Mao is a synthesizer of the process of Sinifying Marxism, which has long been in the making. The continuity of Chinese thought in spite of foreign ideologies is one aspect of Dr. Chang's study that no discerning reader will fail to grasp.

PING-CHIA KUO  
Southern Illinois University

PAUL F. LANGER. *Communism in Japan: A Case of Political Naturalization*. (Hoover Institution Studies: 30. Comparative Communist Party Politics.) Stanford: Hoover Institution Press, Stanford University, 1972. Pp. xv, 112. \$5.95.

That American scholarship has long exercised a near monopoly of Western-language studies of communism in Japan will come as no surprise to readers familiar with the published research of Professors Robert Scalapino, Hyman Kublin, George Beckmann, Hans Baerwald, and others. It is perhaps fitting, therefore, that Paul Langer, whose pioneer *Red Flag in Japan* (1952, coauthored by Rodger Swearingen) did much to establish this peculiar, Perry-like tradition, should now offer the first monograph-length analysis of Japanese communism aimed at that protean soul, the nonspecialist (that is, general) reader.

This timely study, part of a series on non-ruling Communist parties, has three instantly appealing virtues: it is concise, clear, and reasonably up to date (including footnote references to the Eleventh Party Congress of 1970). With an expert's sure grip, Langer has managed to squeeze an impressive amount of information into seven slim chapters. He skillfully surveys the checkered fortunes of the Japanese Communist party from its origins to the present, giving decided emphasis to the party's postwar efforts to integrate itself into the changing realities of, in Scalapino's words, Japan's "post-Marxist society." The author contributes little that is new and advances no grand thesis other than the common sense one that the Japanese Communist party is gradually being forced to become "naturalized" to its environment. There is also a disturbing degree of repetition for a mere ninety-seven pages of text, though in part this may have been imposed on the author by the prescriptive format of the series (described in Jan Triska's introductory essay in suitably hyphenated and semicomprehensible "behavioralese"). Some fleshing out, finally, would have done wonders for Langer's purpose, such as adding two or three brief but representative biographies illustrating why some ten per cent of the Japanese electorate has now been drawn to communism. But these are annoyances rather than fundamental defects and should not significantly detract from the welcome this handy book will receive in many quarters. College students with awkward term papers, in particular, will likely pay Langer the ultimate campus honor of cribbing his material for years to come.

ROBERT JOSEPH GOWEN  
East Carolina University

BAIJNATH SHARMA. *Harṣa and His Times*. With a foreword by RAJ BALI PANDEY. Varanasi: Sushma Prakashan 1970. Pp. xxxii, 527. Rs. 75.

Dr. Sharma has put a great deal of labor into his book. Its value is basically that it is a compilation of all that is known about Harṣa and his times. Interesting headings do not always meet the expectations aroused, but the author has been conscientious in the collection of his material, though one doubts if he has been able to check the Chinese of Hsuan Tsang. He

is very respectful of other scholars, and the style is perfectly readable. For those scholars or libraries that do not have monographs on Harṣa the book can be fully recommended.

My real criticism of the book is that it is not better than previous works. The author's historical reconstructions are not correct any more frequently than those of his predecessors. There is also what might be called a pro-Harṣa chauvinism, but as his antagonists were also Indian this is not necessary, and it denigrates many for the sake of one. One would have liked to see Harṣa's policies interpreted in light of the Kautalyan mandala, such as his alliance with Bhaskaravarman of Assam. The reader will see in the first two chapters the extraordinary difficulty of producing order in that jungle of acts known as early Indian political history. It needs originality of approach as well as labor to make real progress. It would be most useful if Dr. Sharma were to collect and annotate all contemporary inscriptions, say from A.D. 600 to A.D. 650, a task for which he is well qualified, thereby clarifying evidence and extrapolation.

R. MORTON SMITH  
University of Toronto

K. ISHWARAN, edited with an introduction by. *Change and Continuity in India's Villages*. (Southern Asian Institute Series, Columbia University.) New York: Columbia University Press. 1970. Pp. 296. \$11.00.

DAVID G. MANDELBAUM. *Society in India*. Volume 1, *Continuity and Change*; volume 2, *Change and Continuity*. Berkeley and Los Angeles: University of California Press. 1970. Pp. xi, 323, 37, 14; ix, 328-665, 37, 14. \$10.00 each.

Recent years have produced a spate of Indian village studies. The Ishwaran essays are a collection of ten such papers with an exemplary prologue by S. N. Eisenstadt. All of the other essays describing investigations of particular villages scattered across the face of the subcontinent are first-rate. We are indebted to William H. Newell, Alan R. Beals, Gerald D. Berreman, Joseph W. Elder, Kathleen Gough, K. Ishwaran, Joan P. Mencher, Henry Orenstein, Yogendra Singh, and Stephen Tyler for their illuminating contributions. The overriding consideration of this anthology is to establish the patterns of social change in traditional and modern India.

It is apparent that the Indianess of Indian culture stems from the Brahmanic system of values. One aspect of this traditional culture is the autonomous character of the entire network of castes and villages. Theoretically standard jatis existed on a country-wide basis. This in fact was not the case. Probably the census inaugurated by the British gave the greatest impetus to the establishment of a unified social hierarchy. (This same point has been made by Thomas Metcalf in another place.)

The new pattern of change, which has been called "modernization" as opposed to "Sanskritization," advanced along various lines: economic, professional, and political. The British, of course, introduced a new polity when they created political centers, and the All-India Congress then shaped this ideology into a broader political unity.

Such developments provide the underpinning for a new type of change in Indian history. The new order was relatively autonomous in relation to the old, but it generated cultural and social change of a most significant kind. Indeed the caste order itself changed from a sectarian to an associational one. Even if the protest of regional and linguistic areas seemed to be traditional such protest was phrased in political and secular terms.

David Mandelbaum's comprehensive work stems out of his doctoral research of 1937, ideas steeped in years of field work and honed by discussions with colleagues. He deals with the same central problem that is presented in the Ishwaran essays. His first volume includes sections on "Family and Kinship Relations," "Relations among Peoples of Different Jatis," and "Relations with the Jati." A similar organization in the second volume provides sections on "Village, Region, Civilization," "Recurrent Change Through Social Mobility," "Recurrent Change Through Religious and Tribal Movements," and "Continuities and Trends."

Mandelbaum indicates certain contradictions in contemporary Indian society. For example, although the gross national product has risen steadily India has had to run hard to stay in the same place because of the absolute increase of population. Improved health measures, although they lengthen life, exacerbate the population problem. And the distribution of land, goal of the egalitarians, may in the beginning

lower production because of the lack of experience of the small holder.

The author is quite critical of Gunnar Myrdal's picture of the apathetic Indian villager. Village studies, he finds, show that the desire for status is a constant incentive to hard work. Such studies also indicate that it is not lack of initiative or the suffocating nature of caste that weighs down society. The main problem is not how to eliminate caste, but how to use productively the traditional forces of village society. Villages usually adapt traditional standards to embrace new roles and institutions.

One of the best indications of the improved position of women is the fact that widows are now allowed to remarry. The seclusion of women is less evident than formerly. The education of girls has been increasing rapidly, and many women are now employed in clerical, administrative, or professional work. These changes were probably hastened by the change in attitude of young men as they gained broader experience.

Elaborate weddings and large dowries are still common, but they serve the function of increasing social status, and even financial credit. Some marriages occur between members of different castes, but such unions are not common even among the urban and educated. Even so there is evidence that the social distance between castes has lessened. Some Harijans have become Buddhists to escape caste restrictions.

Many of the same devices seen elsewhere enable Indian politicians to manipulate votes, but a large jati cannot be ignored. Caste membership has become significant in wide political arenas, and provincial or national contests often display the same characteristics that are seen in village elections. The national politicians behave in the same manner as village politicians not because they are only the village elders translated to a higher position, but because the new political elite originates in the same civilization as the villages and holds the same values.

Mandelbaum has written a work that will become a classic, and students of modern India will overlook it at their peril.

MARK NAIDIS

*Los Angeles Valley College*

WAVELL. *The Viceroy's Journal*. Edited by PENDEREL MOON. New York: Oxford University Press. 1973. Pp. xvi, 528. \$25.75.

This handsome book is the most detailed personal account that we have of a British viceroyalty in India. It provides a minute chronicle of political events in India from October 1943 to March 1947. But two caveats need to be given. The editor, Penderel Moon, has subjected the original diary to "substantial cuts." Second, Wavell wrote this diary with one eye on posterity, and thus it needs to be read as a personal apologia.

The historian of the period will find no substantial revelations here, but there is new detail on many points. Examples are Wavell's early advocacy of a firm and public date for the end of British rule in India and the extra detail on his breakdown plan. More generally, the whole book is a case study of a Viceroy's difficulties with the cabinet in London. Churchill and Attlee both disagreed with Wavell on how to govern India, and when conflicting personalities are added it becomes remarkable that Wavell stayed in office as long as he did.

The main value of this book lies in what it reveals of Wavell as a man and as a Viceroy. He emerges as a most estimable member of the British upper middle classes—literate, humane, stolid, hardworking. He was ideally suited to be a troop commander, which is what he had been, or a minor country gentleman, but as Viceroy he was fundamentally out of place in a period when India was clearly, though with difficulty, moving toward independence.

Part of the trouble was that Wavell had too much stiff integrity and too little public charm. Mountbatten's character was the reverse. The job required a little of the "slipperiness" that Wavell so detested in politicians, and that he himself lacked. More basically, Wavell remained a soldier and an imperialist, a man lacking the ability or the imagination to approach the leaders of the national movement with any empathy. His strictures on Gandhi are sure to be widely quoted ("an unscrupulous old hypocrite," p. 353), but other attitudes are more truly revealing. Wavell was able to claim that the British had tried their best to bring Hindus and Muslims together (p. 193).

After the war the British used Indian troops to try and put down the nationalist movements in Burma and Indonesia; Wavell was unable to see why Nehru and others got upset about this. (Nor, it is interesting, can his editor, Moon.) In 1944 Wavell found that the British "were a very great nation, greater than the American, and would remain so" (p. 76). Like many other Englishmen, Wavell found the princes compatible and characterized their myopic demands as showing "moderation and realism" (p. 311). Despite the long history of tension between India and South Africa, he took an appointment as a director of de Beers soon after his retirement as Viceroy. Perhaps most indicative of all, in all the evaluations of personalities in this book, the only people praised without reservations are British, with the exception of Casey of Australia, who presumably was given honorary status as a "man" who "faced his fences."

The point is not that Wavell was a racist, for Americans and the London politicians are vigorously condemned. Rather, he simply lacked the ability to cut through the tantrums, the interminable bickering, the rudeness of Indian politicians and realize that beneath this these men were, in their own terms and toward their own country, as patriotic as he was. He was unable to understand the suspicions of British duplicity that, with some reason, most Indian politicians by now harbored. Most basically, Wavell was simply unable to appreciate the force of Asian nationalism in his time: as a result he was ineffective when he was placed in the middle of the maelstrom.

M. N. PEARSON

*University of Pennsylvania*

ROUNAQ JAHAN. *Pakistan: Failure in National Integration*. New York: Columbia University Press. 1972. Pp. ix, 248. \$12.50.

Rounaq Jahan's book is one that can be read with profit by specialists of several kinds. It is, first and most importantly, a meticulous examination of the efforts at national integration introduced during the Ayub regime (1958-69). That these efforts failed miserably in East Bengal is a thesis carefully documented by the author and underlined by history—the emer-

gence of Bangladesh in December of 1971. Second, the data on Pakistan are presented within the context of the literature on political development; one can review the "Pakistan case" in the language of the theorists. It is regrettable that, since general developmental theory is not very refined, the specific Pakistani material does not fit perfectly. The book is, finally, a splendid background to the formation of Bangladesh. For each of these themes—national integration, political development, and the emergence of Bangladesh—there will be further information and interpretations in new works, but Rounaq Jahan's seminal contribution is bound to be a major source for a long time to come.

The book includes a brief overview of the East-West Pakistan imbalances in the 1947-58 period. It then systematically analyzes the Ayub regime's integration policies in economic development, in the bureaucracy, and in local and party government, especially as these policies related to East Pakistan. It is not, of course, a balanced narrative on Pakistan. The thesis shows the failure of the central government of Pakistan to integrate its largest human sector, East Bengal, into the Pakistani nation. Rounaq Jahan is well aware that there were other objectives, such as state-building and economic development, but her thesis is that there was no more crucial objective than national integration and that if it failed, all else would. One used to argue about this, but Bangladesh settled the matter.

The quality of the author's analysis that has its most impressive impact is her evaluation of the role of the Bengali counterelite, more particularly the vernacular elite, in response to thrusts for change introduced by Ayub. My interpretation is that no matter what was done, it was either wrong or not enough. If the emphasis at one time was economic growth, then the counterelite demanded something else instead. If there was East-West parity at a particular bureaucratic level, then the "power differential," in fact, was claimed to be skewed to the West.

Rounaq Jahan's study concludes that Pakistan as a unity failed in national integration. That seems true enough. Her book is for me also good evidence (from both West and East) that it could never have succeeded; indeed, East

Bengal as a part of Pakistan was not a problem but a mistake.

RICHARD L. PARK  
*University of Michigan,  
Ann Arbor*

#### UNITED STATES

STOW PERSONS. *The Decline of American Gentility*. New York: Columbia University Press. 1973. Pp. viii, 336. \$11.95.

In recent years the quantitative study of various elite groups has become fashionable. Stow Persons has now written a more subjective, impressionistic account of one particular group, what he calls the nineteenth-century gentry elite, and its decline, interpreted largely in terms of modern mass theory and based on a limited selection of contemporary writings and secondary works that almost certainly would not satisfy the statistical-minded. Probably traditionalists will criticize him for adopting an approach that they have not yet accepted themselves, while the avant-garde will carp at the way he has done it.

Persons argues that the American Revolution destroyed the colonial gentry ruling class and that in its place there arose several different functional elites—including the gentry elite, which was composed largely of professional people—that often overlapped with the socio-economic elite in a happy combination of wealth and cultivation. Most of the examples he gives of this gentry elite come from New England, some from New York, and almost none from as far south as Philadelphia; it was a phenomenon apparently unknown in the West.

The gentility of manners of this group was joined with high cultural activity, so that the gentleman became the apostle of culture with a mission to elevate the national mind in a society marked by democratic capitalistic mobility. There would be, it was assumed, respect and status for the natural gentleman who represented an aristocracy of virtue and talent and who was often thought to have emerged out of the humble yeomanry.

Readers who prefer specific illustrations to abstract theory will find the most interesting sections to be the discussions of the gentry lady and her partial emancipation en route to

becoming the New Woman, the role of clubs for gentry social life, the rise of various academies to combat mass culture, and the impact of *The Nation* as the inculcator of correct political and cultural attitudes, as well as the excellent analysis of how the novels of William Dean Howells document the social mobility, tensions, and gentry attitudes of the day. Persons also points out that the gentry remained active in political life much longer than has been thought, even in the administrations of such implausible patrons as Jackson and Grant.

The second half of the book is the melancholy chronicle of the decline of this gentry elite as undermined by mass culture. The late nineteenth-century reform efforts of the Liberal Republicans and Mugwumps proved increasingly ineffective until the gentry turned from politics to become instead the experts who had the scientific efficiency for running the welfare state. In the universities the "moderns," such as Charles W. Eliot, hoping through greater educational freedom to produce such expert gentry leaders for a democracy, successfully undermined the efforts of the "ancients," as represented by Noah Porter of Yale, to preserve the classics as the best way to train gentlemen. With increasing pessimism Henry Adams and Charles Eliot Norton recorded the effects of mass society upon culture, documenting the predictions of Tocqueville half a century earlier and anticipating the twentieth century's alienated intellectual. Finally, there are brief summaries, perhaps not too well integrated with the preceding narrative, of modern mass theory from Josiah Royce to Philip Selznick, much of which will be unfamiliar to the traditional historian, if, indeed, there are any left.

Professor Persons, himself a distinguished example of what a New England clerical and Ivy League background can still produce, makes no effort for detailed comprehensiveness but instead has written a series of related essays, sometimes a little disjointed in effect, but often stimulating in their insights. The style is tight, dry, and abstract, seldom relieved by wit or frivolity, but always literate, though not even an Old Blue like Stow Persons has figured out how to present sociological theory with flair. And throughout there is a wistful elegiac tone

to this sad story of the departed glories and no longer relevant values of a social class.

WALLACE EVAN DAVIES  
University of Pennsylvania

RICHARD W. VAN ALSTYNE. *The United States and East Asia*. (Library of World Civilization.) New York: W. W. Norton and Company. 1973. Pp. 180. \$7.95.

In his epilogue Professor Van Alstyne quotes approvingly Hegel's remark that "peoples and governments have never learned anything from history." Yet he must have some hope that his strong revisionist statement of America's role in East Asia will do something to change the way in which that role is played in the future. He has written a brief, general work that incorporates some insights of recent scholarship. It will not tell Asian scholars anything they do not know, but it will give the general reader a sense of the whole spectrum of the American-East Asian relationship and will leave him with an illuminating if disturbing awareness of the disparity between wish and reality in the conduct of that relationship.

Professor Van Alstyne starts, sensibly, not with the *Empress of China* but with a chapter on China's traditional mode of dealing with frontier barbarians. Against this background he then discusses the seaborne Western intrusion, including the United States, and displays the problems presented by the West as something new that Chinese statesmanship, in one of its less creative periods, sought unsuccessfully to meet through repetition of the old formulas. In contrast, Japan's long isolation meant that the American incursion was more of a shock. But it also meant that, once isolation was broken down, the Japanese were not burdened by traditional policies and could deal with the Western barbarians with a realism which long eluded the Chinese. As a sort of coda to these opening chapters, the author discusses the rise of Chinese antiforeignism and of American prejudice against the Chinese, both bitter fruits of mutual misapprehension.

From that point on, the last century in East Asia is a series of variations on the themes of China's disintegration, Japan's rise as an Asian power, and European and American imperialism, ending with the emergence of the Com-

munist regime in China. Here the author is at his best, especially in depicting that sometimes incredible blend of good intentions, opportunism, and myopia through which Americans have, in Professor Van Alstyne's view, led themselves to repeated disasters in the Far East. He overstates his case and really does not give the "recent scholarship," which might add balance to his picture, a fair show. But he is never bland, and between his strong views and a lot of good, well placed illustrations, this is a lively and useful volume.

EDWARD D. GRAHAM  
Michigan State University

WARREN I. COHEN. *America's Response to China: An Interpretative History of Sino-American Relations.* (America and the World.) New York: John Wiley and Sons. 1971. Pp. xii, 242. Cloth \$7.95, paper \$3.95.

FOSTER RHEA DULLES. *American Policy toward Communist China, 1949-1969.* Foreword by JOHN K. FAIRBANK. New York: Thomas Y. Crowell Company. 1972. Pp. xiii, 273. \$7.95.

Neither Warren I. Cohen nor the late Foster Rhea Dulles conceived of these books in terms of primary research. Cohen's *America's Response to China* is part of the ten-volume *America and the World* series, which has been designed for undergraduate reading. Dulles has summarized American policies and attitudes, drawing upon a variety of printed sources. In a word, both volumes are to be regarded as syntheses, not as works contributing new knowledge.

*America's Response to China* is a small book, which, nevertheless, manages to sweep the record of Sino-American relations, 1844 to the late 1960s. Like companion volumes in the larger series, it sticks closely to the story of a bilateral relationship. As a result this is a volume offering a decidedly limited topical coverage. Professor Cohen was precluded from saying much about the Japanese or British impact on American policy. He was similarly limited in discussing the origins of American policy. This is a case of a writer's being cramped by a rigid format. What the general editor attempted was a series that could serve varying purposes in undergraduate instruction. Used individually, these volumes might supplement a basic text, or, when purchased in varying

combinations, they might provide basic reading. The series, however, has been built at the expense of its components. When viewed against the possibilities opened by scholarly research, the substance of this little book seems rather thin.

This is not to say that Professor Cohen is to be faulted for his handling of his assignment. He has met the limitations of format by outlining a few basic themes in a remarkably sure-handed way. He presents the United States as having pursued in Asia (as in Europe and elsewhere) a world in which its material interests and ideals could thrive. Translated into policy, these objectives led to American support (until 1949) of China's administrative and territorial integrity. Yet, since China was distant and tangible American interests there were few, American officialdom was unwilling to risk much in the pursuit of these objectives. The result was a policy characterized by ends that generally were beyond the reach of available means. It was a policy that encouraged Americans to deceive themselves: Senator Kenneth Wherry is puckishly quoted as assuring some constituents that "with God's help" they could "lift Shanghai up and up, ever up, until it is just like Kansas City." But, unlike some other writers, Professor Cohen is not inclined to see any great harm arising from this exaggerated self-esteem. On the whole Americans were ready to accept much less than they hoped for in China. As for China herself, she got very little out of the Americans, but neither did she especially suffer from American conduct. This view, for example, emerges in a summation of Franklin Roosevelt's policy: "Once again, the Roosevelt years demonstrated that American policy was designed to serve American interests without any particular regard for China. The fact that China too benefitted was as incidental as it was undeniable. Roosevelt's East Asian policies gave Americans no cause for grievance—and the Chinese no cause for gratitude." It is only when the narrative reaches the years after 1949 that Professor Cohen begins to criticize. American efforts to "contain" the People's Republic reflected an official concern with China that was all out of proportion with this nation's tradition. Moreover, containment set the United States for the first time in opposition to the mainstream of China's modern development.



Containment thus appears as a "great aberration" in United States policy that has proven harmful to Americans and Asians alike.

Professor Dulles assumes much the same stance, but his book has been organized on a vastly different scale. By writing at greater length and focusing on the cold-war years alone, Dulles has managed to describe the processes of policy formation. His history opens with an account of the uncertainties that confronted Harry Truman when Mao Tse-tung conquered China: in 1949-50 the United States was still bound by legacies bequeathed by its support of the Kuomintang-Nationalist government, but it had begun a search for an accommodation with China's new rulers. America's failure to remain on this path is explained in terms of American politics and Communist ambitions. No other writer has portrayed more vividly the interplay of personalities, ideas, and partisan rivalries that constituted the domestic basis of this nation's China policy: Dean Acheson, a brilliant and able administrator who was flawed in his dealings with Congress; Senator Joseph McCarthy, a marauding and unprincipled opportunist who not only gutted the State Department but who also limited the nation's intellectual responses to China's revolution; and John Foster Dulles (a kinsman for whom the author exhibits little compassion), who envisioned American restraint of China as a kind of holy crusade. The foreign origins of Sino-American difficulties are depicted in more prosaic terms. Nevertheless Dulles makes abundantly clear his conviction that the cold war was not made in the United States alone. If American officials were dead wrong in assuming that Communist China was a Soviet puppet, they are not charged with errors in estimating Russian ambitions. Dulles views the Soviet's role in instigating a Korean conflict as only a single instance of that nation's reaching too far. Nor has the People's Republic, a government that has proven remarkably obdurate and inflexible, made the settlement of Asian problems any easier.

*American Policy toward Communist China* was completed just prior to the author's death in the fall of 1970. The book demonstrates that to the very end of his life Professor Dulles retained the powers which made him one of the nation's most widely read historians. In Dulles's

hands the record of Sino-American relations becomes a story that offers lessons; his characters live, behaving wisely or foolishly; and we are vividly confronted with the dilemmas and uncertainties of those who have gone before. During his time, Professor Dulles understood better than most the importance of narrative. It seems especially fitting, therefore, that Dulles's last book should be coupled in a review with one by Professor Cohen, a younger man who exhibits much of the same understanding.

BURTON F. BEERS

*North Carolina State University*

HILARY CONROY and T. SCOTT MIYAKAWA, editors. *East Across the Pacific: Historical & Sociological Studies of Japanese Immigration & Assimilation*. Santa Barbara, Calif.: American Bibliographical Center, Clio Press. 1972. Pp. xvii, 322. Cloth \$15.00, paper \$5.75.

The tone of this volume is set by the editors in a foreword that emphasizes the success of Japanese-Americans in assimilating with the majority society. They point out that despite hypotheses based on the experiences of European migrants, the Japanese-Americans, with greater cultural and ethnic differences to overcome, experienced less familial disorganization and juvenile delinquency than did most national-origin groups from Christian Europe. More than this, despite the preachings of organized anti-Oriental factions, the Japanese showed swifter and more marked success in integration with middle-class social values as represented by educational and professional standards. It seems to be tacitly assumed that this is good.

Inevitably, in a work of this sort, the papers vary in both type and quality. The first two sections deal with aspects of the history of Japanese out-migration, from the 1868 *Gannen-mono*, who journeyed to Hawaii, to a remarkably dispassionate personal narrative of the World War II relocation experience, as seen by a sympathetic Quaker social worker. David Purcell's interesting paper on "Japanese Entrepreneurs in the Mariana, Marshall, & Caroline Islands" seems strangely out of place here, having no apparent relation to the themes stressed in the rest of the book. Howard H. Sugimoto's bibliographic essay on Japanese relocation and its impact, on the other hand, de-

serves better than its place as an appendage to Esther Rhoads's account of her experiences with relocation and its victims.

Part three, entitled "From History to Sociology," seems to involve a misnomer. T. Scott Miyakawa's treatment of "Early New York Issei Founders of Japanese-American Trade" seems a useful but pedestrian sort of historiography, with little sociological method involved. Sharlie C. Ushioda is scarcely closer to sociology in examining the "value system" of Inazo Nitobe.

Among the sociological essays is the finest example of a mixture of history and sociology, S. Frank Miyamoto's perceptive account of Seattle's Japanese-American community. The paper that follows, concerning accident-proneness among children of Oriental ancestry in the Oakland, California, area, is much more method than matter.

The general effect of the thirteen contributions included in this volume does in fact support the hypothesis advanced by editors Conroy and Miyakawa. The Japanese in America belie the facile stereotypes of both friend and foe. Their successes in the American style have not been bought at the expense of complete destruction of ethnic identity. Both Japanese and American social ethics have guided this minority group along a path toward something more than passive "assimilation." Even third and fourth generation critics of Nisei compromises reveal rather than deny that fact.

DONALD D. JOHNSON  
*University of Hawaii*

WAYNE E. FULLER. *The American Mail: Enlarger of the Common Life*. (The Chicago History of American Civilization.) Chicago: University of Chicago Press. 1972. Pp. xi, 378. \$8.95.

The mail we receive is usually taken as much for granted as the air we breathe. Mr. Fuller's history of the American postal service therefore is a welcome reminder of the central role the mail has played in our social and communications systems. The volume takes its place among the topical group included in the Chicago History of American Civilization series, along with those by Stover on American railroads, Rae on the automobile, and Condit on American building.

Precisely because the mail has been so all pervasive and influential its impact is perhaps more difficult to specify than is that of other transportation or communications media whose consequences have been more dramatically obvious. Mr. Fuller makes a convincing case, which should not be—but probably is—needed, for one of the implications of his subtitle: that the mails have been deeply involved in many if not most of the major social movements from pre-Revolutionary days to the present. To me he is slightly less convincing in respect to the second implication: that the mail served as a causal force for social change and not just as a passive responder to pressures generated elsewhere. The latter case is strongest for the early years of the nineteenth century when congressmen and others, eager to encourage Western settlement, often pressed the mails ahead of the migrating population they were intended to serve. Even here, however, it is difficult to sustain the argument. Consequently the author does not push his case very hard.

After an opening review of the development of mail service in Europe and colonial America there follows a series of chapters detailing the developing postal system's involvement in popular demands for service, in bridging as well as in aggravating the sectional conflict that spanned the middle years of the nineteenth century, in popularizing and diffusing information, in serving and on occasion rivaling the operations of private enterprise, in fostering economic and political expansion overseas as well as at home, in registering as well as censoring changes in the nation's moral standards, and in responding to the continuous pressures generated by the American political and party systems. In most instances the chapters tend to bring the story to an uneven end, roughly around World War I, with occasional references to developments in later periods. A brief epilogue (eleven pages) attempts to reunite the several themes by sketching the problems of the post office in the years following World War II as background for the 1970 law that replaced the traditional system with a government corporation.

Both the advantages and disadvantages of a topical approach are well known and well illustrated here. Individual chapters document

effectively the deep involvement of the mails in the development of their respective subject matters. The necessity of repeating chronology in each chapter makes for somewhat weary and repetitive reading, although the author does succeed in lightening the reader's burden with occasional felicitous perceptions or turns of phrase. On balance this is a valuable reference book and a useful summary of the role played by an important, virtually unknown, institution. Intended for the general reader the book could well serve as an invitation to more detailed studies and fuller analysis of the growth of America's communications systems.

MORRELL HEALD

Case Western Reserve University

FRANK RICHARD PRASSEL. *The Western Peace Officer: A Legacy of Law and Order*. Norman: University of Oklahoma Press. 1972. Pp. xii, 330. \$8.95.

For this discourse on the Western lawman, Frank Richard Prassel throws a very large noose. He brings in marshalls, sheriffs, rangers, chiefs of police, mounties and rurales from across the borders, dog soldiers of the Plains tribes, customs inspectors, postal guards, park rangers, the Secret Service, and the U.S. Bureau of Investigation hunting down draft dodgers, Wobblies, and subversives during and after World War I. Also included are the private detectives and guards employed by railroads, banks, mines, merchants, manufacturers, and cattlemen's associations; posses and deputies; and those who rode shotgun for the express companies. Today's highway patrolmen also qualify as do lawmen of the type who carried out the 1965 shootup of the Muslim mosque in Los Angeles and the synchronized raids on the Black Panther quarters in 1969.

As befits a professor of political science or, as he puts it, the "science of criminalistics" (p. 111), Prassel's approach is analytical. His subject bristles with anecdotes and he does not resist them, but his prime purpose is to assess the role, routines, work habits, and contributions of the men who wore the badge. Because they constitute such a miscellany, generalization or even a clear prototype is difficult to construct. In the end the crux of Prassel's argument is that these lawmen were working

much as their counterparts do today, facing no more crime and using little more violence.

Folksay, he admits, runs to the contrary, and, like President Nixon, he puts the blame on the media and particularly television (also the movies, which apparently Nixon does not watch). Prassel points out that the novelists such as Wister, Rhodes, and Clark have not played up the Western peace officers as trigger-happy gunslingers. Early journalists did to some extent, but it was left for "Gunsmoke" to give this myth its ultimate currency. He insists too, though without explicit analysis of recent historical writings to prove it, that we historians of the West have also been infected by the myth of law and order achieved by right prevailing in those duels in the sun. His book will not stop the reruns of "Gunsmoke," *High Noon*, or *True Grit*, but it has much food for thought and is a useful counterbalance to runaway writing on the lawmen of the West.

JOHN CAUGHEY

University of California,  
Los Angeles

THOMAS HARRISON BAKER. *The Memphis Commercial Appeal: The History of a Southern Newspaper*. Baton Rouge: Louisiana State University Press. 1971. Pp. 336. \$12.50.

JAMES PLAYSTED WOOD. *The Curtis Magazines*. New York: Ronald Press Company. 1971. Pp. ix, 297. \$7.00.

Writing the history of a journalistic enterprise is not an easy task. On the one hand, one must resist the temptation to string together snippets and summaries of editorial opinion and then to offer them up as the history of a newspaper or magazine. On the other, too much concern with the internal machinations of ownership, finance, and managerial politics will tend to make the history little different from that of any other business enterprise. Of these two books, Thomas Harrison Baker's study of the *Memphis Commercial Appeal* occasionally veers too much in the first direction, while James Playsted Wood's history of the *Curtis* magazines leans too much toward the second.

Founded as a Jacksonian Democratic news-

paper in predominantly Whig Tennessee, the *Memphis Appeal* (it did not become the *Commercial Appeal* until 1894) vibrated from a states'-rights position to one of moderation and sectional conciliation, and then back to support of secession on the eve of Lincoln's inaugural. In 1862, forced to leave Memphis ahead of the Union Army, it began a vagabond existence that carried it to many Confederate cities before its return home in 1865. The Reconstruction years saw a predictable stance in favor of white supremacy and the Democratic party, positions it did not abandon for more than sixty years. After 1894 the *Commercial Appeal* became a staunch opponent of free silver, an equally fervid supporter of U.S. imperialism, and, under the editorship of C. J. P. Mooney (1908-26), an ardent booster of Woodrow Wilson's foreign and domestic policies.

It was during Mooney's editorship that the *Commercial Appeal* took on Boss Ed Crump and the seemingly indestructible political machine that ran Memphis and sometimes Tennessee for nearly forty years. It was a losing battle, and eventually the paper backed off. (In retrospect, Crump's major offenses seem to have been his encouragement of black voters and his support of publicly-owned utilities.) In 1936 the *Commercial Appeal* was bought by the Scripps-Howard chain, and here Baker seems to lose interest in his subject, devoting only eight pages to the years since 1941. No explanation is offered, and the reader is left wondering about the role of the *Commercial Appeal* in the postwar period that saw so much Southern political and social history being made.

It is unclear why the *Commercial Appeal* is worthy of attention. With one or two exceptions, it never challenged any basic Southern belief nor did it speak for any particularly discrete constituency. It was not comparable in its impact with the newspapers of New Orleans, Atlanta, Richmond, or Louisville. With the possible exception of the Mooney years, it never represented the personal journalism of a Watterson, a Grady, or a Daniels. Whatever impact the *Commercial Appeal* did have is often obscured by Baker's inclusion of much editorial trivia in many chapters, though there are excellent summaries of the changing na-

ture of newspaper journalism and how the *Commercial Appeal* responded to them.

If Baker has possibly overestimated his subject, the same cannot be said for Wood's 290-page study of the Curtis magazines and the publishing empire associated with them. No one interested in the shaping of middle-class America in the first half of this century can ignore the importance of the *Ladies' Home Journal* or the *Saturday Evening Post*. Not only did they lead the field in circulation and advertising, but they pioneered in the areas of motivational research and consumer preference surveys, now accepted institutions in the advertising world. Wood, himself a former Curtis employee, makes clear his admiration for Cyrus H. K. Curtis, Edward W. Bok, and George Horace Lorimer, who founded and created the Curtis empire and bequeathed it to—in Wood's opinion—lesser and shakier hands in the 1930s. Wood draws upon his inside knowledge of Curtis history to describe the blunders and miscalculations of the last ten years of the original company, though the alternatives are never made clear. He is critical of the company's refusal to diversify into other media, yet such diversification by the rival Luce and Cowles empires did not save *Life* or *Look* any more than it would have saved the old *Saturday Evening Post*. The age of the mass-readership feature magazine, thick with advertising and full of reassuring pats on the back to Middle America, could not have lasted forever in the fact of competition from the electronic media. As in most cases, however, what doomed the old *Post* and *Journal* was not declining circulation but a lack of faith among its advertisers. The collapse of the Curtis empire is all the more stunning when one reads of the days in the 1920s when advertisers stood in line for the opportunity to display their wares in either the *Post* or the *Journal*.

Unfortunately, Wood's book is marred by what is usually referred to as a "racy" journalistic style of writing that does not measure up to the importance of his subject. The comings and goings of obscure managing editors clutter up the pages unnecessarily. Documentation is sparse. Whereas one of the weaknesses of Baker's study of the *Commercial Appeal* is his steady diet of editorial summaries and quotations, we are told little of the edi-

torial stance of the Curtis magazines except that it was conservative and grew increasingly anachronistic. We would like to know more about the impact of the *Ladies' Home Journal* upon American women and their self-image, but Wood passes up this opportunity as well. And finally, though he is an admirer of the Social Darwinism that typified so many businessmen of Curtis's and Bok's era, Wood omits notice of the irony of the Curtis empire being done in at last by the very values it tried to cement into the American mind: balanced budgets, mass advertising, and competitive journalism.

LYNN HUDSON PARSONS  
State University of New York,  
College at Brockport

ROBERT A. RUTLAND. *The Newsmongers: Journalism in the Life of the Nation, 1690-1972*. (Two Centuries of American Life: A Bicentennial History.) New York: Dial Press. 1973. Pp. xv, 430. \$12.50.

Robert A. Rutland has written a good and useful survey history of American journalism in the tradition of Frederic Hudson, Willard G. Bleyer, Frank Luther Mott, and James Melvin Lee. Former reporter, professor of journalism and of history, and now editor of the James Madison Papers, Rutland covers the subject well, considering the definition of journalism he set for himself: the "spoken, printed or visual report of timely interest to a mass audience." All the giants are here—from Benjamin Franklin, Hezekiah Niles, and Benjamin Day to James Gordon Bennett, Horace Greeley, Harry J. Raymond, Joseph Pulitzer, William Randolph Hearst, and Joseph Medill Patterson—as the newspaper evolved from its publication by the entrepreneurial printer-editor to the great corporations that today are attempting to meet the stiff competition of radio and particularly television. Rutland effectively, though unobtrusively, melds in the changes in newspaper technology and the working conditions of newspapermen. His clearly written, swift-flowing narrative sparkles with a sprightly style enhanced by straightforward, sometimes even blunt, language. He is especially good in weaving the main historical events of the time into his account, revealing how often the newspaper has been a clear reflection of American life

and thought. By using descriptive details and striking quotes from editorials and news columns, along with new facts and interpretations, the author underscores the significance, or heightens the drama, of men and events. Along the way he suggests areas worthy of further study. His research is based on excellent secondary sources, numerous newspapers, and many of the more recent scholarly studies. In a provocative final chapter Rutland raises, though he does not answer, many of the right questions.

Contrary to Rutland's assertion that his work is not a conventional history, it is just that, since most of the basic problems and issues newspapers and newspapermen have faced are left virtually untouched, for example, the influence of the advertiser and the newspaper business office, as well as that of the publisher himself, on political, social, and other views expressed by the press. Slanted news Rutland would have us believe only emerged with the appearance of *Time* and *Newsweek* magazines in the 1920s and early 1930s, and that only in 1933 did the debate on newsroom philosophy really begin.

By taking up weeklies such as *Time* and *Life* and monthlies like *McClure's* and *Reader's Digest*, Rutland can be accused of not having included all such types. Other aspects left out or hardly mentioned include the development of the foreign correspondent, the decline of the editorial and religious and ethnic newspapers, and the development and significance of the cartoon and the letters-to-the-editor feature. Finally, his research shows little use of trade journals, such as the *Journalist*, the *Fourth Estate*, and *Editorial Publisher*, and he utilizes only a few histories of individual newspapers and works by newspapermen themselves.

ABE BORTZ  
Social Security Administration,  
Baltimore

BARBARA GUTMANN ROSENKRANTZ. *Public Health and the State: Changing Views in Massachusetts, 1842-1936*. Cambridge, Mass.: Harvard University Press. 1972. Pp. 259. \$9.00.

J. DOUGLAS BROWN. *An American Philosophy of Social Security: Evolution and Issues*. Princeton: Princeton University Press. 1972. Pp. x, 244. \$8.50.

Several excellent monographs have been published in recent years dealing with the history of public health. These include John Blake's *Public Health in the Town of Boston, 1630-1822*; Charles Rosenberg's *The Cholera Years*; James H. Cassedy's *Charles V. Chapin and the Public Health Movement*; and John Duffy's *A History of Public Health in New York City*. Apart from the focus on Massachusetts and the evolution of its Board of Health, the Rosenkrantz volume does not add much to the fund of knowledge. There are some useful biographical accounts of health reformers and administrators: Henry Shattuck, Edward Jarvis, Henry Bowditch, George Derby, and Henry Pickering Walcott (chairman of the Massachusetts State Board of Health, 1886-1914, and "prototype of the physician-statesman"). But the book suffers from the absence of a significant conceptual framework.

I gather the author was trying to say that nineteenth-century public health reform was permeated by humanitarian and moralistic zeal, as compared to twentieth century "substitution of scientific for ethical objectives" and "explicit denial of responsibility for social reform." This line of analysis, while perhaps valid, is also platitudinous. As social or intellectual history, the book's value is limited to variations on a familiar theme.

Public health problems forced Americans in the nineteenth century to expand the welfare and service functions of government. It was not until the 1930s, however, that a relatively coherent, national system of economic security was established. *An American Philosophy of Social Security* is by an economist, a participant in the drafting of the Social Security Act in 1935 who has remained active in the formulation of policy and legislation. Brown deals primarily with the contributory programs, exclusive of unemployment insurance. These include old-age, survivors, and dependents benefits, disability insurance, and Medicare. Although he is concerned mainly with contemporary policy issues, his study has utility for the social welfare historian. Brown frequently refers to constitutional and political factors of the 1930s that influenced the content of the Social Security Act—a dramatic product of American social politics which bal-

anced expediency with long-range objectives.

While useful to historians, the book is indispensable to those concerned with contemporary welfare policy. Brown explores many fundamental issues: the relationship between the social insurances and public assistance; risks appropriate to the social insurance mechanism; the financial role of the federal government; criteria for determining individual and family benefits; the expansion of Medicare into a broader health insurance system. He recommends many improvements in the benefit structure but always in the context of political and economic feasibility.

Brown's view of the social security system as an organic historical phenomenon is akin to a legal scholar's view of the constitution. It must respond to changing circumstances but its integrity must be protected. This means that its functions should not be confused with those of public assistance and that it should not be burdened with costs or responsibilities inappropriate to a wage-related insurance program. It should not be used, for example, to compensate for low wages or confused with the private retirement annuity. The system is designed not to replace lost earnings in their entirety but to replace "that degree of loss which is socially undesirable." This policy justifies weighting benefits in favor of lower-income contributors, but it does not justify using the social security system as a substitute for effective national employment policies.

It is unfortunate that politicians cannot be required to read this volume; piling payroll costs and benefits in a ritualistic fashion onto the social security system has become fashionable as a means of buying votes every two years. The danger is that political expediency might blur the distinction between social insurance and public assistance or might otherwise compromise the distinctive equity, the contributory, contractual, and wage-related character of the social insurances. "Because social insurance is effective in the limited burden for which it is intended," Brown cautions, "does not justify placing burdens upon it which distort its purpose and endanger its acceptance."

ROY LUBOVE

*University of Pittsburgh*

KENNETH J. BERTRAND. *Americans in Antarctica, 1775-1948*. (American Geographical Society, Special Publication number 39. Published under the support of the National Science Foundation.) New York: the Society. 1971. Pp. xvi, 554. \$25.00.

In an age like ours of hurriedly written publications it is a pleasure to encounter a work of thorough scholarship. Professor Bertrand, a geographer, has had a long association with his subject, beginning in 1946 with his assignment to the Special Committee on Antarctic Names under the Department of Interior and an earlier publication, *Geographic Names of Antarctica* (1956). Now, in the work under review, he offers a new departure point for readers interested in American Antarctic exploration. Previously the choices were encyclopedic digests, selected chapters on various expeditions, collected biographical sketches of certain explorers, or the published monographs of particular explorers. Serious readers may forthwith begin with Bertrand's work and proceed to the desired depth of study under his guidance.

Using the Antarctic Convergence as the physical boundary for his study and the activities of the eighteenth-century American sealers as the chronological beginning, Bertrand carries his scholarly summary of both American exploration and scientific investigation through the exploration year 1947-48. The resulting twenty-four chapters are well proportioned, and their varying lengths provide a rough quantitative measurement of the significance of each explorer or expedition.

Controversy is a characteristic in the history of Antarctic exploration and scientific investigation. Who first discovered Heard Island? Did the United States Exploring Expedition under Charles Wilkes actually see the "eastern land-falls" and, hence, prove the existence of the Antarctic continent? Is Antarctica one or two land masses? Why are the Bunger Hills ice free? Bertrand treats judiciously these representative controversial questions.

Especially useful are both the notes, sometimes extensive, and the bibliographies appearing at the end of each chapter. While the bibliographies may not be definitive they come close to that unattainable ideal. Of particular value are the range and depth of manuscript

material comprising letters, journals, logbooks, ship registrations, crew lists, custom house records, and government reports.

Lastly, two deficiencies are manifest. Ironically the twenty-six maps supplementing the text are insufficient. With the multitude of place names cited, there is the need for one or two large, detailed folding maps in keeping with the cartographic standards of older American Geographical Society publications. And given Professor Bertrand's long association with his subject one may only wish that he had provided more interpretation. While each chapter commences with the significance of its subject the result is fragmentation, and one misses the larger interpretation that the erudition, reflected within the book, might have provided.

WALTER L. BERG

Central Washington State College

EDGAR J. MCMANUS. *Black Bondage in the North*. [Syracuse:] Syracuse University Press. 1973. Pp. xiii, 236. \$9.95.

Professor McManus's volume examines slavery in New England and in the Delaware and Hudson valleys—its origins, its place in the Northern colonial economy, its legal underpinnings, how it operated as a system of servitude, the ways in which slaves reacted to it, and its ultimate decline in the late eighteenth century. Those familiar with Lorenzo J. Greene's *The Negro in Colonial New England* (1942), McManus's own book on slavery in New York, the more recondite studies of slavery in Pennsylvania and New Jersey, Benjamin Quarles's *The Negro in the American Revolution* (1967), and Arthur Zilversmit's *The First Emancipation* (1971), will find little new information. Yet the author presents us with a useful and thoughtful synthesis.

On the whole this volume is factual rather than analytical or interpretive. In fact the preface informs us that "an attempt has been made to tell the story with a minimum of generalization and interpretation. . . . Since this is a pioneer [*sic*] study, it seemed more important to define factual boundaries than to plunge into theoretical thickets." Yet in a concluding chapter McManus plunges into the thicket of comparative slavery in the New World. Adopting a rather simplistic economic

interpretation he concludes that differences in the character of slavery were based on the type of work that slaves performed. Black artisans with specialized skills, such as were unusually common in the colonies analyzed in this book, possessed "leverage" that secured them "a privileged position within the system." Similarly, though in my judgment his evidence would not fully support his conclusion, McManus dismisses the ideological factors emphasized by Zilversmit in explaining the abolition of slavery in the North. Instead McManus attributes the demise of the institution there simply to a decline both in the economic importance of slavery and in the proportion of blacks in the population.

Although one must thus enter reservations concerning the analytical sophistication of this volume, and although the author has unfortunately not included a discussion of slavery in the Old Northwest, he does offer a long-needed factual synthesis of the subject, based upon a wide array of monographic and primary sources.

AUGUST MEIER

Kent State University

JAMES ROBERT ENTERLINE. *Viking America: The Norse Crossings and Their Legacy*. Garden City, N.Y.: Doubleday and Company. 1972. Pp. xix, 217. \$6.95.

FREDERICK J. POHL. *The Viking Settlements of North America*. New York: Clarkson N. Potter; distrib. by Crown Publishers, New York. 1972. Pp. xii, 339. \$7.95.

Our knowledge of Scandinavian contacts with the North American continent before Columbus is based upon two different kinds of information. One consists of scattered references found in Icelandic and medieval European sources and certain late medieval and early modern maps, of which the two well-known *Vinland Sagas* are the most important. The second is composed of archeological evidence from Greenland, Northern Newfoundland, the Ungava area of Labrador and certain Canadian Arctic Islands, some of which is relatively recent and still difficult to interpret. There have also been a few isolated archeological finds of Norse material beyond the above northern American area of which only that uncovered in the James Bay region of Northern Ontario has been accepted without question. Furthermore the major written sources for our knowledge of such

contacts, the *Vinland Sagas*, suffer from the fact that they relate events that took place two centuries before they were written down or composed by men who had no direct knowledge of the geographic areas they were describing.

Both of these books by Enterline and Pohl attempt to use this scattered and often unsatisfactory material to present us with a coherent picture and both share an apparent belief that the *Vinland Sagas* present an accurate picture of parts of the coastline of the North American continent, which were visited and briefly settled by Scandinavians around the year 1000 A.D.—surely a rather dangerous assumption. If one accepts, however, the general accuracy of this saga evidence, and there is some reason why we should be willing to do so in the light of archeological discoveries, much of Enterline's book seems relatively satisfactory and his initial thesis quite probable.

This thesis is that the *Vinland Sagas* show that the Helluland, Markland, and Vinland they mention were located along Baffin Island and the Ungava Bay area of northern Labrador. Especially probable is his contention that somewhat later on in the twelfth, thirteenth, and fourteenth centuries the Scandinavian Greenlanders of the northern settlement who included many hunters advanced north along the Greenland coast and proceeded into the central Arctic by way of the strait between Devon Island and northern Baffinland towards the Coppermine River area of Canada. There is archeological evidence of their presence along this route.

His attempt, however, to link the disappearance of the inhabitants of this northern settlement later on with their absorption into an Eskimo population advancing east from Alaska seems more doubtful. So, too, is his thesis that cartographic features depicted on the Vinland and other late medieval and early modern maps were derived from knowledge initially provided by these same Eskimos. We would do well also to question seriously his contention that there was a knowledge of the Labrador coastal region in Western Europe before Columbus, which was derived from contacts with the dying fourteenth- and fifteenth-century Greenland colony—though this is possible.

On the other hand, Pohl's attempt to link



evidence from the *Vinland Sagas* to a series of specific locations along the eastern Atlantic seaboard of North America seems much more unsatisfactory, especially since none of the archaeological evidence he provides can be termed conclusive, nor can one accept his reading of the Vinland map as definitive. Perhaps all that can fairly be said of his thesis, as advanced in this and other books, is that it may turn out to be true but must at this time be termed extremely doubtful until more concrete evidence is provided for us.

ARCHIBALD R. LEWIS  
University of Massachusetts,  
Amherst

LARZER ZIFF. *Puritanism in America: New Culture in a New World*. New York: Viking Press. 1973. Pp. xiv, 338. \$10.00.

In yet another attempt to understand the Puritans, Larzer Ziff has traced, perhaps more effectively than any previous writer, the subtle matter of interrelationship between word and act, minister and people, New England theology and New England behavior. Viewing Puritanism as a particular body of ideas held by dominant members of society, influencing and reflecting the way men lived their common lives and reacted to their daily problems, he defines this combination of ideology and social pattern as Puritan culture, a concept that provides the theme for the book. Not a factual history, it is, rather, a discursive, often impressionistic, interpretation.

Puritans, as Ziff sees them, were liberated from both corrupted church and government by their sense of being a chosen people possessing the potential of saving grace and preserving the pure word of God. Such a belief met the needs of their condition, providing them with a new culture in an age when thousands found themselves masterless and landless in the face of economic forces and political orders that did not satisfy. In the New World where they hoped to construct a better social, economic, and political order they shaped an ideology that was appropriate to their conditions of life.

How the Puritan applied his ideology is the story of Puritan culture in New England. The author ranges with searching intelligence and a profound knowledge of New England's his-

tory to show how Puritans reacted to domestic problems, like the antinomians, Roger Williams, the Indian wars, or the witchcraft outbreak; how they thought on abstract matters like love and death; and what their response was to developments abroad like the Civil War, the recall of their charter, and the wars of empire.

Changes that came in the colony are linked with changes in the thinking of the leaders. Offering a number of challenging and revealing interpretations on subjects like the witchcraft frenzy and the Great Awakening in New England, the author also hazards a few conjectures about the persistence of Puritan culture in later American life and letters.

The author's method may be adequate for literary scholars, but historians will want better evidence than inferences about ordinary life in the colony drawn from the writings of leaders. Rich and metaphorical in language, Ziff's literary figures will not satisfy those who seek precision in meaning, not to mention quantitative data. He acknowledges reliance on the work of many scholars, but mentions only Palfrey, the Adamses, and Miller. In this largely interpretive study one looks in vain for the insights of the behavioralists. The author's interpretation rests often on appearances, the mere correspondence of ideas and actions. Readers without an extensive background in the subject will find this book hard going, although its grace and subtlety do, in the end, appeal.

GEORGE M. WALLER  
Butler University

SEYMOUR VAN DYKEN. *Samuel Willard, 1640-1707: Preacher of Orthodoxy in an Era of Change*. Grand Rapids, Mich.: William B. Eerdmans Publishing Company. 1972. Pp. 224. \$5.95.

Samuel Willard was clearly an important character in Puritan New England. From his position as pastor of Boston's South Church, and, later, as acting president of Harvard College, Willard spoke out on the major religious-political issues of his day. He became personally involved in many of the important events he witnessed. Seymour Van Dyken pictures Willard's public life—his sermons, writings, participation in events—as an effort to defend the

orthodoxy of the founding generation in a way that would adapt it to each contemporary crisis without compromising its fundamental principles.

It is this approach to understanding Willard's life that dominates Van Dyken's book. His view of Willard as a preacher of orthodoxy in an era of change, it seems to me, is much like that of denominational historians who portray the reactions of their small denomination to events in the world around them as the history of their group. For much of his book (certainly the first half) Van Dyken's goal is to explain how Willard fit into and effected his "era of change." The author's approach tends ultimately to distort the context in which historical events occurred and to exaggerate the separation between Willard and the larger community in which he lived. The result is a rather separated and repetitious account of seventeenth-century Puritan history. Unable to integrate his subject and his subject matter, Van Dyken loses the chance to use his biography to raise meaningful questions, even if only indirectly.

The second half of the book is more satisfying. The final five chapters are a straightforward discussion of religious doctrine. As a history of ideas, it is very well done. Van Dyken offers us in detail, and with skill, an account of the theological principles of late seventeenth-century orthodoxy as represented by Samuel Willard. The latter part of the book is of considerable value to historians.

DAVID KOBIRIN

*State University of New York,  
Albany*

*A. History of Missouri. Volume 1, 1673 to 1820, by WILLIAM E. FOLEY; volume 2, 1820 to 1860, by PERRY MCCANDLESS. (The Missouri Sesquicentennial Edition.) [Columbia:] University of Missouri Press. 1971; 1972. Pp. ix, 237; ix, 325. \$9.50 each.*

The above volumes are the first two of the five-volume Missouri Sesquicentennial History, a joint project of the University of Missouri and the University of Missouri Press; the remaining three volumes are projected for publication by 1976, the 150th anniversary of Missouri's statehood. The authors of volumes 1 and 2, William E. Foley and Perry McCandless, are both professors of history at Central Missouri

State College at Warrensburg. William E. Parrish of Westminster College at Fulton is the general editor of the series, the purpose of which is the presentation of a broad survey of the political, social, economic, and intellectual trends that have dominated the state's history. With only five hundred pages of printed text allotted to the two authors they have done well in their attempt to allocate the space and still present a balanced account of all phases of the state's early history. Though concise and straightforward with little detail, the presentation is still interesting and readable. One wishes there had been space for more detail, but on the whole I feel the authors have fulfilled their assignment well.

The absence of footnotes, except for a few of an explanatory nature, will prove a disappointment to the serious student but will no doubt meet the approval of the general reader. Sources used in the preparation of the volumes are listed in the bibliographies at the end of each volume; these are also intended as a guide for additional reading.

Professor Foley has chosen to devote only three of his ten chapters to the period before 1803 when Missouri (or "Upper Louisiana" as it was first called) was successively under French and Spanish control and instead has placed the main emphasis on the last seventeen years of the somewhat turbulent territorial period. This treatment is in contrast with the pre-statehood histories of the states east of the Mississippi in which much space has been given to the French and British occupation. The principal result of the Spanish occupation was the land grants that were made; the claims that evolved out of these grants continued to plague American officials for many years before they were finally settled.

The interesting constitutional questions that arose over the purchase of the Louisiana Territory by the United States and the setting up of a new government for the region form one of the best chapters in volume 1. The significance of Missouri as the gateway to the West was foretold by the presence of Meriwether Lewis and William Clark in St. Louis in March 1804. They were making preparations for their expedition up the Missouri at the same time that the American flag was being raised in the city, signifying the transfer of the area to the

United States. These and other explorers would in time be followed by those seeking fame and fortune in the West, and finally by the Conestoga wagons filled with emigrating families. The unique geographic position of the state, strategically located between the East and West, was to prove helpful to its later development. Similarly, however, its central position between the North and South caused Missouri to become deeply embroiled in the slavery issue.

Professor McCandless begins his volume with Missouri's struggle for statehood, the approval of which was delayed for almost two years by the debate in Congress over the slavery issue, finally resolved by the Missouri Compromise. Again in the late 1840s and in the 1850s, during the debates over slavery in the territory gained from Mexico and the admission of Kansas and Nebraska, Missouri was drawn into the maelstrom of a national issue that had an effect on local problems.

In between his consideration of the slavery issue Professor McCandless had the opportunity to discuss other political issues and their exponents, including the emergence of Jacksonian Democracy in the 1830s and its continued supremacy down to the time of the Civil War. Other chapters deal with the people who made up the state (most of whom came from the South), their occupations, the cities they built, their search for better educational opportunities, the emergence of a feeling of responsibility for those less fortunate, and their ability to express themselves in literature and in art.

The typography used in the volumes is pleasing to the eye and adds much to their readability. The indexes seem inadequate; for example, in volume 2 there are sixty page references to Thomas Hart Benton without any subheads. The volumes would have been enhanced by the addition of illustrations and maps; the one or two maps that appear are too small to be of help.

DOROTHY RIKER

*Indiana Historical Society*

*History of the Supreme Court of the United States. Volume 1, Antecedents and Beginnings to 1801.* By JULIUS GOEBEL, JR. (The Oliver Wendell Holmes Devise.) New York: Macmillan Company. 1971. Pp. xxv, 864. \$30.00.

At long last the Holmes Devise series of the Supreme Court is launched, and rather uniquely, with Julius Goebel's volume on antecedents and the pre-Marshall Court. A product of more than a decade of intensive research and a lifetime of general scholarship, Goebel's *magnum opus* will titillate the specialist, but frustrate the generalist. Crammed with little or unknown facts about the origins and procedures of the judiciary in America, as well as rich in unadulterated legal history, the book is surprisingly thin on the substance of the Supreme Court itself.

In many ways Goebel's book is legal and constitutional history in its best and purest form. He provides rich detail on the reception of common law in America, the traditions of colonial judicial control over legislation, the evolution of judicial review, the position of the judiciary in colonial and revolutionary America, the debates about the judiciary in the drafting and ratifying conventions, the conflict over the Judiciary Act of 1789, the ingredients of the process acts, and the procedural business of the circuit courts. The freshest part of Goebel's study, and the most exasperating, concerns practice and procedure before early American courts—colonial, state, and federal. Even the heretofore obscure process acts that accompanied the Federal Judiciary Act of 1789 are examined with remarkable thoroughness. From this patient viewing Goebel concludes that the federal statutes were significantly influenced by a variety of state procedures. For example, Congress, influenced by state precedent, departed from the English rule that service had to be made in person and instead provided the rule that service could be perfected by leaving a copy of the process at the defendant's abode. But unlike state provisions, section 7 of the Senate committee's initial process bill made jury trial available to either party "regardless of the nature of the action, and [the] defendant was specifically given the opportunity to set aside the default judgment simply by an appearance within the instant term and payment of costs" (p. 530). One suspects an eighteenth-century lawyer, faced with innumerable technical hurdles and ceremonial pitfalls would have emerged from federal court battle unscathed after a single reading of Goebel.

Other sections, in spite of their awesome particularity, lack contextual substance. Goebel's discussion of the circuit courts provides much information about the revolt against circuit riding, original civil jurisdiction, judicial review of state and federal statutes, removal of cases from state to federal courts, and criminal and admiralty jurisdiction, but little about the tribunals in the constitutional reality and politics of the times (save for a brief section on "The Attacks on the Judicial"). How extensive was the business of the courts? How did the litigating public respond to the circuit courts? Did the courts favor absentee landlords and foreign creditors? What was the place of the courts in the Federalist-Republican conflict?

A more serious deficiency in a book constituting the first volume of a multivolumed history of the Supreme Court is the rather shallow treatment of the business and significance of the high tribunal itself. Only 71 of 793 pages are devoted to what the author terms the Court's "political and constitutional issues," with another 59 pages concerned with rather technical questions of jurisdiction and procedure. Illustrative of the book's shortcomings in this area is Goebel's treatment of perhaps the most important case of the Court's first decade, *Chisholm v. Georgia*. The author spends too much space on summaries of counsel's arguments and opinions of the justices and not enough on the background and settlement of the conflict. Goebel would have profited from a reading of Doyle Mathis's excellent studies of the case published in 1967 and 1968.

Surprising also is Goebel's failure to utilize expeditiously the Jay Papers housed at his home school, Columbia University, and the short but revealing study of Jay by his colleague, Richard B. Morris. Because of this inattention the book is void of material on Jay's accomplishments as a presidential adviser and judicial administrator as well as on the impact of his political and judicial philosophy. Greater use of the papers, too, might have furnished more information on the internal proceedings of the Court, including the give and take of judicial politicking on and off the bench.

It perhaps follows that with Goebel's preoccupation with roots and procedure he fails

to define adequately the significance of the Court as an institution in the first decade of the new nation. Was the high tribunal simply a preliminary footnote to the Marshall era, as earlier historians have suggested, or important in itself, as more recent scholars argue?

Those historians not trained in the law will find this study hard going. And those scholars interested in determining the place of the Court in the constitution and politics of the first decade of the new nation will be disappointed.

ROBERT M. IRELAND

*University of Kentucky*

THOMAS H. POPE. *The History of Newberry County, South Carolina*. Volume 1: 1749-1860. Columbia: University of South Carolina Press. 1973. Pp. xvii, 389. \$17.95.

Local history ought to be history at the grass-roots, detailing the social, economic, and political development of a single community and providing a basis for generalizations about regional and national trends. Thomas H. Pope has written an outstanding example of local history that transcends parochialism and family pride. His study of Newberry County in its transition from frontier to plantation will be of real value to historians. It is grass-roots history, written in the context of regional history and based on thorough research.

Newberry County was created in 1785 from an area in the fork of the Broad and Saluda rivers. It was settled by Scotch-Irish and German pioneers, coming from Pennsylvania, Maryland, and the Shenandoah Valley of Virginia. German-speaking Brethren (Dunkers) from Maryland settled in the Dutch Fork and Quakers along Bush River. The first Baptist church was organized from a Rockingham County, Virginia, congregation.

The author describes the back-country society of the eighteenth century and the county's share in the Cherokee War and the regulator movement. The county was the scene of partisan warfare in the Revolution, with 500 men from the Dutch Fork enlisting in the South Carolina Royalist Regiment.

From a region producing flour, beeswax, furs, tobacco, cattle, and hides for market, Newberry County shifted to a cotton economy in the nine-

teenth century. Between 1800 and 1860 the white population dropped from eleven to seven thousand and the slave population rose from two to fourteen thousand. The city of Newberry became the leading inland cotton market of South Carolina, as well as early rail center.

Although written during the leisure moments in a busy career of an attorney and state legislator, Pope's book can serve professional historians as a model in the imaginative use of local records. The author has also used monographs to good advantage. Unfortunately the recent study of the Church of the Brethren in the Carolinas by Roger Sappington of Bridgewater College appeared too late for inclusion in the book, and Pope's treatment of the Dunker settlement is thin by comparison.

The book is written in an attractive style and enriched by handsome photographs of county landmarks, mainly dating from the antebellum period, but including a few earlier buildings as well.

RICHARD K. MACMASTER  
*Bicentennial History Project*  
*Montgomery County, Maryland*

WILLIAM B. WILLCOX *et al.*, editors. *The Papers of Benjamin Franklin*. Volume 16, *January 1 through December 31, 1769*; volume 17, *January 1 through December 31, 1770*. (Sponsored by the American Philosophical Society and Yale University.) New Haven: Yale University Press. 1972; 1973. Pp. xxiv, 359; xxxii, 430. \$17.50 each.

As the massive project of assembling the Franklin Papers approaches its halfway mark, the character, the mind, and the temperament of the man shine through every volume; and there also shine through every volume the superior scholarship of the editors, their lucid insight, and their literary skill.

The editors continue to resist any temptation to use the edition to construct an exegesis of Franklin's writings, to impose upon them extraneous interpretations, or otherwise to encumber them with obiter dicta. These volumes, like their predecessors, evince the editors' constant awareness that their function is wholly different from that of the historian. To have value, history must distill meaning from the experience of a people or an era. Its distinction consists not only in the quality of its scholar-

ship but also in its validity as what is essentially a work of art, in that—like all works of art—it is the product of a gifted or knowing way of looking at things, of giving point and emphasis to reality. The task of the editor of documents and papers is quite different: to gather and present in useful form the raw materials of history. He must not intrude upon the substance of those materials. At the same time he must place them in time and in context, identify often obscure individuals, events, and loci, and furnish frequently elusive bibliographical data—all this in fidelity to a consistent editorial design and a reliable methodology, while scrupulously allowing the papers to speak for themselves. Certainly no editors have taken a more disciplined view of their high function and carried it out with more professional skill than the editors of the Franklin Papers.

The present volumes embrace two events of the utmost significance in the life of Franklin, in the imperial scheme of the British establishment with which he was negotiating, and in the vexations of the American colonies, four of which he was representing by 1770. The first of these was the collapse of the brief, hapless ministry of the duke of Grafton, followed by the long, catastrophic ministry of Lord North, after whose rise to power Franklin's views underwent a significant change: he denied explicitly, as he had not done before, the claim of Parliament to legislative authority over the colonies, his position being that, like all Britons, Americans were subject to the Crown but not to a Parliament in which they had no representation. His idea of a British Empire became an eighteenth-century vision of the twentieth-century Commonwealth. And by 1770 he placed all his hopes in the Crown. "Let us therefore hold fast [our] Loyalty to our King (who has the best Disposition toward us, and has a Family-Interest in our Prosperity), as that steady Loyalty is the most probable Means of securing us from the Arbitrary Power of a corrupt Parliament. . . ."

The second shattering event of 1770 was the first ominous action of the determinedly inept North ministry: the repeal of all the Townshend duties except that on tea, the exception being made not as a revenue-raising measure but as a gratuitous reminder of Parlia-

ment's right to impose taxes on the colonies. Repercussions to this blunder were not to be felt fully for some time, although it had an immediately deteriorative effect on Franklin's strategy of maintaining colonial nonimportation agreements to advance the economic independence of the colonies, thus depriving him of his most prized weapon to combat British political arrogance.

Both his public papers and private correspondence in these volumes reveal anew how thoroughly and how longingly Franklin was an Englishman—not by heritage alone (his father was twenty-six when he left Northamptonshire for Boston), but also by temperament and by as deep feelings as his bemused and detached nature permitted. Although he had moments of acute political uneasiness in Whitehall, he was wholly at home in England, fond of Grafton and Lord Chatham, and appreciative of the honors the English learned community had heaped upon him; and, as late as 1768, he had looked with favor upon his rumored appointment to the service of the Crown as under-secretary of state for America. Yet events and his own resiliency were to make him also thoroughly, inherently, and devotedly American—even to the estrangement of his only surviving son, who remained to the end loyal to the Crown. A Daedalian man and a totally absorbing one, Franklin has a place in history that is well served by this masterly edition of his papers.

ARTHUR BERNON TOURTELLOT  
New York City

JACKSON TURNER MAIN. *The Sovereign States, 1775-1783*. New York: New Viewpoints. 1973. Pp. vii, 502. Cloth \$10.00, paper \$4.50.

The bicentennial of the American Revolution is two years away, but the floodgates of historical writing already have been thrown open and the deluge is beginning to roar down upon us. Main's book is part of the flood, the bulk of which seems intended for that elusive creature known as the "general reader." This book is also aimed at the nonspecialist. It is therefore devoid of footnotes, and the index is skimpy. The time frame is the war period, and the topics covered are of a standard order—for example, the economic, political, and social im-

pact of the war; the formation of constitutions and new governments; and the loyalist problem. There is one striking (and welcome) omission—namely a word on military history. The basic organization, exclusive of the introductory chapters, adheres to the familiar macrocosm-microcosm format. Main begins with broad strokes and then moves to a state-by-state summary of what transpired. Complementing the text is a lengthy bibliographical essay, arranged under chapter headings. This section contains many recent significant works and, because of Main's incisive annotations, holds as much value for the specialist as it does for the general reader.

Because the book is part of a series that falls under the rubric of "new viewpoints," it is incumbent upon a reviewer to address himself to this point. Main's principal "new viewpoints" are not exactly novel, but they are away from the "mainstream" of current historiography of the Revolutionary era. Thus he places an added value on the importance of the British governors and executive departments and correspondingly downplays the power and significance of colonial assemblies, a favorite theme of modern scholars—although, in a later section, he backtracks a bit, thereby weakening his position. On the well-known "conflict" or "consensus" issue (that is, should one emphasize that which divided or united the rebels. Main steers a middle course between these two divergent interpretations and acknowledges the validity of both, with some minor modifications. If his position is not novel, it at least has the virtue of conforming to common sense. He offers some penetrating and arresting theories on the economic and political development of the embryonic states, and these sections constitute creative scholarship of a high order. He also takes exception to the Franklin Jameson school of interpretation on the social consequences of the Revolution. The factor of incessant social mobility, Main asserts, mitigated the effects of the demise of the loyalist leaders and other social upheavals.

Viewed as a study for the average American, this book is a solid success; and specialists will profit from Main's chapters on economic and political developments. A consummate scholar who has researched this period in depth and utilized many primary resources, Main has pro-

duced an authoritative account that supersedes the older standard work of the period, Allan Nevins's *The American States During and After the American Revolution, 1775-1789* (1924). Main has set a high standard for the rash of surveys destined to appear before Washington once again says farewell to his officers in 1983 and the bicentennial comes to an end.

LOUIS LEONARD TUCKER  
State Historian of New York

HOWARD C. RICE, JR., and ANNE S. K. BROWN, translated and edited by. *The American Campaigns of Rochambeau's Army, 1780, 1781, 1782, 1783*. Volume 1, *The Journals of Clermont-Crèvecoeur, Verger, and Berthier*; volume 2, *The Itineraries; Maps and Views*. Princeton: Princeton University Press; Providence: Brown University Press. 1972. Pp. xxviii, 351; v, 204, 177 plates, 345-62. \$100.00 the set.

GATESBY WILLIS STEWART. *The Life of Brigadier General William Woodford of the American Revolution*. In two volumes. Richmond, Va.: Whittet and Shepperson. 1973. Pp. xiii, 743; 745-1446. \$30.00 the set.

Both of these large, handsomely bound, two-volume works deal with the American Revolution, and both are chiefly valuable for their documents, not their narrative or interpretation. There the similarity ends. The Rice and Brown work is meticulously edited and carefully and logically arranged, whereas the Stewart biography of William Woodford seems more like a series of notes put together with scissors and paste.

The first of the volumes in *The American Campaigns of Rochambeau's Army* consists of the journals of three French junior officers who came to Newport with Rochambeau in 1780 and participated in the Yorktown campaign and later in the operations in the Caribbean. The second volume contains the itineraries prepared by the engineers of Rochambeau's army on the march from Newport to New York to Yorktown and maps and views that include routes and encampments on both the march to Yorktown and the return trip to New York and Connecticut.

The three journalists are the Comte de Clermont-Crèvecoeur, a first lieutenant in the Auxonne Regiment; Jean-Baptiste Antoine de Verger, a sublieutenant in the Royal Deux-

Ponts regiment of infantry; and Louis Alexandre Berthier, a captain attached to the Soissonnais regiment as assistant quartermaster general. Some portions of these journals have been published before, but this is the first complete edition, in either French or English. Indeed it was only by some clever detective work that the author of the first journal was identified by Rice and Brown as Clermont-Crèvecoeur; parts of the journal had previously been published as the work of one De Robernier, a subaltern in the Soissonnais regiment. In addition to the three journals printed here, an appendix to the first volume contains a most helpful checklist of all other extant writings of French soldiers who served in the American Revolution, either with Rochambeau's army or with Washington's.

Of the three journalists Berthier was the only one to achieve any great measure of fame in the world as one of Napoleon's leading generals. Clermont-Crèvecoeur and Verger, though both pursued military careers on their return to Europe, were numbered among the foes of the French Revolution and never escaped the relative obscurity of emigré colonels and generals. Yet it is the journal of Clermont-Crèvecoeur that holds the greatest interest for the general reader with its comments on American flora and fauna and on the strange customs of the natives. One is surprised, for instance, to learn from him that French officers after the surrender at Yorktown got along "famously" with their British counterparts but seldom associated with the Americans whose customs were so different from their own, or that Virginia women of twenty would pass for thirty-five in France. Berthier, on the other hand, writes more prosaically, if quite accurately and precisely, of military matters, as does Verger, though the latter repeats all sorts of outrageous stories about British atrocities and accepts many myths about America and Americans at face value. All three note, with almost similar language, that peculiar American animal, the opossum, that carried its young in a pouch.

All in all this work provides the student of the American Revolution with an admirable source book on the French army in America. The arrangement is excellent, the translation well rendered, the explanatory notes informative, and the reproduction of the French maps

extraordinarily good as one follows Rochambeau's army through some fifty encampments. The introductory biographies give brief but adequate accounts of the men who wrote the journals. There is little a reviewer can quarrel with in these two volumes except the price, which will make it difficult for even libraries to acquire the set.

Mrs. Stewart's biography of William Woodford, one of the Virginia brigadiers of the Revolution, is not nearly so successful. She has, it is true, in documents and text, illumined the life of Woodford and his contemporaries, social and political life in eighteenth-century Virginia, and military affairs during the Revolution; and for this reason the two-volume biography will be of some interest to historians. But the whole work is something less than adequate as historical narrative, consisting of a formidable number of printed documents or excerpts from documents with inadequate connective tissue between them. The work lacks logical organization and the style is rambling. Genealogists, antiquarians, and historians will find here, however, a compendium of information if they dig hard enough.

ROBERT W. COAKLEY  
Center of Military History  
Washington, D. C.

JAMES MCLACHLAN. *American Boarding Schools: A Historical Study*. New York: Charles Scribner's Sons. 1970. Pp. x, 381. \$10.00.

STANLEY K. SCHULTZ. *The Culture Factory: Boston Public Schools, 1789-1860*. (The Urban Life in America Series.) New York: Oxford University Press. 1973. Pp. xvi, 394. \$11.50.

The contemporary financial plight of many U. S. elite secondary schools and colleges suggests a need for thorough inquiry into their reason for being. This coupled with the conventionally strong urge to admiration for the public school in its classic relationship to the American Dream emphasizes the desirability of a prompt review of each sector. James McLachlan's *American Boarding Schools* fills rather adequately this first requirement and Stanley K. Schultz's *The Culture Factory*—despite its clumsy title—the second. Both authors are to be complimented for their aspiration to place profiles of the national record within the main-

stream of American history—a performance most frequently bypassed by the upper echelons of the country's historiographers to the considerable eclipse of American educational achievement.

Following sociologist Digby Baltzell's lead, McLachlan limits his field to fourteen down-East college preparatory schools (thus omitting two Virginia institutes not especially pertinent to the study). These include Phillips Andover, Phillips Exeter, Hill, St. Paul's, St. Mark's, Lawrenceville, Groton, Taft, Hotchkiss, Choate, St. George's, Middlesex, Deerfield, and Kent. The book is arranged around three sections: Federalists, Victorians, and Progressives, with 20 pages of engravings and photographs, and 65 pages related to notes and sources.

Beginning with a brief summary of the intellectual climate, he attempts to relate—through vignettes of leading prep school headmasters—educational philosophy in the American East with the newer pedagogical thought then stirring in Western Europe of the early 1800s. Pursuing this ideological pattern into a retrenchment (Victorian) period, McLachlan judges the emergent philosophy in the schools he has selected as "progressive," a conclusion justified only by referring pedagogical developments to the political climate—hardly an expression of thought about school practice, which remained (and this its saving grace) largely classical and idealistic.

He shows curriculum models, generally replicated by these private Etons and Harrows of America (although strongly disputing such association), and reiterates headmasterly ideology bent on setting the day's work toward the goal of "preparation for life"—here interpreted as a classical mode paralleling the old Fürstenschulen of Prussia, yet with a strong emphasis on sports and other group activities. Quoting Domhoff it appears that the prime value of the select school is the inculcation of "upper-class value, upper-class manners, and most of all upper-class speech."

Nonetheless McLachlan's focus remains myopic; with other than this select group of chiefly New England boarding schools the target, further academy treatment, especially that of female education, is left in obscurity. Little of the color of boarding school's daily life is conveyed, and indeed one might gain more of this



from a quick reading of Mr. Chips or John Knowles's *Devon*. Neither does McLachlan bring his account to the current innovation in coeducation (recently at Exeter and St. Paul's) nor greatly stress the recent urge to admit more black students and members of other minorities—Exeter, for example, now expending half a million annually in diversified scholarships. Sizer's *Age of the Academies* appears to have been little used or the popular historians of American education—Butts, Good, Meyer, Mulhern, or Commager—consulted.

Schultz's work, subtitled *Boston Public Schools, 1789-1860*, presents in summary the development or maturation of this city's elementary school system from its colonial legacy to the inauguration of the urban superintendency and the enactment of the compulsory attendance laws, first in the U. S. Five rubrics organize *The Culture Factory*: Origins of Urban Schools; Society and Schools; The Machinery of Public Education; Segregation and Integration: Blacks in Public Schools; and Poverty, Immigration and Public Morality. Schultz's documentation is monumental with 70 pages of notes and references; there are 6 maps, 16 tables, and 12 reproductions of old prints. The author's theme, education for social change, is magnified by his choice of Boston for detailed inquiry; small (in the period subsumed by his title), virtually landlocked—an almost perfect laboratory. Yet it was this very insulation together with Boston's historic list of primacies in public education that now served to obstruct nineteenth-century European educational innovation and the fruits of pedagogical experimentation in neighboring states and in the new American West. Thus, little is said of the great educational reforms of Rousseau, Pestalozzi, and Froebel—the Prussian system as visited by Massachusetts's indestructible gadfly, Horace Mann, seemingly offering the greatest challenge to Bostonian pedagogical programing.

Schultz's topical arrangement makes for easier reading, but this reviewer thinks that a more thorough integration of the various currents hailed by Schultz as tributary to the Boston response to provision of a more perfect education system—since he has opted for a historical motif—could have importantly improved his account. But one could also easily infer that such a method results in the production of

Schultz's best contribution: first, that of the black education problem existing in the early half of the 1800s which includes a good exposition of the background of *Roberts v. City of Boston*, the fountainhead of "separate but equal" treatment of minorities and forerunner of the more famous *Plessy v. Ferguson*; second, that of the urban crisis occasioned by the influx within the Jacksonian period of "those hordes yearning to be free," especially by the vast Irish migration. For, regardless of sometime rioting, Boston never became a New World Belfast (chapters 9 and 10).

Since Schultz writes of education and society one wonders why he did not choose to enlarge his scope to encompass something of the vast social movement and innovative religions whose effect on the body politic is so well cataloged by Constance Rourke and Alice Felt Tyler. Surely these manifestations operated in the direction of education as well as in the socioeconomic sphere and are certainly as pertinent as the Know-Nothingism described in chapter 8. Furthermore one might, I think, properly quarrel with his title, the child of a presumed intuition rather than an objective statement of Boston's history. Nor does it appear that Schultz has mined the Bay newspapers as thoroughly as, say, Vera M. Butler in her University of Pennsylvania monograph.

Finally, while school buildings are described at great length, the same industry is not exhibited in a survey of the changing curricula over the seventy years considered, seemingly a highly pertinent fraction of the total maturation process. But before Schultz is accused of an antipathy toward public education or, at least, anti-educational administration, it must be admitted that of the two volumes described his is the much more scholarly endeavor, although James McLachlan's *American Boarding Schools* may be considered the better-rounded account.

KENNETH V. LOTTICH  
Scottsdale, Arizona

CHRISTOPHER MCKEE. *Edward Preble: A Naval Biography, 1761-1807*. Annapolis: Naval Institute Press. 1972. Pp. x, 394. \$16.00.

Little more remains in the American memory of the war with Tripoli in the early years of the nineteenth century than the second half of

the line from the Marine Corps hymn: "From the halls of Montezuma to the shores of Tripoli." The fact is that only a handful of Marines were involved. It was for the most part the Navy's war, and the leading figure in the conflict was Captain Edward Preble, commander of the Mediterranean Squadron in 1803 and 1804.

The salient facts about Captain Preble's life can be found in the large collection of family papers in the Library of Congress, Yale, and elsewhere, and Mr. McKee in his most recent biography of the captain has used this, as well as other material, to illuminate Preble's career. Edward Preble, third son of General Jedidah Preble, was born at Falmouth (now Portland), Maine, in August 1761. At sixteen he ran away to sea, serving for several years on a privateer. In 1780 he joined the Massachusetts navy, fought in several engagements, and was briefly a prisoner of war. With the return of peace he entered the merchant marine and in 1798, at the age of 37, was appointed lieutenant in the newly established United States Navy.

An important opportunity for him came in 1803, when Preble, as one of the most junior captains in the Navy, was given command of the Mediterranean Squadron during the war with Tripoli. Blockading Tripoli as well as he could with his tiny squadron (six vessels, the largest of which, the frigate *Philadelphia*, was captured and later burned), Preble assembled half a dozen gunboats and several other vessels. In August 1804 the small American force attacked the strongly defended North African port. Despite repeated assaults and heavy casualties, Preble was unable to take the town and soon after was relieved by Commodore Samuel Barron. Bitterly disappointed, he returned to the United States where Jefferson put him to work building gunboats for the Navy. In declining health, he finally returned home and in August 1807 passed away.

Preble's career in the young Navy, though brief, was important in establishing professional standards and in shaping a tradition that has served the Navy well. A hot-tempered, rough-tongued disciplinarian, he insisted on and received from his men the best that was in them. As commodore of the Mediterranean Squadron, he fashioned a professional fighting team. Many of his subordinates, later to become famous,

were then young men under thirty who came to be known as "Preble's boys," a title they carried proudly into the War of 1812 and beyond.

McKee has done full justice to Preble and to the history of the early years of the United States Navy. In doing so, he has added to our understanding of the role of the Massachusetts navy in the Revolution, the development of a professional corps of naval officers, and Jefferson's naval policy and strategy in the war with Tripoli.

LOUIS MORTON  
Dartmouth College

DONALD O. DEWEY. *Marshall versus Jefferson: The Political Background of Marbury v. Madison*. (Borzo Series in United States Constitutional History.) New York: Alfred A. Knopf. 1970. Pp. x, 195. \$2.95.

MAURICE G. BAXTER. *The Steamboat Monopoly: Gibbons v. Ogden, 1824*. (Borzo Series in United States Constitutional History.) New York: Alfred A. Knopf. 1972. Pp. vii, 146. \$2.95.

RICHARD C. CORTNER. *The Jones & Laughlin Case*. (Borzo Series in United States Constitutional History.) New York: Alfred A. Knopf. 1970. Pp. ix, 191. \$2.95.

During the past few years the Alfred A. Knopf Company has sought to profit from college courses in constitutional history through the publication of a series of books focusing on pivotal Supreme Court decisions. At present three volumes have appeared dealing with the cases of *Marbury v. Madison*, *Gibbons v. Ogden*, and *National Labor Relations Board v. Jones & Laughlin Steel Corporation*. These books do not purport to offer fresh findings or revolutionary interpretations. Rather, they seek chiefly to synthesize past studies and present the student with a complete and accurate account of certain landmark cases.

Each of the works follows a standard format, beginning with a summary of the specific situation or controversy from which the case arose. Thus Donald O. Dewey's volume on *Marbury v. Madison* describes the midnight appointments of John Adams and the failure of Secretary of State John Marshall to deliver the appointees' commissions. Likewise Maurice Baxter's volume on *Gibbons v. Ogden* offers a de-

tailed narrative of the Livingston-Fulton steamboat monopoly, while Richard Cortner briefly portrays the origins of the National Labor Relations Board and the early antagonism felt toward that body. Each author then describes the judicial proceedings that arose from these controversies and summarizes the arguments of counsel. There next follows a detailed summary and analysis of the Supreme Court's decision. And lastly each author seeks to identify the impact or effect of the case on future constitutional development. In other words, they each present an accurate and detailed description of their specific case from beginning to end, from cause to consequence.

This narrow preoccupation with individual case histories, however, seriously limits the value of the Knopf series as an instrument for teaching constitutional history. The books in this series are not histories of legal or constitutional development but biopsies of "great" cases. Donald Dewey's work does not attempt to trace the origins and development of the doctrine of judicial review but rather the origins and development of the single case *Marbury v. Madison*. And in pursuing this purpose, he presents a detailed political history of the conflict between Jefferson and Marshall during the early nineteenth century. The history of this political controversy is the center of his attention and not the broader subject of judicial review. Moreover, this restricted focus often leads him to dwell on trivia while neglecting those vital factors that determined the evolution of constitutional doctrine. Thus while Dewey devotes four pages to the lives of those nonentities who acted as plaintiffs in the *Marbury* case, he dedicates not a single page to the political philosophy of balanced government that underlay the notion of judicial review.

Baxter and Cortner offer a somewhat fuller picture of constitutional development, devoting greater attention to the evolution of legal doctrine and less to the political machinations surrounding their specific cases. But again their volumes might have been more useful had each adopted a broader perspective and not limited his focus to a single landmark decision. For example, Baxter feels compelled to dwell heavily on the history of the steamboat because it was fundamental to the particular case he is examining. Yet he generally ignores other notable

changes in transportation and economic structure that might have equally influenced the Marshall Court's views on the federal commerce power. Likewise Cortner's volume would have been more helpful to the student if it had examined the constitutional development of the 1930s as a unit rather than singling out one case as the focus of attention. In general, then, the Knopf series suffers from a misguided preoccupation with the particulars of landmark decisions and as a consequence fails to describe adequately the broader factors influencing and determining the course of America's constitutional history.

JON TEAFORD

Iowa State University

RICHARD DRINNON. *White Savage: The Case of John Dunn Hunter*. New York: Schocken Books. 1972. Pp. xix, 282. \$12.50.

J. NORMAN HEARD. *White into Red: A Study of the Assimilation of White Persons Captured by Indians*. Metuchen, N.J.: Scarecrow Press. 1973. Pp. ix, 180. \$6.00.

CARL F. KLINCK and JAMES J. TALMAN, editors. *The Journal of Major John Norton, 1816*. (Publications of the Champlain Society, number 46.) Toronto: the Society. 1970. Pp. cxxiv, 391. By subscription.

The narratives of *White Savage*—marginal men and women who frequently preferred life in Indian society to being returned to a society and culture now alien to them—early attracted a wide readership here and abroad. For fifty years scholars have paid serious heed to captivities as documents of acculturation in cases of individuals caught between two cultures. During the Boasian emancipation from nineteenth-century racism, the late John R. Swanton first saw this genre as evidence opposing the doctrine of inherited psychological differences between races, and his brief paper in the *Journal of the Washington Academy of Sciences* (1926) is cited in two of these studies. Swanton's lead was followed by Erwin Ackerknecht, historian of medical anthropology, who interpreted the captivities as case histories in the enculturation of individuals (*Journal of the History of Medicine*, 15 [1944]: 15–36). Later historians, in the present instance, Richard Drinnon, and librarians, like J. Norman Heard, have found critical study of these documents

facilitated by the bibliographies of R. W. G. Vail (*The Voice of the Old Frontier* [1949]), the publications of the Newberry Library, and the work of Marius Barbeau for the American Philosophical Society (*Proceedings*, 94 [1950]: 522-48). The recent rage over the Indian as symbol for a literary image takes off from Roy Harvey Pearce's *The Savages of America* (1953). The three works under review owe something to each of these intellectual movements.

Drinnon's treatment of the case of John Hunter is most difficult to assess. There is only Hunter's memoir to go on, and it exhibits as remarkable an ability of its author to acquire literacy in a hurry as he purports to have demonstrated in learning the languages and assuming the cultural roles of the Kickapoo, Kansa, and Osage Indians on the southern plains. He had the advantage of being taken captive young enough so that he may well have learned the language (or languages), but he must have been something of a genius at that—as some observers seem to attest—to have later mastered literary forms so rapidly to have been able to publish his memoir by 1823—all within the span of two decades. In England he was introduced to royalty, held in high esteem by the gentry, vouched for in the Royal Society, and praised in literary circles. Like other prophets at home he was censured as an "impudent imposter" by Lewis Cass and Henry Rowe Schoolcraft, men who had a stake in the lands of the West and in Indian administration, which Hunter criticized; and his reputation for knowing the native languages was demolished by Duponceau. Catlin and others were more kind to him. Drinnon treats this controversy with circumspection. He writes in an engaging manner, and he has brought to his argument for Hunter complimentary evidence of some weight. For example, it is possible that Hunter could have learned to communicate in Osage without mastering its morphology and syntax and would therefore fail Duponceau's examination. But unlike Catlin, who gives a far better explanation of Plains culture, which he illustrated so nobly, Hunter pales as an ethnographer. This fascinating book does not really satisfy the issue of the authenticity of Hunter's memoir.

The question of how and why certain white captives rejected white civilization in favor of

"savage society" is examined by Heard on the basis of culture, family background, national origins, differential treatment in the Eastern woodlands and Plains west of the Mississippi, the time required to become an Indian, and especially the critical age of the captives when taken. He also examines the struggles, successes and failures to readjust, and as a control, looks at Indian children in white civilization. The treatment is systematic, always clear, and the documentation is superb. Among his cases is John Hunter, captured first in 1801 by Algonquian-speaking Kickapoos, treated miserably and lost by them to the Kansa—close linguistic relatives of the Siouan-speaking Osage—where he was adopted into a family in the place of a son killed by the Pawnees. Hunter spent fifteen years in captivity before escaping, and Heard estimates that he was 80 per cent assimilated. But he makes no comment on the authenticity of his memoir. Captivity early in life, before the age of puberty, seems to have been essential for learning the language and was detrimental to rehabilitation in white society. From an ethnographic point of view James Smith, who was eighteen when taken and spent four years among the Caughnawaga Mohawks in Ohio, and whom Heard estimates was but 20 per cent assimilated, made the most penetrating observations of Iroquois hunting techniques.

Major John Norton, not a captive, seems to have been the son of a Cherokee father and a frontier mother who was a Scot. Parts of his career are as obscure as his birth, but he lived principally among the Six Nations on the Grand River in Upper Canada—particularly among the Mohawks—serving as interpreter to the Indian Department. As the protégé of Joseph Brant, he was installed in the chiefly title of Teyoninhokarawen, a Mohawk form which in all the dialects means "It keeps the door open" and not what Klinck says it means on bad scholarly advice—namely, the title of the last office on "The Roll Call of the Iroquois Chiefs" (Smithsonian Miscellaneous Collections [Washington, 1950]). Indeed it is a pity that the otherwise excellent biographical introduction is marred by errors of an ethnological and linguistic nature that any of several Iroquoianists could have caught. Talman's historical introduction places Norton in his period.

The journal itself, written in England and long the property of the dukes of Northumberland, is especially valuable in reporting the Indian side of events on the Niagara frontier. It confirms the tradition of the settlement of the Six Nations on the Grand River after 1784; it contains a priceless sketch of the Deganawidah epic of the founding of their confederacy, which is paralleled by a contemporary version of Joseph Brant. Apparently Norton was Brant's literary heir and incorporated into his journal the traditional history that Brant had promised Samuel Kirkland he would write. This material is valuable up to a point, for it contains an Iroquois view of events as far back as the seventeenth century, but it also paraphrases much that was taken from books. Norton was a better reporter than John Hunter with whom he shared preferential treatment in England and Scotland.

WILLIAM N. FENTON  
State University of New York,  
Albany

R. T. HUNTINGTON. *Hall's Breechloaders: John H. Hall's Invention and Development of a Breech-loading Rifle with Precision-Made Interchangeable Parts, and Its Introduction into the United States Service*. Edited by NANCY BAGBY. York, Pa.: George Shumway. 1972. Pp. xiv, 369. Cloth \$15.00, paper \$12.00.

More than anything else this is a book for the specialized arms collector. It is strongly based on the author's own collection and his wide acquaintance with other collections. Additionally, extensive use has been made of original reports and personal correspondence, which are preserved in the National Archives. The texts of some of the most important and lengthy of these are reprinted in the appendixes. Each chapter is followed by extensive footnotes.

John Hall is a very important figure in the evolution of the American system of interchangeable manufacture. He is substantially more important than Eli Whitney, who popularly receives all the credit. The book is not strong, however, in placing Hall's achievements in sharp focus vis-à-vis the accomplishments of Simeon North, Asa Waters, and a host of others, both here and abroad, who contributed increments, both before and after Hall, toward the eventual goal of close-limit interchangeability. The sig-

nificance of the Hall rifle as a vehicle for technological advance ends before 1830 when Hall's system reached its fullest development. The Simeon North armory in Middletown, Connecticut, later incorporated an extension of Hall's system, but other than this, Hall's work did not contribute to further advances.

For the collector of military arms and accouterments the book will probably always stand unchallenged and unsurpassed for the wealth of minute detail that has been painstakingly discovered, organized, and illustrated. Even the Confederate modifications and adaptations of obsolete Hall rifle parts, which were removed from the captured Harpers Ferry armory, are covered, as are the earlier civilian and experimental models.

This book will prove invaluable to those interested in identifying or collecting all the various models of Hall rifles and their accessories. For students of the evolution of manufacturing techniques and of interchangeable manufacture in particular, the book is a must, by far the most soundly based and complete point of departure that has appeared.

EDWIN A. BATTISON  
Smithsonian Institution

MARK T. CARLETON. *Politics and Punishment: The History of the Louisiana State Penal System*. Baton Rouge: Louisiana State University Press. 1971. Pp. xii, 215. \$8.50.

GERALD N. GROB. *Mental Institutions in America: Social Policy to 1875*. New York: Free Press. 1973. Pp. xiii, 458. \$10.95.

Prisons and mental institutions, long an outward manifestation of an inward disgrace, excited the sympathy of humanitarians and provided an opportunity for malefactors to prey upon unfortunates who either flouted the law or, by no fault of their own, were unable to adjust to normal behavior patterns. Until recently, no attempt has been made to distinguish between the criminally sick and the mentally ill, so that, in far too many instances, each group has been "treated" with punishment by confinement in jails little different from asylums and asylums scarcely distinguishable from penitentiaries.

Competent studies of penal institutions and

mental hospitals, during any period of the nation's history, are scarce and many are less than objective. Administrators have cloaked their philosophies, their methods, and their discipline in secrecy. The hammer of research has failed to crack openings in institutional walls wide enough to more than peek through. Yet each attempt, no matter how small, widens the general vista. Both Carleton, in his study of the Louisiana state penal system, and Grob, in his discussion of the social policy of American mental institutions to 1875, provide material—although sometimes incomplete and marred by questionable generalization—that is sturdy enough to warrant publication and to lean upon.

Carleton's volume, which traces the changes, if not the progress, of Louisiana's varying methods of handling convicts from 1835 to 1968, seems to rest primarily upon the thesis or assumption that thoroughly materialistic motives were responsible for thwarting what he calls "enlightened" correctional policies. Although Carleton does not use the term "enlightened" in his text, this surely is what he means (pp. 7-8). What is even more difficult to believe, even though it might be true, is the connecting of racism with prison labor and reform. He lays heavy emphasis upon "this species of slave labor" during the decade from 1880 to 1890, maintaining that the desire to make money from convict labor was "so compelling and popular throughout Louisiana that the advocates of reform themselves were infected by it" (p. 42). This may answer one question, but it gives rise to another: Why, during the 1940s and 1950s, did the objective of the prison profit system remain unchanged? Why were the findings of professional investigations unchanged? Carleton, to his credit, lays bare the brutality, the political administrations ill-disposed toward penal reform, and the dehumanization of inmates. With equal objectivity he recounts attempts at reform. His concluding chapter, tinged with sadness and tintured with at least semirationalization, admits that prior to 1952 the state's penal system was essentially a business enterprise, administered "either by politicians or by lessees" (p. 192). He writes also that "as for public support of penal systems, the rest of the nation does not seem to be any more enthusiastic or reliable than Louisiana" (p. 198). It

would be most difficult to document that declaration.

Although the pattern of handling or rehabilitating convicts and lunatics was much the same for years, more change, if not progress, has been made in the care and treatment of the latter than the former, at least in many areas of the nation. Grob's sweeping survey, which in a sense could be regarded as a companion volume to Albert Deutsch's *The Mentally Ill in America*, not only is stimulating and provocative but also charts with an understanding objectivity and clarity the long, trying search by those who realized they had a problem to solve, but did not quite realize what their problem was. The author puts it in capsular form when he writes that to define the nature and causes of mental illness was only a beginning, not an end. "Psychiatrists then faced the question that had the greatest policy implication: what could be done to alleviate or cure mental disease?" (p. 165).

The dilemmas raised by that query, by the theory and practice of mental hospitals, and by the inability to distinguish between curable and incurable patients offer new and additional insights. Certainly the chapter discussing class, ethnicity, and race in mental hospitals is among the book's better sections. As in prisons, the author writes, some administrators "manifested unconscious hostility toward patients coming from backgrounds different from their own" (p. 222). Some promoted discriminatory practices, and others shared the racial and class prejudices of the larger society.

Not until after the 1850s did workers in the field of mental health realize that the mere founding of hospitals contributed little to problems posed by mental illness. From then until 1875 concerned individuals concentrated more and more upon the development of public policies, by which is meant the inclusion of mental hospitals as an integral part of political units such as states and counties. For this, legislation was necessary. Grob not only traces the difficulties and triumphs inherent in securing such legislation but also examines the structure and functions of institutions born of statutes throughout the country. The results, he points out, verged upon the tragic, for although participants in the mental health movement generally were "well intentioned" their "actual behavior

gave rise to less than desirable results" (p. 342). Like the prisons of which Carleton writes, "mental hospitals were not fundamentally dissimilar from most human institutions, the achievements of which usually fall far short of the hopes and aspirations of the individuals who founded and led them" (p. 342).

PHILIP D. JORDAN  
Burlington, Iowa

DAVID M. PLETCHER. *The Diplomacy of Annexation: Texas, Oregon, and the Mexican War*. [Columbia:] University of Missouri Press. 1973. Pp. xiii, 656. \$20.00.

There has long been need for a fresh examination of the diplomacy of American expansion in the 1840s. A diplomatic history of the Tyler and Polk administrations has not been published since 1907. Scholars interested in the international aspects of the annexation of Texas and the Mexican War have had to rely on the outmoded and biased studies of Justin Smith, and analyses of the Oregon question have been scattered in scores of articles. David M. Pletcher, through intensive multiarchival research and the examination of nearly four hundred books and articles, many Mexican, has now provided a thorough reconsideration of the diplomatic aspects of these issues.

Pletcher's focus is on the origins and diplomacy of the Mexican War; however, he treats the Texas issue as a diplomatic problem in itself, as well as a cause of the war, and he examines the Oregon question as a collateral but separate consideration. On the diplomacy concerning the Texas and Oregon issues especially, he stresses the influences of economic and foreign political considerations, and he devotes considerable attention to the role of American politics throughout the book. Public opinion serves mainly as a background.

Pletcher's interpretation of the diplomacy of annexation is broad and provocative. Along with many other theses and revisions, he argues that American expansion generally, but particularly the annexation of Texas, created an international problem of major proportions; that America's diplomatic successes in the Oregon and Mexican War settlements occurred despite Polk's blundering; and that war with Mexico was neither necessary nor the best available means by which Polk could have satisfied

America's lingering continental aspirations. Much like Polk's contemporary critics, Pletcher is harshly critical of the president. Although Polk had limited goals in Oregon and wanted to acquire California he had no developed strategy for accomplishing these ends. His policies were expedient, politically-oriented, and lacking in direction. His tactics of bluff, bluster, and threat needlessly prolonged the Oregon issue and created the conditions that led to the war with Mexico. Skillful diplomacy and patience, Pletcher argues, would have led to the same American successes and would have served the United States better.

Pletcher's evaluation of Polk's diplomacy is well argued and supported, and his full, sensitive, and balanced discussion of Mexico's internal and foreign problems corrects the distortions of past scholarship. Unfortunately, although his treatment of the American side is superb, Pletcher has not considered as deeply the economic or ideological sources of European aims, policies, or purposes. His examination of the European role tends to diminish as he concentrates on the tortuous Mexican-American wartime diplomacy, thus weakening an important part of his thesis.

On the whole this is a fine book. Pletcher has challenged a number of recent interpretations (he denies that conspiracy played a part in Polk's program), and he has established an important new perspective to the historiography of American expansion in the 1840s. Whether or not one agrees with his interpretations and speculations Pletcher has provided an immensely valuable synthesis and has added considerable new material. This book supersedes all existing diplomatic studies of the problem and will long remain the standard text.

KINLEY J. BRAUER  
University of Minnesota,  
Minneapolis

LEONARD GILHOOLEY. *Contradiction and Dilemma: Orestes Brownson and the American Idea*. New York: Fordham University Press. 1972. Pp. xv, 231. \$10.00.

One of the reasons often advanced for the study of secondary figures in history is the insight they offer into the careers of more important persons and the movements of their time. Orestes Brownson's life should be of great in-

terest for this reason. He knew many of his more prominent contemporaries, particularly the Transcendentalists, and was part of the movement himself for a while. American scholarship could use a new and full biography of Brownson.

Regrettably, Professor Gilhooley has chosen to give us only a limited study of Brownson's thought. The format of the book is a summary of the articles Brownson wrote between 1838 and 1859. The result is rather choppy, superficial, and repetitious. The author never gets below the surface of what Brownson says and never really traces the development of his thought because we do not see what causes him to think as he does.

What does come through is a picture of Brownson as a man of incredible vanity and conceit who always tried to show how much more he knew than everyone else. Theodore Parker, Ralph Waldo Emerson, Edgar Quinet, Jules Michelet, and George Bancroft, to mention only a few, were all demolished by his brilliance and learning, or so he thought. Posterity has decided otherwise. Parker and Emerson are in the mainstream of American thought. Michelet and Quinet still rank as great scholars. Bancroft's significance in American historiography is secure.

Evidently something was eating Brownson, and until we find what it was, we cannot really understand his thought processes. His conversion to Catholicism was the focal point of his life. For the rest of his career he was venomous toward his former associates among the Transcendentalists, in particular Parker. Brownson never missed a chance to denounce Protestantism as inimical to American democracy, while insisting that the nation could not survive without Catholicism. As Gilhooley unwittingly shows, however, Brownson did not believe in democracy or religious freedom, but grew steadily more bigoted as he aged. Regrettably the author has omitted Brownson's discussion of the Mortara case, which shows how little regard Brownson had for the rights of others.

Perhaps sometime someone will produce a significant book on Brownson, based on real research. It should be fascinating.

HAROLD SCHWARTZ

Kent State University

KATHRYN KISH SKLAR. *Catharine Beecher: A Study in American Domesticity*. New Haven: Yale University Press. 1973. Pp. xv, 356. \$12.50.

The accomplishments of the eldest child of Lyman Beecher have for too long been overshadowed by the fame of her younger siblings, Henry Ward Beecher and Harriet Beecher Stowe. After surviving the perils of being Lyman's daughter and the trauma of the death of a fiancé selected by her father, Catharine Beecher (1800-78) became an expert in domestic economy, an advocate of moral education for children, the founder of numerous seminaries to train young women to become teachers, and the author of religious and philosophical tracts. Her most important work, *Treatise on Domestic Economy* (1843), "simplified and made understandable the mysterious arts of household maintenance, child rearing, gardening, cooking, cleaning, doctoring, and the dozen other responsibilities middle class women assumed" in the nineteenth century (p. 152). It also made her name a household word in the 1840s. Catharine Beecher herself never had a home or children, disdained teaching, and consistently believed that her personal life was one of self-sacrifice. Her moments of greatest pleasure seem to have occurred when she visited water-therapy establishments for "the cure," which provided women a socially acceptable opportunity to indulge in sensuality and to discuss frankly their own bodies and their attitudes toward men.

The strength of Mrs. Sklar's biography lies in her analysis of the intellectual development of her subject. Catharine Beecher believed in common sense, the submission of the self to the general good, the possibility of women dominating social change by expertly managing the home front, and the moral superiority of the traditional submissive role of women. One of America's pioneering Victorians, she believed "the protection of knowledge" should govern relations between the sexes. The tenets of Calvinism remained central to her thought; she insisted, however, that God saved not only those who had been convicted of sin but all who wanted to be saved. Both she and Harriet attempted to translate "Calvinism into a social rather than a religious system" (p. 242).

Despite many subtle revelations of Catharine Beecher's humanity and occasional references



to her "deep psychological problems" (p. 169), the author has refrained from the kind of penetrating analysis that might have made more interesting reading about a woman whose life, except for her visits to the water cure, was usually unexciting. At the same time the author has avoided the pitfalls of historians who use too hastily a discipline in which they are not well trained. The facts are skillfully enough collected and presented to allow the reader to draw his own conclusions. *Catharine Beecher* is a welcome addition to the literature about the famous Beecher family and a notable contribution to our understanding of the social and intellectual development of the United States.

E. STANLY GODBOLD, JR.  
Valdosta State College

JON L. WAKELYN. *The Politics of a Literary Man: William Gilmore Simms*. (Contributions in American Studies, number 5.) Westport, Conn.: Greenwood Press. 1973. Pp. xiv, 306. \$10.00.

Professor Wakelyn's objective "is to illustrate Simms' social and political values as revealed in his fiction and to show the slow shift in the views of this young intellectual seeking maturity in the South." Interpreters of Simms have generally classified him as a potentially gifted writer neglected by his social superiors whose cause he served or as a man who dissipated his literary energies trying to prop up the Southern Muse. Wakelyn maintains that Simms became a man of influence in his state and the South and that politics—rather than diverting him from literature—enriched his work. For him, "literature and politics were always combined, never separated." Fiction and historical writing were vehicles for his political and social theorizing.

Although Simms began as a nationalist and an adherent of his state's Unionist faction, his social philosophy remained conservative. From the 1830s on, he defended slavery even while he attacked nullification or held forth on the dangers of Western expansionism. Sectional strife turned him into a strident secessionist as well as affectionate critic of the South. He scolded the planters' narrow agricultural bias and destructive agronomy, worried in fiction and nonfiction about divisive tensions within

his section, and worked constantly to undermine the South's old political allegiances in favor of a great disunion party.

Wakelyn painstakingly documents Simms's public career and his passionate defense of his state. The evidence he presents for Simms's political influence is less persuasive. Whose ideas did he change, whose "dormant minds" arouse? Why did his publishing ventures invariably fail? Wakelyn gives us a fresh view of the writer-politician, but his political analysis of Simms's fiction and occasional comments on Simms's ambivalent response to Southern "leadership" suggest a personality more complicated and contradictory than the well-adjusted patriot of his book.

DANIEL AARON  
Harvard University

NEAL C. GILLESPIE. *The Collapse of Orthodoxy: The Intellectual Ordeal of George Frederick Holmes*. Charlottesville: University Press of Virginia. 1972. Pp. x, 273. \$9.50.

George Frederick Holmes (1820-97) enjoyed considerable prominence during his lifetime as an author, social critic, and educator. Although scholars have written about him previously, this is the first full-length biography to appear. Based on Holmes's private papers and his extensive publications as well as on other pertinent materials, this book provides a complete and balanced account of the man and his achievement.

Choosing his perspective wisely, the author uses the intellectual ordeal of Holmes to illuminate the revolution in thought that was occurring in the nineteenth century. Professor Gillespie skillfully combines the story of a life and of a mind. An ambitious and condescending Englishman with one year at Durham University, Holmes arrived in the South in 1838, living there until his death without ever becoming a United States citizen. He married into a prominent Virginia family and aspired toward the law but quickly turned to writing and teaching. The choice brought years of privation and disappointment before he finally landed a permanent professorship at the University of Virginia, but it enabled him to grapple seriously with ideas.

Three interrelated issues dominated his

thought. The problem of faith and reason was basic. Despite a period of religious infidelity, the reaction against which is never adequately explained, Holmes struggled throughout his life to reconcile Christian faith with contemporary science and historical scholarship. Honest doubts triumphed during his last years. Closely related to his religious crisis was a search for the laws of human society, which led him to Aristotle, Bacon, and Comte, but not, significantly, to contemporary German thought. Although he remained critical of Comtean Positivism, Holmes helped introduce the philosophy to America. This book supersedes earlier accounts of Holmes and the origins of the science of society in the United States, though it stresses the historian at the expense of the sociologist in the man. Last was Holmes's defense of the civilization of the Old South and apology for slavery, to which the author imparts freshness by treating the subject within the total configuration of Holmes's thought. Taking a lead from Eugene Genovese, Gillespie views Holmes as a bourgeois defender of slavery.

*The Collapse of Orthodoxy*, which began as a doctoral dissertation, is in all important respects a credit to its author.

WINTON U. SOLBERG  
University of Illinois,  
Urbana-Champaign

NORMAN C. DELANEY. *John McIntosh Kell of the Raider Alabama*. University: University of Alabama Press. 1973. Pp. 270. \$8.50.

John McIntosh Kell, executive officer of the ill-fated Confederate raiders *Sumter* and *Alabama*, was an aristocratic, vain, handsome, and self-satisfied Georgian whose role in American naval history has been largely obscured by his shipboard associations with the legendary Raphael Semmes.

This compact biography brings Kell out of the shadow of the towering Semmes and establishes him in his own right as an officer of enormous technical skill and considerable tactical insight. A man of great personal bravery in combat, he was also professional and sensible enough to have advised his commander not to fight the superior *Kearsarge* on that sunny day in June 1864. Indeed, Delaney has given us the most realistic (and demythologized) account

ever written of the long and successful cruise of the *Alabama* and the brief, historic, and foolhardy fight off Cherbourg that ended Confederate naval pretensions. Semmes's later rationalization that he had somehow been tricked into fighting a camouflaged ironclad (or chainclad) was never particularly convincing. Nor is Delaney convinced.

Like most American career naval officers of the mid-nineteenth century Kell had little political sophistication and less social sensitivity. He really had no idea what the Mexican and Civil Wars were all about, although he saw combat in both. He accompanied the Perry expedition to Japan without in any way understanding its larger purpose or significance. His sudden conversion to the Southern cause in 1861 was an automatic reflex rather than a reasoned decision.

Coupled with Kell's sophomoric political opinions was a deep sense of personal honor and an abiding faith in the supposed prerogatives of gentlemen. This last was so exaggerated as to cause him to be court-martialed (for failure to obey a direct order he considered demeaning) and briefly dropped from the service in 1849. Not surprisingly, his attitude toward the polyglot crew that manned the unhappy *Alabama* bordered on the contemptuous: "A sailor is a sailor: He has few attachments, and as a general thing doesn't care what flag he is under."

Nor did he or Semmes have high regard for the brain power of those ersatz Confederates, those "liars, thieves, and drunkards," who were shot to bloody bits by the far more efficiently served guns of the *Kearsarge*. "My crew were never so happy as when they had plenty to do, and but little to think about. Indeed, as to thinking, I allowed them to do very little of that," Semmes later boasted. This view was shared by Kell and is one that has not changed much among American naval officers since the Civil War.

Delaney has relatively little specific Kell manuscript material with which to work. He has, however, thoroughly mined the primary and secondary sources near and around Kell and has skillfully pieced together a variety of related family letters, printed memoirs, and official records. Included among these are the postwar recollections of Kell himself and those who had fought with and against him. The

result is a wholly satisfying book that belongs on the shelf of every American naval historian and every Civil War buff.

ROBERT SEAGER II  
University of Baltimore

THOMAS LAWRENCE CONNELLY. *Autumn of Glory: The Army of Tennessee, 1862-1865*. Baton Rouge: Louisiana State University Press. 1971. Pp. ix, 558. \$15.00.

In *Autumn of Glory* Thomas L. Connelly completes his monumental two-volume study of the Civil War in the West. In well-written narrative he follows the Army of Tennessee from the renewed offensive of General Braxton Bragg, which began near Murfreesboro, Tennessee, in late 1862, to the lingering days of retreat and defeat in North Carolina in 1865. With meticulous research and careful interpretation, he traces this army's actions as it dramatically defends but loses mile after mile in battle after dreary battle the "heartland" of the South—thus sealing the Confederacy's doom.

Connelly milled much of this work as a labor of love from his summer home in the midst of the heartland, and his descriptions come from firsthand knowledge of the region. In his occasional lightning flashes of genius, he brings the war to life—in the mountains, beside the rivers, and in the towns and cities of Tennessee and Georgia. His scenes of involvement extend from the planning table, through the march, and into the blood and death of the battlefield. The intent reader cannot but remember the vivid pictures of the battle of Franklin, the campaign for Atlanta, or the siege at Chattanooga.

Though he pictures the battles in intimate detail, the author is at his best in analyzing the men in command. He excels at stripping the leaders of their glory, and through his pages generals lose much of the glamour that has grown up around them. His penetrating evaluations confirm my own belief that generals lost as many battles as they won, their victories achieved more by indecision and mistakes in the enemy command than by genius in their own. Connelly's generals also usually appear as insensitive, even callous, to some of the larger issues of war that brought them to command in the first place and to the men whose

lives they too freely squandered in their efforts to win victories and glory.

Connelly is critical of the actions of leaders on every level of command, including those in power in Richmond whose decision making often left the West to its own dwindling resources to protect a region as integral to East as to West. In Richmond it is Jefferson Davis who is the prime target for criticism, but Robert E. Lee also falls in the onslaught. Sometimes the Virginian appears as indecisive, as in the summer of 1864 in his unwillingness to make a firm commitment on John B. Hood's appointment to high command; as uncooperative, as in the summer campaign of 1863 when he refused to consider a larger concerted defense to relieve the siege of Vicksburg; or as possessing narrow vision, as both he and the Confederate president failed to comprehend the realities and importance of the war beyond the Virginia theater.

In the Army of Tennessee Bragg and Joseph E. Johnston drew heavy criticism, but the subordinate generals, including William J. Hardee, John Hood, and Leonidas K. Polk, also feel the author's wrath. In analyzing Bragg's command, Connelly weaves through the intricate patterns of intrigue and confusion in the bizarre events surrounding that general's incompetency before William Rosecrans, especially at Chickamauga, and before Grant at Chattanooga. Bragg, an "irritable and impatient" man, possessed of "an amazing sense of poor timing," presents his worst side in reacting to the round robin from his generals that threatened his removal from command. Johnston, always seeking greater personal recognition, fares little better in the author's judgment: for his unwillingness to accept responsibility at Atlanta, in the struggle between Bragg and his generals, and in other assignments in the West and for his personal dislike of the Confederate president, which affected his decision making. Hood, though courageous and loyal, is pictured as a near incompetent in his leadership at the fiascoes at Atlanta, Franklin, and Nashville.

Among the "lieutenants," Thomas C. Hindman shows "a remarkable disinterest in taking the offensive"; Breckinridge is drunk on the field of battle; and Polk stalls when ordered to make a direct attack on the Federal position at Rock Spring Church.

Among the war's leaders, human weaknesses too often prevailed, and through their carping correspondence, the memoirs of those with whom they fought, and the official records and reports, Connelly lets these men seal their own fates. Though he recognizes the precariousness of the Confederacy in its bid for power before the superior strength of the federal government, Connelly sees internal discord among these commanders, the intricacy of Confederate command decisions, and the absence of decisive decision making by these commanders as important as the limited resources and manpower and dwindling supplies in contributing to defeat.

*Autumn of Glory* is a study of the Civil War that subjects men and events to close scrutiny and then pronounces judgment upon them. This is a rare quality in writing, especially when backed by intense research, a sharpness of interpretation, an effective writing style, and an uncanny depth of perception of human nature. This book is destined to join the library of Civil War military classics along with Douglas Southall Freeman's *Robert E. Lee* and *Lee's Lieutenants*, Bruce Catton's trilogy on the Army of the Potomac and his *Centennial History of the Civil War*, and Kenneth P. Williams's *Lincoln Finds a General*. It may be equaled by historians writing about the Civil War in the West, but it will not be surpassed.

ROBERT HARTJE  
Wittenberg University

ROBERT F. DURDEN. *The Gray and the Black: The Confederate Debate on Emancipation*. Baton Rouge: Louisiana State University Press. 1972. Pp. xi, 305. \$10.95.

Several scholars, as Robert F. Durden points out, have published studies of the discussions among Confederates in the winter of 1864-65 that culminated in the enactment in March 1865 of the law providing for the enlistment of slaves in the Confederate army. But Durden maintains in *The Gray and the Black* that those discussions were part of a larger debate among Confederates over whether their government should adopt a policy of emancipation. Durden's book describes that larger debate over emancipation by reprinting portions of it in chronological order as it was carried on in newspaper

editorials, letters, addresses, and other such sources. He has encased those materials from the 1860s in a running narrative commentary written by himself, and to this he has added a seven-page "Note on the Historiography of the Problem."

The Confederate debate over emancipation as described by Durden was motivated primarily not by humanitarian considerations but by the prospect of military defeat. The goal of those Confederates who urged emancipation was to increase the manpower in the Confederate armies and/or to secure the support of European nations; their rationale was that winning Confederate independence took precedence over preserving slavery. The highest ranking Confederate who advocated emancipation was the Confederate president himself, and the major focus of *The Gray and the Black* is upon Jefferson Davis and his message to his Congress of November 7, 1864. In that message Davis recommended the purchase by the Confederate government of 40,000 slaves, who would be used as laborers with the Confederate army, and who would be promised their freedom on "discharge after service faithfully rendered."

Durden has performed a service of great value in reminding us that Confederates debated emancipation among themselves and in describing and analyzing that debate more completely (so far as I am aware) than any previous scholar. My guess is that all readers of *The Gray and the Black*, even those readers who have studied the Confederacy, will learn much from it. Moreover, Durden makes illuminating comparisons between the Confederate experience and that of the Union, as well as many thoughtful comments on the significance and meaning of what he describes.

*The Gray and the Black* contains generalizations about opinions of Confederates concerning emancipation that raise difficult problems of evidence. The advocates of emancipation are described as a minority, opposed by the great mass of Confederates. But the large majority of the selections reprinted in the book were written either by high ranking Confederate officials or by newspaper editors. These selections may constitute evidence of the views of "leaders" or of the "elite public," but not necessarily of the mass of white and black Southerners. In fair-

ness to Durden, he has simply followed the practice of most historians (myself included) in generalizing about mass public opinion when his evidence pertains chiefly to "leaders." I think that historians (myself included) could and should be more precise and accurate in generalizing about opinions, and I regret this weakness in an otherwise valuable book.

THOMAS J. PRESSLY  
University of Washington

ROLLIN G. OSTERWEIS. *The Myth of the Lost Cause, 1865-1900*. [Hamden, Conn.: Archon Books. 1973. Pp. xii, 188. \$8.50.

In this slim volume Professor Osterweis returns to a theme sounded in his earlier work, *Romanticism and Nationalism in the Old South* (1949). He here rounds out the analysis by an examination of the persistence of antebellum Southern romanticism into the post-1865 era in the form of the myth of the Lost Cause. In eleven closely written and largely self-contained chapters he sketches the emergence of the myth in the aftermath of defeat; its institutional expression in the original Ku Klux Klan, the United Confederate Veterans, and the United Daughters of the Confederacy; its literary evocation by Southern regionalist writers, poets, reports, and orators; its dissemination to Northern readers by circulation-hungry editors of national periodicals; and its ultimate triumph on Northern stages, in Northern and Southern textbooks and classrooms, and in segregated Southern churches. An epilogue traces the checkered career of the myth through the vicissitudes of the twentieth century into the social turmoil of the 1960s.

Methodologically, Professor Osterweis has relied on a concept of myth articulated by Ernst Cassirer and Susanne K. Langer. "A myth," he has noted, "usually has only a shadowy basis in empirical fact; its power depends upon the effectiveness with which it displays in symbolic form the value aspirations of a people" (p. x). Given this orientation, the selection of evidence tends to be impressionistic or illustrative; younger historians with quantitative, behavioral, or social structural biases will be dismayed at the lack of rigorous methods of proof and by the hypostatization of explanatory concepts.

Chronologically this work reflects the presuppositions of the debate on the Reconstruction era as it stood in the late 1940s and early 1950s. The updating of sources in the footnotes seldom is reflected in substantive changes in the main text of the argument. Given these limitations of method and scope, the book is still important for the questions that it raises and attempts to answer: Why has a romantic myth persisted over time, in spite of the destruction of the social setting that gave it birth? How can the intellectual historian take into account the arational, emotive symbols that shape the content of popular thought? Why were whites in the North so willing to accept the myth?

ROSS EVANS PAULSON  
Augustana College,  
Rock Island

MARTIN E. MANTELL. *Johnson, Grant, and the Politics of Reconstruction*. New York: Columbia University Press. 1973. Pp. 209. \$9.00.

ERNEST MCKAY. *Henry Wilson: Practical Radical. A Portrait of a Politician*. (National University Publications, Series in American Studies.) Port Washington, N.Y.: Kennikat Press. 1971. Pp. 262. \$11.00.

RICHARD H. ABBOTT. *Cobbler in Congress: The Life of Henry Wilson, 1812-1875*. [Lexington:] University Press of Kentucky. 1972. Pp. xii, 289. \$13.50.

To the already considerable body of scholarly works on the political history of Reconstruction we may now add three more. *Johnson, Grant, and the Politics of Reconstruction*, a revised version of the author's doctoral dissertation, purports to be a fresh examination of the election of 1868 and the role of Ulysses S. Grant in Radical Reconstruction. Unlike most recent historians of the Radicals, Mantell downplays their moral platform and characterizes the Republicans in general as shrewd politicians who enacted what legislation they could and then retrenched themselves in power by picking a sure-fire presidential winner in 1868. He also suggests that bonds and greenbacks were issues of as much importance as Negro suffrage in the Midwestern congressional elections of 1867. But none of this is entirely new, nor does Mr. Mantell really fulfill his promise to shed fresh light on Grant as a politician.

While the general's actions and statements in 1866 and 1867 may be interpreted, as they are by Mantell, to favor the Radicals, the author has not done very much to enlighten us as to why Grant acted the way he did. Grant's political motives and sympathies remain an enigma. The book has all the ponderous earmarks of a dissertation, replete with obvious statements like this one: "In summary, the northern political system showed the overriding influence of the war" (p. 147). The footnotes contain an impressive array of primary sources, but most of these materials have been thoroughly digested elsewhere. This is the kind of thesis that should have been presented to the profession in two or three concise articles.

Henry Wilson has long stood in need of a biographer, and now he has two. The junior senator from Massachusetts might very well serve as the archetype of a Civil War Republican, but neither author has explored this possibility fully. Wilson was born in poverty in a small New England town and spent his boyhood and youth as an indentured servant. From these humble beginnings he went on to learn to read and write and eventually acquired a small shoemaking business. The "Natick Cobbler" then challenged the political hegemony of the first families of Massachusetts in his attempt to ride the crest of a new antislavery party into government. Wilson's life and views affirm in microcosm the ideology Eric Foner attributes to antislavery Northerners in his recent study of the Republican party in 1850s. Wilson's self-improvement was evidence of the harmonious interests of worker and businessman, the value of the Protestant ethic, and the reality of social mobility. He argued from the 1840s that an insidious "slave power" governed in Washington and that slavery would collapse if the federal government withdrew its support from the institution. At the same time, Wilson seems to have been less racist than the average Republican; his own servitude gave him a lasting empathy with blacks in bondage. He had consistently supported education for Afro-Americans and an end to discrimination and segregation long before such measures became official Republican policy.

The Republican party was primarily developed to serve as a political vehicle for these values, but Republicans had to be as ruthless

as other politicians to enact their point of view. Unlike so many biographers of the Radicals neither McKay nor Abbott belabor the idea that Wilson was inconsistent because he was both an idealist and a consummate politician. He was entirely capable of disguising his most radical opinions if the populace was not yet ready for them. He often sought to disassociate himself from the more radical Abolitionists and temporarily became a Know-Nothing in the 1850s when nativist sentiment hit its peak in Massachusetts. No other politician was more responsible for building a Republican organization in Massachusetts. During the Civil War he became chairman of the Senate Committee on Military Affairs and was instrumental in drafting the conscription bill that allowed draftees to escape military service by paying three hundred dollars or providing a substitute. He promoted bounties to attract volunteers and used his influence to assist Governor Andrews in obtaining black recruits in the South to fill Massachusetts quotas, thus avoiding the drafting of needed factory workers. Like most Republicans Wilson thought it best to devise policies that would promote productivity (keep workers working) and harness blacks to fight for their own freedom. Wilson consistently aligned himself with other Radical Republicans during Reconstruction, and in 1872 the party rewarded his long service by making him the vice presidential nominee. Wilson had long since given up his shoe business and had lived modestly on his political salaries; he had to borrow money from Charles Sumner to buy a suit of clothes for the inauguration. He died in office in 1875.

These are traditional political biographies and are based on almost the same primary materials. Neither author has used secondary sources to any great extent or done as much as he might to put to use the work of Eric Foner, John and LaWanda Cox, W. R. Brock, or David Montgomery. Both amply tell the story of Wilson's life, although Mr. Abbott's writing is the more precise and lucid.

Students of Reconstruction have been overwhelmingly interested in political history and these three authors are no exception. But as Willie Lee Rose and David Montgomery suggested some time ago the real action may have taken place elsewhere. These books round out

our political knowledge, but they do nothing to break new ground in the social and economic history of Reconstruction.

SHARON HARTMAN STROM  
*University of Rhode Island*

JIMMY M. SKAGGS. *The Cattle-Trailing Industry: Between Supply and Demand, 1866-1890*. Lawrence: University Press of Kansas. 1973. Pp. ix, 173. \$8.00.

This study is illustrative of the current reaction of historians to the romanticized views of Western economic enterprise, a new emphasis upon institutions rather than biography. Recent studies of the fur trade, mining, and the range-cattle industry have illustrated the ways in which American business objectives and methods were practiced on the frontier as well as in Eastern urban centers. Slowly but surely the frontier experience is being integrated into the economic history of the United States, and its impact on foreign trade and international relations in the nineteenth century is being made apparent. Jimmy M. Skaggs is specifically concerned with cattle-trailing contractors, entrepreneurs who seized the opportunity for profits from delivering vast herds of Texas cattle to the Northern market. They were typical businessmen in the Gilded Age. The importance of this little-known group of middlemen is appreciated when one recognizes that not more than ten to fifteen percent of all cattle moved to market were handled by the lionized cattle barons who raised the livestock. That job was handled by contractors, "hip pocket businessmen," who left few records, but Skaggs has located and analyzed evidence on twenty-two men either working alone, in partnerships, or with a family enterprise. The dominant characteristic of the group was the ability to improvise and innovate, thus no two contracting groups functioned the same way, nor did individual contractors find it expedient, apparently, to try the same scheme twice. Some were financially successful enough to become cattle barons, real estate promoters, or bankers in later years. Both the railroads and the Midwestern slaughterhouse towns reaped an economic benefit from the work of trail contractors. Had their enterprising genius and innovative skills extended to establishing large

meat-packing companies in Texas and the rail facilities to ship eastward, their significance in the nation's history would have been even greater. Skaggs suggests that the cattle-producing West and Southwest were colonial outposts of the East in a "mercantile framework," and speculates on the impact to the economy of the Western states had the meat-packing industry been centered there.

This is not a pretentious book. Four of the eight chapters, probably the best, have been previously published as articles in regional journals. The additional material, however, fills important gaps, achieves unity in handling the topic, and provides interested readers easier access to the material. Skaggs has performed a service in focusing the attention of historians on an important but neglected aspect of the cattle business.

W. TURRENTINE JACKSON  
*University of California,  
 Davis*

TED C. HINCKLEY. *The Americanization of Alaska, 1867-1897*. Palo Alto, Calif.: Pacific Books. 1972. Pp. 285. \$8.95.

DAVID WHARTON. *The Alaska Gold Rush*. Bloomington: Indiana University Press. 1972. Pp. xiii, 302. \$8.95.

Ted C. Hinckley has produced a masterfully written, thoroughly scholarly book. *The Americanization of Alaska* is essential reading for anyone concerned about the Far North's development. Hinckley's study is both an intelligent reconstruction of the past and a contemporary document, contemporary because it concerns Alaska's current pressing problems: native acculturation, the clash between conservationists and exploiters, a population overwhelmingly urban despite all the breast beating about life in the wilds, and a precariously narrow economic base composed of absentee-controlled natural resources.

These problems existed from the United States purchase of the Great Land in 1867, through the abandonment of military rule ten years later, through the first Organic Act of 1884 making Alaska officially a territory, and through the opening scenes of the Klondike stampede of 1897-98. The issues were fought out mostly on the Alexander Archipelago and the south-

eastern littoral against a backdrop of silvery green, spruce-covered hills. Hinckley argues conclusively that the southeastern panhandle had passed beyond its uncertain pioneer stage by 1897 and was ready for separate statehood. Its stability rested, he finds, upon a tripod of effective government, economic development (sealing, fisheries, and gold), and social maturation. While pursuing his argument the author chronicles a good many setbacks such as Sitka's wilting in the postpurchase years. He paints a number of lively, warts-on portraits, including those of the dynamic Presbyterian missionary Sheldon Jackson and two of Jackson's early antagonists, Governors John H. Kinkead (1884-85) and Alfred P. Swineford (1885-89).

Hinckley convincingly demonstrates the differences between frontiers in Alaska and in the contiguous West. Alaska was irrelevant to Reconstruction and post-Reconstruction concerns. Its frontier was urban or, in those days, town centered. Alaska was far away from the rest of the country, and its white population was numerically insignificant. These circumstances delayed its rise to full territorial status. Hinckley's sympathetic but firm rejection of the inevitable sourdough wails about federal "neglect" places him in the revisionist school of Alaska historians.

What was understandable in the nineteenth century is not acceptable in the twentieth. Hinckley closes with the warning that Alaska's survival as a wilderness area requires careful policy decisions. The policy makers, and their staffs, could begin by reading this book.

David B. Wharton's *The Alaska Gold Rush* takes up where Hinckley leaves off, but only in a chronological sense. Wharton recounts the human drama of gold stampedes in the Canadian Yukon and in Alaska west of the panhandle. His is a good popular history. He writes well without indulging in cloying purple passages. He has traveled the old trails, visited the decayed mining camps, and has done more archival research than most writers of the genre. Yet his book falls short of reaching the standards represented by Hinckley's study.

The nonspecialist may glean some entertaining, useful information from *The Alaska Gold Rush*. Wharton, however, deprives his reader when he fails to provide anything like the con-

ceptual framework guiding Hinckley's equally facile pen. What about the political impact of the Alaska gold discoveries? Wharton is mute. What about the extension of federal activity northward? Army officers, telegraph building, and road construction are mentioned, but not related to the growing bureaucratic effort in the territory. What about native acculturation? Wharton limns a stark, oversimplified portrait: a few traders and missionaries befriended the natives, but other whites were mean.

Wharton's paean to Alaskan individualism strikes a false note. He parades all the well-known characters—"Soapy" Smith, Jack McQueston, Trader Bean, "Tex" Rickard—and others less well known. Then there are the women—the unsung heroines of the trail who worked diligently alongside their men. Or may we now call them "sung" heroines, for their exploits have been chronicled so often? The individualism in all of this, if individualism is the name for it, was incidental and often bizarre. Most stampedeers did their work for companies or under other cooperative arrangements. The gold seekers reconstructed an elaborate community life when they arrived in the Far North, an effort Wharton depicts only to deny. Frequently he confuses struggle and hardship with individualism.

Wharton is concerned with giving his reader the "truth" and the full story, but his narration of the Nome claim jumping is partial and simplistic. His discussion of Valdez railroad developments is partisan and erroneous. The general reader to whom Wharton's book is addressed probably will enjoy the sourdough tales and profit from some of Wharton's judgments about the last stampede. If he has time for but one of the books, however, he should join the specialist in reading Hinckley's.

WILLIAM H. WILSON  
North Texas State University

GERALD ROSENBLUM. *Immigrant Workers: Their Impact on American Labor Radicalism*. New York: Basic Books. 1973. Pp. vi, 189. \$8.95.

Why has the United States not generated a sustained radical working-class movement? Professor Rosenblum, a sociologist, brings to bear upon this historical problem the theories of modernization. His answer in brief is that dur-



ing the most intensive phase of industrialization (1880-1910) a massive immigration muted the "strains of discontinuity" thus forestalling the mobilization of labor requisite for its radicalization. The "new immigrants" of peasant origin became "expedient members" of American society related to it only by a "cash nexus." Lacking basic socialization in American values, retaining a primary orientation to their homeland, and living in insulated "ethnic subsystems," they did not experience the changes wrought by modernization as discontinuities. Willing to accept wage labor and technological innovations, the immigrants were "poor recruits for radical activity." Rosenblum recognizes that these same immigrants often participated in union activities and labor struggles. But he interprets this involvement, even in the IWW-led strikes of Paterson and Lawrence, as an expression of conservative "job consciousness" rather than revolutionary class consciousness. Thus the immigrant workers are viewed as largely responsible for the "unique characteristic" of the American labor movement: its business unionism.

Native workers, on the other hand, as "genuine members" of American society, were mobilized and radicalized by the contradictions between values and actualities arising from modernization. They and those immigrants coming from industrial backgrounds (Germans and East European Jews) were the source of all labor radicalism. In this scheme the Knights of Labor, the American Socialist party, the Western Federation of Miners, and the IWW in its more extreme form are interpreted as manifestations of the "strains" experienced by native workers in the course of modernization. However, the movement of acquiescent foreigners into the most exploited jobs and the rise of the natives to more favorable positions curbed this radical response.

In my judgment the author has substituted a myth of native radicalism for the myth of immigrant radicalism. Rosenblum's conclusions flow from his initial assumption that native Americans and new immigrants comprised ideologically homogeneous and contrasting entities. Such a dualism, however, can not stand the test of empirical evidence. If native workers responded radically to the strains of modernization how then does one explain the lack of a

radical labor movement among Southern textile workers? If East European peasants were immune to radicalism how then does one explain the socialism-IWWism-communism of the Finns? A pluralistic model that would comprehend the broad spectrum of experimental-ideological sets which the industrial recruits, both native and foreign born, brought to the factory would, it is suggested, provide a more fruitful strategy for future research.

Although Rosenblum asserts that this essay is "not intended as single-factor theorizing but, rather, theorizing *about* a single factor," his argument has a dogmatic tone about it. Other explanations of the conservatism of American labor are dismissed rather summarily. While the study draws upon statistical sources and monographs for supporting data, one has the feeling that evidence which does not support the argument is either ignored or argued away. The author's familiarity with the relevant historical literature appears to be somewhat spotty. Among the historians whose works are not cited are Herbert Gutman and Victor Greene. However, Rosenblum should not be held responsible for the deficiencies of the historical literature upon which he does rely. The fact is that the labor movements among Eastern and Southern European groups (excepting the Jews) have been little studied by American historians.

Rosenblum deliberately formulated his argument in bold terms so, as he says, to provide critics with a "large target." Certainly he has provided historians with a challenging hypothesis that ought to stimulate discussion of this important issue.

RUDOLPH J. VECOLI  
University of Minnesota,  
Minneapolis

JAMES B. MURPHY. *L. Q. C. Lamar: Pragmatic Patriot*. (Southern Biography Series.) Baton Rouge: Louisiana State University Press. 1973. Pp. 294. \$11.95.

We now have a second major biography of the distinguished nineteenth-century politician, Lucius Q. C. Lamar of Mississippi, one of the few secessionist leaders to become prominent in national political life after the Civil War.

Murphy's recent work immediately invites comparison to W. A. Cate's study, *Lucius Q. C. Lamar: Secession and Reunion* (1935).

Why another biography? While Murphy never formally justifies his effort, it is clear from the text that this is revisionist history, less in terms of new facts or sources than interpretation. The current work does indeed, and perhaps properly, attempt to deflate Cate's overly eulogistic approach to Lamar. Nevertheless, this new interpretation—aside from some useful reassessment of Lamar's role in the Compromise of 1877—suffers from a basic lack of evidence. Murphy's characterization of Lamar as a politician without principle, who flew with the wind, just does not fit. Of course Lamar was often pragmatic—any politician must be—but his career also demonstrated great political courage on more than one occasion, enough that he was included in the late President Kennedy's *Profiles in Courage*. As for virtue and honesty, no one, including Murphy, has successfully accused him of a lack of either in a day when both characteristics were in extremely short supply. If Murphy is correct that Lamar's image was somehow contrived (pp. 272–73), he succeeded in accomplishing something almost no other nineteenth-century politician was able to do.

The most distressing difficulty with *Pragmatic Patriot*, however, is that at no point does Murphy's Lamar "come alive." The man whom Henry Adams once described (*Education*, p. 185) as an extraordinary personality "quite unusual in social charm" is faceless here. And Cate's extensive documentation of Lamar's great skill as an orator and raconteur is almost completely missing.

Unquestionably Lucius Q. C. Lamar had style. Cate succeeded in portraying him in terms that Henry Adams would have recognized and in a style of writing equal to that of the subject. The present biography, by contrast, is terribly dehydrated (it is half the length of the earlier work). It has the accuracy and swift sequence of a chronology, but also most of its defects as history. The plot is there, but the story is gone, and along with it, I am sad to say, most of Henry Adams's (and Cate's) Lamar.

PAUL P. VAN RIPER  
Texas A&M University

JOSEPH LOGSDON. *Horace White, Nineteenth Century Liberal*. (Contributions in American History, number 10.) Westport, Conn.: Greenwood Publishing Corporation. 1971. Pp. xiii, 418. \$13.50.

Those who judge the merits of a biography by their desire to spend an evening in the company of its subject had best stay away from this one. Horace White, editor of the *Chicago Tribune* during the 1860s and 1870s and of the *New York Evening Post* during the 1880s and 1890s was, according to Henry Watterson, "an iceberg," and White himself stares out from the frontispiece to this volume looking not so much stiff as stuffed. It is thus a measure of Professor Logsdon's commitment to the craft of history that he has provided us with this detailed and enormously suggestive study of White as a figure symptomatic of nineteenth-century political and ideological dilemmas.

Although Logsdon's brilliant discussion of High Calvinist culture in the upper Midwest during White's boyhood provides clues to his personality, the book's focus is on his career. By anyone's standards Horace White was "important." As a young journalist in Chicago, he became active in the Illinois Republican party and in the antislavery National Kansas Committee. When war broke out White went to Washington to report on politics as a *Tribune* correspondent and then as a free lance. In 1865 he returned to Chicago to buy up John Locke Scripps's shares in the *Tribune*, of which he soon became editor-in-chief. In this capacity during the next ten years, he brought the paper national prominence and considerable political power through his advocacy of the Radical position on Reconstruction and equal rights.

In a Republican era, White rode the tide of Republicanism to wealth and influence. When he saw that tide ebbing, he looked for a new wave in the organization of the Liberal party of 1872. He guessed wrong. Defeated both in the Cincinnati convention, where he was a Trumbull man, and then more severely in the general election, White, and by extension the *Tribune*, lost not only prestige but also the close ties with the Illinois Republican party upon which the success of the paper depended. Where the defeat was personal for White, the *Tribune's* loss of status as a party organ threatened the paper's financial viability and

ultimately led to White's removal as editor in 1874.

In a well-ordered world White's career would have ended there. But then this would have been a less interesting book, a careful and well-documented but extremely limited study of the inside of politics during the mid-century. Instead, White went about picking up the pieces. With cash obtained from the sale of his shares in the *Tribune*, he joined Henry Villard in a variety of business enterprises, including purchase of the New York *Evening Post* and the *Nation*. All of these turned out to be financially profitable for White, and his return to journalism as an associate of Carl Schurz, E. L. Godkin, and Wendell Phillips Garrison restored him to a position of some prominence within the Republican party. As a spokesman for the neo-Whiggery of this group of Mugwumps and free-traders he could never achieve the influence he had possessed when he rode the Radical tide in the 1860s. Now he rode the wave of the past. Mistrustful of democracy and its consequences, like others of his generation he found himself an outsider in American politics, yet unwilling to pay the price to become an insider. Instead he turned to the study of political economy as a way of understanding his alienation and discovered structural defects in the American system at all levels. Real politics he could not understand, however, and, again like others of his generation, he reacted with equal hysteria to the emergence of class conflict and the exposure of corruption in government, to imperialist ventures abroad and high-tariff legislation at home, as if these were of equal moment.

It is the emergence of White's conservatism after the mid-1870s that most intrigues Logsdon, as an example of the "crisis of Liberalism" in the nineteenth century. Unfortunately, as an ideologue White characteristically made public problems into private issues, rather than the reverse, and his career can best be understood as a sometimes successful and sometimes unsuccessful attempt to follow the main chance.

HENRY D. SHAPIRO  
*University of Cincinnati*

JULIUS WEINBERG. *Edward Alsworth Ross and the Sociology of Progressivism*. Madison: State Historical Society of Wisconsin. 1972. Pp. xii, 260. \$8.95.

A product of the middle border and a conspicuous member of the first generation of academic social scientists in the United States, Edward A. Ross (1866-1951) was the most popular sociologist of the Progressive era. He was, as Harry Elmer Barnes once said, "a crusading sociological publicist for a whole generation." The author of this trenchant, well-written biography does not portray Ross as a paradigm of the Populist or Progressive "mind," nor does he present him as a typical example of the muckraker or the prohibitionist, the reformer of the law, the nativist, or the rising, turn-of-the-century academician. Ross played each of these roles, but he managed to transcend all of them. Weinberg interprets him as a transitional figure in modern American intellectual history, a critic who sought "to bring into harmony the old and the new, to reconcile the values of the nineteenth-century, rural, Anglo-Saxon, Protestant countryside with the requirements of a twentieth-century, urban-industrial, polyglot nation" (p. xii).

Weinberg's study is based on a large collection of Ross papers and on the sociologist's voluminous publications. The result is an authoritative account of Ross's intellectual development and academic career, a terse, judicious evaluation of his scholarly writings, and an illuminating treatment of his place in American reform. Weinberg, while showing the limitations of Ross's sociological thought, makes a convincing case for his contributions as a creative scholar. In *Social Control: A Survey of the Foundations of Order* (1901) and other significant works Ross emphasized the need for an organic view of society and a positivistic methodology; he produced a pluralistic theory of social forces that seemed to reflect the realities of American life, and he stressed a concept of social processes that struck a balance between group determinism and individual freedom. Students of the Progressive period will find the chapters on "The Sociologist as Progressive" and "The Sociologist as Nativist" particularly instructive, though some readers may share my disappointment that a more thoroughgoing synthesis of the various elements in Ross's progressive thought was not attempted.

DEWEY W. GRANTHAM  
*Vanderbilt University*

GEORGE E. MOWRY. *Another Look at the Twentieth-Century South*. Baton Rouge: Louisiana State University Press. 1973. Pp. ix, 90. \$4.95.

GEORGE BROWN TINDALL. *The Disruption of the Solid South*. (Mercer University Lamar Memorial Lectures, number 14.) Athens: University of Georgia Press. 1972. Pp. xiv, 98. \$4.00.

These two slender volumes of lectures update, if they do not materially revise, a chapter in more recent Southern political history. In the Louisiana State University Fleming Lectures Professor Mowry examines Southern political history against a comparable agrarian background of the western Middle Western states. He does this by constantly tapping the underlying vein of conservatism in the two regions. The Midwestern section after 1900 he says bore remarkable similarity to the South in its one-party loyalty, and the results flowing therefrom were approximately the same. Much of the substance of this provocative, brief study is taken up with comparisons of political phenomena that have controlled the mind and reactions to central issues in the two regions.

Mowry, like his colleague George Brown Tindall, concerns himself with the disruptive forces at work in the country that have tended to shatter Southern economic and political traditions. He takes a longer perspective on these forces than does Tindall. In his lecture dealing with "The Paradox of Southern Conservatism" Mowry traces the course of change through the political behavior of John Sharp Williams, Josiah Bailey, and James F. Byrnes. Mowry concludes that perhaps the party label meant little if anything to these three in the face of their determination to use it as an instrument to "maintain the existing socioeconomic society at home in the South, and, of course, to secure their own personal careers."

Mowry's final lecture, "The Persistent Elite," examines the anatomy of Southern conservatism in a high degree of reality. It was the issues rather than the romance that governed the course of Southern conservatism. Of these there were threats, antilynching legislation, taxes, wage scales, internationalism, race, and agriculture. Mowry's final lecture constitutes a rather full but succinct analysis of the course of action and reaction in the South during the past half century. The Southern

conservative elite exercised a remarkable control both regionally and nationally.

George Brown Tindall has dealt with the rise of the Republican party in the South largely since the inception of the Eisenhower ground swell in 1952. He has, obviously, emphasized the campaigns of Eisenhower, Goldwater, and Nixon as their Southern strategy approach had a bearing upon a region already involved in an enormous upheaval of social and economic change. Goldwater, he says, performed a role in the South similar to that of Al Smith in the Northeast in 1928. He helped to consolidate urban labor and ethnic groups.

Tindall gives a penetrating analysis of the efforts to wean the South over to the Republican party. He has described in some detail the "Southern strategy" as it has worked from within the region. He parades some of the master builders who have sought to effect a Southern defection from political traditions. But whatever prognostications about the degree and direction of change that has already occurred or will take place, one question still remains. What effect will the end of the George C. Wallace hold on a significant segment of voters have on the two major parties? Tindall views the change that has come within the sectional political organization during the past two decades as beyond concept in 1940. For the first time, the region has approached a nationally recognizable two-party system that gives some promise of durability.

These brief studies signal an end to traditional Southern social and political history. The South has felt the impact of enormous economic and social forces. Too, the impact of worldwide opinion has at least made the region a self-conscious one. Both sets of lectures are penetrating in their scholarship and provocative in their conclusions.

THOMAS D. CLARK

*Eastern Kentucky University*

HARVEY A. LEVENSTEIN. *Labor Organizations in the United States and Mexico: A History of Their Relations*. (Contributions in American History, number 13.) [Westport, Conn.:] Greenwood Publishing Company. 1971. Pp. x, 258. \$11.00.

This is a well-written survey of seven decades of U.S.-Mexican labor relations. Professor

Levenstein demonstrates that during most of this century labor leaders on both sides of the border have favored contacts for a variety of personal, ideological, and practical reasons. Some of the factors were Sam Gompers's vanity and desire to seal off the United States from revolutionary unionism, American labor's desire to halt strikebreakers and immigrants at the border, Mexican labor's hope of eliminating discrimination against Mexican workers in the United States, and the desire of both movements to be involved in political decisions in their respective countries.

The book's coverage is best, or perhaps more meaningful, during the years of Wilson's presidency. The period was marked by Sam Gompers's support for the Mexican Revolution and his creation of the Pan-American Federation of Labor. The account of later years treads more familiar ground and also seems less well documented. Between the time this work was finished (as his dissertation in 1966) and the time of publication, Professor Levenstein was a bit reluctant to make revisions. There is nothing, for instance, about such CTM actions as its 1966 rallies in support of the farm workers strike in the Rio Grande Valley of Texas. Nor is there any mention of the influx to the valley of green-card workers who scabbed against Mexican-American picketers while claiming that there was obviously no strike since, after all, workers were crossing the picket lines (an extremely rare occurrence in Mexico). Moreover, the author does not cite Ronald Radosh's *American Labor and United States Foreign Policy*; a dissertation concluded at the same time and place as his own, Henry Berger's "Union Diplomacy: American Labor's Foreign Policy in Latin America, 1932-1955"; or any of the dissident labor press in the U.S. and Mexico.

Though the reasons for contacts between the two labor movements have doubtless been real enough hardly a single issue has even been resolved as a result of these contacts. Indeed labor relations across the border have meant so little that allegedly important meetings have been postponed for years because of the cost of travel, and some leaders have regarded the expense-paid trips as a primary benefit of the boring conferences. Finally, after the author notes that the vast majority of union members

on both sides of the border played no role at all in the international proceedings and that most of the problems between Mexican and U.S. labor can be handled best at the state and local level, one is almost left to wonder whether this subject deserved an entire book.

GEORGE N. GREEN  
University of Texas,  
Arlington

DONALD SMYTHE. *Guerrilla Warrior: The Early Life of John J. Pershing*. New York: Charles Scribner's Sons. 1973. Pp. ix, 370. \$10.95.

That Father Smythe's research efforts, spread over a twelve-year period, were not misplaced becomes obvious with the most cursory inspection of his notes. Though his subject was no longer living when his quest began, many survived who knew the general during his early days. The result is the existence of valuable data that would be unobtainable now because of the demise of many of his sources. It is hoped that this material will at some future time be made available to other historians.

Smythe's Pershing differs significantly from the stern martinet of popular conception. While not neglecting his subject's driving ambition, the author fleshes out his character into a three-dimensional human being. Pershing could relax, fall in love, and grieve deeply at the loss of most of his family in a tragic fire. A high point is Smythe's discussion of Pershing's courtship and marriage. Pershing's facility in working with people of various races is examined in depth. Any study of Pershing's early life should deal with several difficult questions. Smythe handles the following in a convincing manner: Did Pershing live openly with a mistress in the Philippines and father several illegitimate children? What role did his father-in-law, Senator Francis E. Warren, have in Captain Pershing's being promoted over 862 senior officers to brigadier general? And, what responsibility did Pershing bear for killing women and children during the attack on Bud Bagsak in the Philippines in 1913? One early relationship, that between Lieutenant Pershing and Assistant Secretary of War George Meiklejohn, needs clarification. Smythe mentions that Pershing helped his friend Meiklejohn become assistant secretary, but the author does not

explain how the appointment was secured. The significance of such a close relationship between a junior officer and a high-ranking civilian official is not explored. Pershing corresponded directly with Meiklejohn throughout the Cuban campaign: one wonders what the effect of his frequent letters was on Meiklejohn. As Smythe indicates, Pershing's favor was repaid following his return to the United States when Meiklejohn saw that he was made chief of the War Department's Division of Customs and Insular Affairs, a post that Pershing's friends thought could lead to promotion to brigadier general.

Smythe's photos add much to the book. More sharply drawn maps would have been easier to follow. A map of Jolo was sorely missed. These items distract but little from a fine book. I eagerly await its sequel.

THOMAS R. STONE

*United States Military Academy*

REYNOLD M. WIK. *Henry Ford and Grass-Roots America*. Ann Arbor: University of Michigan Press. 1972. Pp. viii, 266. Cloth \$10.00, paper \$3.95.

This book sets out to document the impact of Ford's products and person on the mode of living and thinking of rural Americans. It begins by describing how the 15 million Model T Fords produced between 1908 and 1927 brought mobility to the population and a fundamental improvement in rural life. Later chapters cover Ford's tractors and agriculture and his views and interventions in such areas as science, economics, politics and education.

The rural masses, who considered him one of them—a champion of the little man—bombed Ford with personal letters. It is through innumerable quotes from these letters and newspapers of the era that the author illustrates the following that Ford attracted as a folk hero up until about 1929. The picture that emerges of Ford's importance in the minds of the rural population is impressive, as is the detailed research carried out by the author to build up that picture.

In the main the wealth of material has been well synthesized, and the book makes easy and interesting reading. Occasionally the numerous quotes used to make a point become something

of a litany—almost as if the author were reluctant to discard any of the pearls uncovered by his laborious research—but this is a minor criticism of a book that gives such a valuable insight into the Ford legend and its effect on rural society.

The book is also objective in its evaluation of Henry Ford; while fully recognizing his farsightedness in many spheres, it also shows his inability to match his thinking to the changing world. Thus we see him in the years before the depression as a progressive champion of the worker and farmer, while during the depression he appears as a man at a loss to grasp events, antagonizing his erstwhile supporters with crass pronouncements. (The depression was "only a state of mind that would pass when people became more flexible" was one of his comments.) Finally, we see him totally unable to come to grips with the New Deal philosophy.

And so Henry Ford the progressive, through stagnation of thinking, had become reactionary. The author could, with advantage, have illustrated this detachment from the interests of the little man with a description of the despotic manner in which he allowed his plants to be run in the last years of his life, a sad change from the man who was the first to pay his workers five dollars a day. Overall, however, this is an excellent book, worthy of a place on the bookshelf of anyone interested in history in the widest sense of the field.

COLIN FRASER  
*Rome, Italy*

DONALD F. ANDERSON. *William Howard Taft: A Conservative's Conception of the Presidency*. Ithaca: Cornell University Press. 1973. Pp. ix, 355. \$15.00.

Professor Anderson, a political scientist, has produced a book that should go far to reduce the prevailing ambivalence in our estimation of William Howard Taft as president. Historians, and especially textbook writers, have tended to give Taft a low rating and then to offer excuses for his mediocre record. Drawing extensively on the voluminous Taft manuscripts and also examining the observations of such contemporaries as Archie Butt, Anderson unfolds a record that is devastating.

Anderson considers Taft's bad relations with

the public and the press; and for this he blames Taft himself. "Taft's indifference toward his public image," Anderson believes, "was the prime cause of his failure as president." Nor does Anderson score Taft high as an administrator, pointing out that he delegated too much authority, neglected to keep himself informed adequately on administrative affairs, was too hesitant to replace undesirable officials, and failed to inspire that "camaraderie and loyalty to the chief executive which had marked the Roosevelt administration." His administrative style gradually transformed the departments into nine independent kingdoms and created political difficulties.

The book contains interesting and meaningful comparisons between the views and performance of Theodore Roosevelt and Taft. On the much discussed matter of the degree of constitutional restraints on a president, the views of the two men were much more alike than their own declarations have led us to believe. After presenting convincing evidence to that effect, Anderson asserts that Taft's "constitutional views permitted him to be as big and strong as a Roosevelt or a Lincoln, if and when he chose to be."

Another similarity lay in the relationship of the two presidents with the Old Guard leaders in Congress. Both men found it necessary, or at least they concluded it was necessary, to cooperate rather extensively with leaders in Congress, such men as Nelson W. Aldrich and Uncle Joe Cannon. "The major differences, however, between the two presidents lay, finally, not in their cooperation with the established leaders, but in the degree of their public identification with them." Anderson's treatment of the nature and the impact of the exaggerated rhetoric of both Roosevelt and Taft is especially important to an understanding of how political styles of presidents can have an enormous effect on the nation.

The author does not examine every important aspect of the Taft presidency, wisely confining his effort to an in-depth treatment of important matters in which as a political scientist he has special competence. The result is a very important contribution to the gradual unfolding of a better and much needed understanding of Taft and his presidency. The

book is so timely, so sound, so fair, so well written, that anyone interested in political history, and especially of the presidency, will find it rewarding reading. One can read the book and in the end retain a good feeling toward Taft because of his decency, intelligence, and likability, and at the same time feel sorry that he occupied the White House.

HORACE SAMUEL MERRILL  
University of Maryland,  
College Park

SALLY M. MILLER. *Victor Berger and the Promise of Constructive Socialism, 1910-1920*. (Contributions in American History, number 24.) Westport, Conn.: Greenwood Press. 1973. Pp. xii, 275. \$11.50.

Professor Miller has added significantly to the limited body of published materials about Victor Berger. This volume is a meticulously detailed and well-documented account of his role in the leadership of the right or "reformist" wing of the Socialist party in its most significant years and in its subsequent decline.

The author presents Victor Berger as an embodiment of Max Weber's thesis that "moral man can participate in immoral society and yet retain his soul." In so doing, Professor Miller takes issue with Daniel Bell's thesis that the failure of the Socialist party in America was its inability to resolve this basic dilemma of ethics and politics, that it was "in but not of the world." She argues that Berger's record renders such an explanation of Socialist failure inadequate. Professor Miller is persuasive in arguing that Berger and his followers satisfactorily resolved the conflict between party ideals on the one hand and American political exigencies on the other. Had it not been for his personal characteristics of egotism, bossism, and aggressiveness, the author argues, Berger might have held undisputed leadership of the party and thus been able to fit it to American conditions. That they did not succeed is an important aspect of party decline.

The study is limited to an explanation of the differences within the party and within the reformist wing of the party during the years 1910-20 and thus becomes a valuable supplement to Ira Kipnis's detailed history of the party from its origin to 1913 with an emphasis on factional disputes.

The author disagrees with historians David Shannon, Kipnis, and Daniel Bell that the loss of the syndicalists in 1912 triggered the slow downward drift of party fortunes. The ramifications of World War I were the most likely factors contributing to the decline of the party. Contrary to Professor Miller's main thesis, it was on the war issue that Victor Berger betrayed the Weberian view and thus condemned the party to diminution. The party, with Berger's concurrence, chose to oppose the war, thus signalizing the failure of his efforts to fit the party to American conditions. "Thereafter," the author maintains, "the revolutionaries refused to relinquish the initiative and Berger witnessed the disintegration of the party he had known."

The author has made use of all the major archives, and the result is a scholarly and substantial contribution to the history of the Socialist party in America.

D. JOY HUMES  
Wells College

JOEL ARTHUR TARR. *A Study in Boss Politics: William Lorimer of Chicago*. Urbana: University of Illinois Press. 1971. Pp. xi, 376. \$12.50.

RICHARD J. CONNORS. *A Cycle of Power: The Career of Jersey City Mayor Frank Hague*. Metuchen, N.J.: Scarecrow Press. 1971. Pp. iii, 5-226. \$6.50.

Bossism has become a fertile historical field in the last decade. The literature of the field tends to cluster at the ends of a sloppy continuum. On one side, most work begins with the characteristics of populations and treats the dance of politics as an expressive act, displaying the potentials of public value agreement and conflict. On the other side, a few books and articles treat government as an industry producing goods and services, distributing them in varied markets under conditions of uncertainty, and inducing the risky investment of scarce resources.

Joel Tarr's study of William Lorimer is an example of the first approach. Lorimer was a turn-of-the-century Chicago politician who in 1912 achieved the distinction of being expelled from the U.S. Senate. The last third of the book, which deals with his abbreviated Senate career, has the flavor of reading the newspapers

for yesterday's scandals. Today's scandals are, however, a difficult act to follow, and I find little delight in old news. The first sections, however, are fascinating. Lorimer was a Republican operating within an ethnocultural setting that encouraged Democratic voting. Struggling with this electoral dilemma by softening the force of polarized political orientations, Lorimer (and countless politicians like him) created a middle ground in which governance was possible.

The form, dynamics, and output of the processes of governance are, however, not easily derived from Tarr's work. The omission is, in part, a measure of the topic. Lorimer, for all the angry charges against him, was not much of a boss. No man held Chicago in his pocket, certainly not William Lorimer. His hold over political resources was fragile; his options limited. Beyond this limitation of topic, the omission stems from an approach and purpose that are embedded in the concentration upon the expressive ethnocultural bases of politics. In the attempt to explain the incidence of electoral change and the symbolic uses of politics, the processes and outcomes of government are ignored, more by default, I suspect, than by conviction.

Richard J. Connor's study of Frank Hague is closer to the other end of the continuum and remedies the gaps in the ethnocultural literature. Connor begins, like Tarr, with an argument that Hague's power in Jersey City was rooted in the style and interests of second-generation Irish Americans. He quickly moves, however, to describe Hague's organization as an integrated bureaucracy conceiving of the population, the institutional structure, and the economy of the city as political resources and converting these resources into desired outputs: streets, schools, hospitals, and strengthened popular commitment. The decay of this commitment in the late 1940s was connected both to rigidities in the strategies of the organization and shifts in the resource base. It is difficult to know which was more important in Hague's defeat, his inattention and constricted recruitment or his failure to keep the streets clean and to counter the declining tax base of the city.

Despite its unpretentious format, Connor's book is one of the most interesting and im-



aginative in the entire bossism literature. It serves particularly well that old-fashioned but still valuable function of history to educate decision makers. It explicates constructive principles that novices can understand and may test both vicariously and in practice. (In contrast, the ethnocultural literature tells them little more than to watch out for "trivial" issues that are highly charged.) The major limitation of these principles is that they are drawn wholly from Hague's policy agenda. In at least one perspective, drawn from a larger concern with urban dynamics, the transition from the Hague to the Kenney machine was trivial. The interesting phenomena in Jersey City was its inability to influence the regional redistribution of population, income, and amenities despite enormous stability in its political leadership. Hague had a generalized and overt commitment to active, "progressive" government, and he was capable of sustaining coordinated policies over long periods of time in a manner rare in the United States. Yet he could not prevent—indeed he could hardly seriously address—the shabbiness that became Jersey City. I would be happier to pass on Connor's principles to my students if he had more fully explicated the constraints Hague accepted, the battles he did not fight and the powers he respected.

SEYMOUR J. MANDELBAUM  
University of Pennsylvania

CLARKE A. CHAMBERS. *Paul U. Kellogg and the Survey: Voices for Social Welfare and Social Justice*. Minneapolis: University of Minnesota Press. 1971. Pp. xii, 283. \$10.00.

Clarke Chambers, author of *Seedtime of Reform* and director of the Social Welfare History Center at the University of Minnesota, has put his expertise to good use in this intellectual profile of the *Survey* and its editor Paul U. Kellogg. Although Kellogg began his career when he joined *Charities Review* in 1901, it was as editor in chief from 1912 to 1952 of the successor journal, the *Survey*, that he made his major contribution to social reform. Between his work for the two magazines, Kellogg coordinated the famous Pittsburgh Survey of 1908, the first important effort to analyze in depth the entire life of a single community by team research. From the Pittsburgh experience, Kellogg's

magazine took its name in 1909 at a time when the social-welfare movement was turning from charity to social action. After 1923 *Survey* was published in two autonomous, though allied, magazines—*Survey Graphic*, to educate the general public, and *Survey Midmonthly*, to keep the new profession of social workers abreast of their field.

Although Kellogg was disillusioned in his hopes of a thoroughgoing reconstruction of American society after World War I, the *Survey* magazines achieved their greatest influence and sustained circulation—30,000 for the *Graphic*—from the 1920s through the early New Deal years. Still, it was never easy going. Contributions from friends and foundations plus the practical skills of Paul's brother Arthur, who served as managing editor until his death in 1934, were necessary to keep the magazines alive. In general, *Survey* maintained a nonpartisan political stance, and Kellogg's initial support of the New Deal was modified by his disappointment over its failure to adopt a more comprehensive Social Security program. From 1935 to the end of the decade the magazines became more lively and independent, but divisions among the staff in regard to the approaching war and Kellogg's own increasingly heavy work load took their toll. After World War II financial problems proved insurmountable. In May 1952, six years before Kellogg's death, and over his stubborn protests, *Survey* published its last issue.

As Chambers points out, *Survey's* dual role, combining professionalism and popular reform, was being assumed by the technical journals and magazines of opinion. *Survey* thus became old-fashioned and out of touch with the post-war generation. By making full use of the extensive papers deposited at the University of Minnesota, Chambers succeeds in bringing to life both the *Survey* and its long-time editor.

ARTHUR A. EKIRCH, JR.  
State University of New York,  
Albany

DONALD E. GREEN. *Land of the Underground Rain: Irrigation on the Texas High Plains, 1910-1970*. Austin: University of Texas Press. 1973. Pp. xvii, 295. \$9.50.

Although specialized studies of particular land

company projects in the Texas panhandle region have appeared before, this volume is the first generalized treatment of the ground-water irrigation epoch in Texas high plains agriculture. It is an excellent study in every respect and brings to national attention the environmental preconditions and technological and entrepreneurial advances that started an agricultural boom in this most unpromising of geographical regions. At the turn of the century notice was taken of the deep-lying aquiferous stratum of Ogallala sand and gravel, varying from two to three hundred feet in thickness, underlying this region. Technological breakthroughs wedded the centrifugal pump and the internal combustion engine, using cheap oil, with sophisticated well-drilling techniques to bring up the precious fluid.

The first boom lasting from 1910 to 1917 was a land promoters' venture that demonstrated the efficacy of the pump in creating the new ground-water irrigation frontier. New conditions prevailed during the depression years when the combination of drought and low prices forced these high plains farmers to accept the new improved pumping technology in order to survive. Ingenious arrangements to pool credit, the introduction of more efficient and cheaper pumps, more powerful gasoline engines, along with experimentation with cotton, grain sorghums, winter wheat, and alfalfa, brought ground-water irrigation agriculture to a take-off point before World War II.

This scholarly depiction of the boom documents every aspect of unparalleled growth that kept farm youth on the land, augmented land values by gigantic strides, and introduced capital-intensive agriculture. The author is at his objective best in his somber portrayal of the decline of this form of agriculture occasioned by a rapidly falling water table. Apparently high-plains Texans are not unlike most Texans who, according to Justice William O. Douglas, adopt conservation measures that are too little and too late. Donald Green concludes that experience under the so-called water conservation statute of 1949 demonstrates that these farmers would rather continue to deplete their nonrenewable underground reservoir at today's scale of pumping than impair their vested rights in capital plant and water rights. All this absorbing monograph requires to make

the story complete is comparative data describing other examples of ground-water irrigation in the nation.

LAWRENCE B. LEE  
*California State University,  
San Jose*

E. CLIFFORD NELSON. *Lutheranism in North America, 1914-1970*. Foreword by KENT S. KNUTSON. Minneapolis: Augsburg Publishing House. 1972. Pp. xvi, 315. \$7.50.

Reading this book is like eating lunch at a Howard Johnson's. Certain kinds of historians' appetites are satisfied while others are left wanting. If one is looking for surveys, outlines, and charts to assist him in understanding the denominational differences, structures, and programs within Lutheranism, then this book will help. More particularly, if one wants to understand the broad outlines of the success and failure of the efforts of Lutheran denominations to unify themselves, then this work, written by a Lutheran professor of religion at St. Olaf's College, a committed ecumenicist, will be gratifying. In clear and lucid writing, Professor Nelson has described the "painful" and "agonizing" efforts of Lutheran denominations to iron out their differences in fellowship rather than demanding consensus prior to such union (p. 167ff).

From a methodological and conceptual standpoint, however, this work is more of the same standard fare that has dominated American denominational histories. With one notable exception this work is an extension of the William W. Sweet "school" of American religious history: Lutheranism is described in terms of its leadership; awkward theological controversies are mentioned but not examined or explained; and the interplay of social, economic, political, and cultural forces that affected American Lutheranism are broached but left undeveloped. For example, one continuing theme of American Lutheranism, according to the author, is the assimilation and "Americanization" of Lutheran immigrant denominations. The author claims, however, that Lutherans were generally opposed to the "culture-religion" of middle America (p. 167ff). Employing H. Richard Niebuhr's phrase, it would appear that many Lutherans endorsed a "Christ against culture"

stance. I see here a contradiction that is difficult to reconcile. How can Lutherans be "Americanized" and, at the same time, resist America's "culture-religion"? If such a proposition is true Professor Nelson would do American historiography a great service by amplifying his explanation.

The one exception to the above criticisms is the author's usage of data collected among Lutherans in 1969-70. This "profile of Lutherans" was the result of a survey given to nearly five thousand Lutherans. It produced some seven million answers to questions about values, attitudes, and beliefs. The survey, known as "A Study of Generations," revealed a wide variety of opinions among Lutherans on topics such as social change, theological commitments, clergy-lay differences, moral stances, and denominational programs. What we would like to know, however, is whether Lutherans qua Lutherans differ from other members of American mainline denominations. At least Professor Nelson gives us a hint as to how American religious history might be more quantified.

The most useful portions of this book may be the many pages of footnotes. They point to the vast reservoir of documents extant in denominational archives, many of which Professor Nelson has combed. Perhaps now we can await from him a fuller explanation of their meaning. After all, lunching at Howard Johnson's does not necessarily preclude dinner at Henri Quatre.

JOHN W. STEWART  
*Hope College*

ROBERT MOATS MILLER. *How Shall They Hear without a Preacher? The Life of Ernest Fremont Tittle*. Chapel Hill: University of North Carolina Press. 1971. Pp. xii, 524. \$12.50.

In a work that is both solid and smooth, Professor Miller continues, through Tittle, to examine twentieth-century American Protestantism, the better to understand American culture and society. He is thoroughly convincing.

Preeminently a preacher, Tittle filled for thirty-one years (1918-49) the prestige-laden pulpit of First Methodist Church, Evanston. Armed with physical vigor, a penetrating voice, an inquiring mind, broad reading habits, a bulldog tenacity, and a sympathetic wife, he con-

vinced his congregation's conservative lay leaders to understand, even if they could not always accept, his advanced Social-Gospel stands. Thus the official board reiterated their support of the ideal of a free pulpit. Throughout his "prophetic preaching," Tittle urged nonviolent resistance, economic reform (he regularly voted for Norman Thomas), racial justice, and ecumenical cooperation, as well as the usual Methodist tilt on temperance. Not content with mere advocacy, Tittle encouraged institutional church involvement in community issues and services. Moreover, he led in fostering the Christian life, through Sunday education, pastoral counseling, and esthetic enrichment based on Tittle's Ralph Adams Cram-inspired reconstruction of the sanctuary. So enthusiastic were his parishioners that one observer ruefully called them "a congregation of damned Tittleites."

Miller offers no "authorized" biography. Yet he builds on unrestricted access to family papers (except for one group of scissors-censored letters to the preacher's wife), the well-kept archives of First Church, and interviews with Tittle's widow, children, other family, and associates. To the resulting immersion in congregational life, Miller adds Parkman-like insights from his own recollections of the Evanston environs. He rarely misses a resource, although the Methodist *General Minutes* might expand the data about Tittle's early Ohio churches. Miller's organization—chronological a third of the way, then topical—both heightens analysis and insures repetitions. The volume's 200,000 words bring a leisurely pace that occasionally threatens surfeit. At least, Tittle frequently speaks for himself, as: "Granted that earth can never be heaven, it hardly follows we should allow it to become hell."

Altogether, the study is well done. Miller has given us a virtually unique, detailed view of a successful local-church pastor at work. What next, Bob? Harry Emerson Fosdick?

THEODORE L. AGNEW  
*Oklahoma State University*

FRANCIS PICKENS MILLER. *Man from the Valley: Memoirs of a 20th-Century Virginian*. Chapel Hill: University of North Carolina Press. 1971. Pp. xvii, 253. \$8.75.

This is an interesting autobiography of an unusual humanistic liberal. From a long line

of Presbyterian clergymen he received an early religious evangelism. From the start of his educational career Francis Miller demonstrated an enduring affinity for causes of a compassionate humanistic nature.

After graduating from college with a Phi Beta Kappa key he joined the YMCA on the national level. His service there gave way to active military service during World War I. At the end of the war he entered Oxford University and subsequently became a Rhodes scholar. As a leader in the British Student Christian Movement he lectured on college campuses throughout the British Isles.

Back in the United States he became a leader in the American Student Christian Movement and later served as secretary of the World's Student Christian Federation with headquarters in Geneva, Switzerland. After serving in Geneva for three years he returned to become a lecturer at Yale. Miller states that his work with WSCF was excellent preparation for his leadership role in the World Council of Churches after World War II.

As field secretary of the Foreign Policy Association, he promoted its expansion throughout the country. When the war began in Europe he organized an American war spirit against Hitler. He joined the Office of Strategic Services. Stationed in London, his contributions were significant in the OSS and later in SHAEF.

With the Army of Occupation in Berlin, he secured valuable German documents that found their way into American archives. In Berlin he witnessed the debasing of the character of the American soldiers through black-market activities and moral decay. He returned to American civilian life as the gentleman from Fairfax.

The last part of this delightful memoir deals with Miller's political activities. As a member of the Virginia Assembly he refused to join the Harry Byrd reactionary machine. Defeated in 1949 by the Byrd-selected John Battle for governor, he lost to Byrd in a race for the United States Senate in 1952. His efforts to reunite the northern and southern Presbyterian churches failed also. He defeated the Byrd machine, however, on the integration issue in Virginia. Indeed, he frequently led movements before the masses were ready for the reforms. As a member of the State Department from 1961 to 1965, he liked Kennedy's innovations

but hated Johnson's "business as usual" approach.

Francis Miller has rendered a service to American liberalism and to the understanding of Virginia politics by publishing his memoirs.

GEORGE OSBORN

University of Florida

PAUL L. MURPHY. *The Meaning of Freedom of Speech: First Amendment Freedoms from Wilson to FDR*. (Contributions in American History, number 15.) Westport, Conn.: Greenwood Publishing Company, 1972. Pp. x, 401. \$14.50.

As with his *The Constitution in Crisis Times*, Murphy points to World War I and its immediate aftermath as commencing a new era for the freedom of speech in American society. This book is different, however, in that it approaches the meaning of free speech less through pronouncements of the high court than from an examination of the sociointellectual record. In this sense it is not constitutional history of the sort we normally expect. Neither should it be compared to the closely argued studies of William Finley Swindler and Zechariah Chafee. Rather, it is a selective chronicle of conflicting interpretations, chiefly during the years 1919-31.

As a narrative, it is richly documented and splendidly told. The ingredients from which attitudes of the time were fashioned are found in the "red scare" mentality of the immediate post-war years: the multiplication of sedition and criminal-syndicalism statutes at the state level; the coal and steel strikes of 1919; New York's legislative fight over the seating of properly elected socialists; and the use of blanket search warrants, raids, and deportations under Attorney General Palmer. From the welter of such events congealed the stiff-necked conservatism of "normalcy."

A good deal of Murphy's material deals with episodes in which the ACLU was involved. And during the 1920s these were primarily problems of labor. Much of this was the result of a suppressive alliance between industrial establishments and local law enforcement officials, as with the violence in Logan and Mingo Counties, West Virginia; beatings by the coal and iron police of Pennsylvania; or shootings at Columbine mine in Colorado. Fundamentalist controversies and academic freedom are men-

tioned but given far less attention than disputes with labor. There is, however, an abundance of information regarding societies like the DAR, the American Legion, and the Better America Federation. And Murphy skillfully traces the reappearing usage by such groups of influences such as New York's Lusk Committee findings or Mrs. Lucia Maxwell's "Spider Web Chart," which purported to demonstrate the interlocking leadership of dissident organizations throughout the United States.

The book finds the Hoover administration marking a liberal turn in attitudes toward dissent. A number of developments are adduced to support this conclusion but especially the Near and Stromberg decisions with their incorporation of federal restraints into the due process clause of the Fourteenth Amendment. If it is difficult to discern the "subtle" meliorism Murphy contends the twenties to have generated, especially in the face of so much oppression and a steadfast commitment to property interests by the Taft court, there are abundant and persuasive grounds for agreeing that the early thirties mark a definite departure from the brutalities of Passaic, Gastonia, and Marion or from the brooding surveillance of the likes of Ralph M. Easley and William J. Burns. And, the book does convincingly document the many divergent employments to which the notion of free speech was put, illustrating the fractured nature of a decade traditionally seen as something of a philistine, sociolegal unity.

Everything considered, as an account of the free-speech issue during the twenties, it is a solidly constructed and indispensable work.

B. CARMON HARDY  
California State University,  
Fullerton

HAYWOOD S. HANSELL, JR. *The Air Plan That Defeated Hitler*. Atlanta: [the author.] 1972. Pp. x, 311.

FLINT O. DUPRE. *Hap Arnold: Architect of American Air Power*. (Air Force Academy Series.) New York: Macmillan Company. 1972. Pp. xiv, 144. \$5.95.

In World War II airpower added new dimensions to warfare both as a part of combined operations and in its own right as an independ-

ent striking force. Tactical air support was requisite for all surface operations, while strategic air campaigns conducted by the United States and Great Britain undermined the will and capability of the Axis powers. Among American air commanders it is fair to say that strategic air operations were always thought to be more important than tactical air support, but, ironically, the historical treatment of tactical air operations has been much more extensive than that of the strategic air campaigns. Most military service histories inevitably cover air support to some necessary extent, but independent strategic air operations have been an intimate concern only in air histories. Where it might have been expected that the U.S. strategic air commanders of World War II would have provided enlightening memoirs, very few of them have elected to do so. In Great Britain, Sir Charles Webster and Noble Frankland have authored a splendid history of the Royal Air Force Bomber Command and British air commanders have been notable memorialists, but British strategic air experience was only half of the story of the Combined Anglo-American Bomber Offensive against Axis Europe. Moreover, British strategic air experience was significantly different from the kindred endeavor of the U.S. Army Air Forces. Through all these circumstances, the historical memory of U.S. strategic air operations in World War II has grown dim and has lost much in perspective in the quarter century since the defeat of Germany and Japan.

In the U.S. Army Air Corps of the 1930s, Captain Haywood S. Hansell, Jr. was already recognized as a coming thinker and planner, and he continued to distinguish himself in the wartime Army Air Forces as a staff planner and strategic air field commander. In the latter role, he commanded B-17s in the United Kingdom in 1942-43 and the XXI Bomber Command in B-29 attacks against Japan in 1944-45. He was recalled from retirement during the Korean War and finally retired in 1955 with the rank of major general, U.S. Air Force. In *The Air Plan That Defeated Hitler*, General Hansell has taken a major step toward telling the story of the U.S. strategic air offensive in Europe in its own right. His narrative recalls the development of U.S. strategic air thinking through the Air Corps Tactical School

in the 1930s, describes the engrossment of the concepts in major Army Air Forces planning prepared in Washington in 1941-42, records the application and modification of the planning that resulted from aerial combat over Germany, and finally appraises the effect of the strategic bombing from the standpoint of original conceptions. As a historical narrative Hansell's effort is important for the perspective that it provides; in addition, his personal reminiscences are significant vignettes of air force history. Some merit of his work is indicated by the evaluation of the American strategic air commander, General Carl A. Spaatz, who has said that this is the "most complete and authoritative book" on the subject that he has seen. *The Air Plan That Defeated Hitler* was privately published and was given a fairly wide distribution. In addition, it may be obtained on interlibrary loan from the Air University Library, Maxwell Air Force Base, Alabama.

In a brief description of General Henry H. Arnold, Hansell gives high praise to the wartime U.S. Army Air Forces commander who deserved the credit for the building of the air forces prescribed in planning. A more extended look at this remarkable air officer is provided by Flint O. DuPre in a highly appreciative biography entitled *Hap Arnold: Architect of American Air Power*. DuPre is a long-time and lately retired member of the Secretary of Air Force Office of Information and offers a brief and highly readable account of General Arnold's pioneer association with U.S. military aviation from his first flights in 1911 through his retirement in 1946. The volume will be of interest to younger readers, but—quite unlike Hansell's *Air Plan*—it offers little to a serious and critical historian.

ROBERT F. FUTRELL  
*Air University*

GARY R. HESS. *America Encounters India, 1941-1947*. Baltimore: Johns Hopkins Press. 1971. Pp. xi, 211. \$9.50.

L. P. MATHUR. *Indian Revolutionary Movement in the United States of America*. New Delhi: S. Chand and Company; distrib. by South Asia Books, Columbia, Mo. 1970. Pp. vii, 169. \$7.00.

Any study of India's freedom movement would reveal that the Indian leaders expected under-

standing, sympathy, and even enthusiastic support for their cause from the American people and the American government. A section of the American people became interested in India's independence and some prominent liberals even championed the cause. By and large, however, the general public in the United States and more particularly the Roosevelt administration were naturally preoccupied with the war against the Axis powers. They were forced to respond to the nationalist struggle in India because of its potential for adversely affecting the war effort in the India-Burma-China theater. The record of the American response, unfortunately however, was not unequivocal and remained ambiguous at its best. The American government was confronted with the dilemma of choosing between the prized ally and the nationalist movement seeking independence from that ally. From the perspective of hindsight we can see that the United States was not really faced with such a clear cut either/or choice in the matter. The British needed the Americans much more than the other way around. By an astute combination of resolute opposition, emotional outbursts, and the sheer force of personality, Winston Churchill was able to brow beat Roosevelt into a retreat. He succeeded in making the U.S. government defer to the British on all important matters relating to "the Raj." Successive American governments traditionally considered India to be the British responsibility. *Habits die hard*.

Roosevelt undoubtedly had a fund of goodwill and sympathy for popular struggles for national self-determination in general and the Indian nationalist movement in particular. The available evidence, as demonstrated by Gary Hess and many other recent Indian and American writers, indicates that Roosevelt believed the British tactics of delay and repression to be wrong, unwise, and likely to be ineffective in any case. But, when forced to choose between the two (the British and the nationalists), he shied away from his better judgment and deferred to the wishes of Churchill, the adamant. Gary Hess delineates the contours of the ambiguous American response during the war and early postwar years with clarity and skill. Since 1945 the U.S. was confronted with similar choices elsewhere in Asia and Africa, and the American response to India's na-

tionalist movement, Hess rightly argues, constitutes an illuminating case study. The detailed analysis of America's encounter with India in the forties is preceded by a rapid, historical survey of the earlier developments in India and the American response to them.

One of the exciting episodes in this story was the so-called "Ghaddar" or the Indian revolutionary movement that flourished in the United States during the first two decades of this century. L. P. Mathur tells of the heroic but futile struggle of the revolutionary exiles who congregated on the West Coast and elsewhere in the United States and Canada. The American government kept a close watch over the activists. According to Mathur, however, they were allowed to carry on their propaganda activities, including plotting and planning against the British rule in India. The British government's protests in this regard were mostly ignored (except for a brief period after 1917) because such scheming unaccompanied by any overt action was not against the American law. But when the Indian revolutionaries started preaching anarchism, they became liable to legal action. Many of the leaders were arrested and deported to India for plotting against the British government in collaboration with some German agents. During this period the attitude of the American government was "liberal," on the whole, Mathur concludes. One only wishes that Mathur in his account of the "heroic" exploits of the revolutionary exiles and especially of the scope and reach of their activities in the United States were less prone to accept at face value the exaggerated versions recorded by the revolutionaries themselves.

The detailed analysis presented by Hess highlights a recurring pattern in the American response to key developments in India and the British policies toward them. The inside stories related by Hess regarding the Cripps plan, Johnson mission, Chiang's telegram to Roosevelt, the Merrill plan, the Phillips mission, and the like revealed the nature of the American response and the constraints within which it occurred. The U.S. government felt compelled to recognize the need for a new initiative to break the deadlock between the nationalists and the British; made sincere behind-the-scene efforts to put across to Whitehall some potentially fruitful avenues as suggestions from a

friend and ally; then came the inevitable Churchillian response—the resolute and inflexible stand, followed by the predictable and quiet retreat on the part of Roosevelt. The British propaganda in the United States was successful indeed, especially after the failure of the Cripps mission. From that time on the tide of popular sentiment in America turned in favor of the British government and against the Indian nationalists. Hess ably brings out Roosevelt's failure to comprehend the continuing importance of India and the impact of U.S. retreat on Indo-American relations. *America Encounters India* is a well-written and well-researched book and deserves wide attention.

B. RAMESH BABU

University of Bombay

CHIN-TUNG LIANG. *General Stilwell in China, 1942-1944: The Full Story*. (Asia in the Modern World Series, number 12.) [Jamaica, N.Y.: St. John's University Press under the auspices of the Center of Asian Studies.] 1972. Pp. xviii, 321. \$6.95.

As one of the most controversial figures in the history of Sino-American relations, General Joseph W. Stilwell has been the subject of many books. Since these works are written by Americans and based on American materials, they have been criticized by Nationalist officials and Chinese scholars as one-sided, representing only the American point of view. In the present work Chin-tung Liang of the Academia Sinica sets out to correct what seem to him unfounded American assumptions and misrepresentations.

After an extensive study of American and Chinese documentary materials, especially the unpublished private papers and official archives of Generalissimo Chiang Kai-shek, Liang came up with a lengthy bill of indictment against General Stilwell. The latter is described as an arrogant, vindictive, jealous character with an intensive disdain and hatred of Chiang Kai-shek, his supreme commander in the China theatre of war. Stilwell, on Chinese evidence, was guilty of "insubordination" five times in a short period of three months between 1943 and 1944. He went so far as to threaten moving the United States forces from China to Russia if Chiang did not appoint him commander of all Chinese forces.

Liang rejects the narrow approach that the

episode of Stilwell's recall can be explained by personality conflicts alone. The author traces the fundamental cause of the conflict to the strategic differences between the two nations. As soon as the island-hopping operations across the Pacific proved successful, the United States de-emphasized China as a military base for attacks on Japan. As strategies changed, China's needs were ignored. She was not represented at the Combined Chiefs of Staff Conference, which decided on strategy; lend-lease material assigned to her was under the control of Stilwell, while neither Britain nor the Soviet Union was subject to such control. As the author shows, this "unequal treatment" inevitably led to conflicts.

Liang considers it a gross injustice that China, cooperating with the United States to the best of her ability, should have been subject to pressures, threats, and abuses from the Americans. His indignant criticism departs from the impersonal manner of the objective historian, but the book is a forceful presentation of the Chinese case. If Liang gives little attention to American interests and difficulties, his arguments for China are firmly based. This provocative study deserves the attention of all serious students of Sino-American relations during World War II.

CHESTER C. TAN  
New York University

WILLIAM M. BUELER. *U.S. China Policy and the Problem of Taiwan*. Boulder: Colorado Associated University Press. 1971. Pp. 143. \$5.95.

During his five years in Taiwan this Chinese-speaking author carefully inquired into the attitudes of Taiwanese and Mainland Chinese. He found the latter committed to but no longer believing in the Chiang Kai-shek-Dulles return-to-the-mainland doctrine. The Taiwanese were uniformly anti-Mainland Chinese and anti-Communist, desiring though not inclined to fight for self-determination for Taiwan. Here, in his chapter on "Political Realities of Taiwan," is Bueler's main contribution.

The bulk of his book, however, is devoted to reviewing and analyzing White House and State Department pronouncements on Taiwan from 1949 to 1970. The review is clear and reasonably well balanced. It shows how the

Korean War caused Truman and Acheson to embrace the Chinese Nationalists; how the embrace tightened under Eisenhower and Dulles, the main architects of America's policy toward Taiwan; how that policy held under Kennedy and Johnson and began to change only after Nixon became president.

The analysis emphasizes Washington's regrettable lack of interest in the Taiwanese independence movement; the persistence of the collapsing-communism-and-mainland-recovery myth as a factor in American policy; and the United States conviction that there is no give in the Communist position on Taiwan (wrong, says Bueler, Peking might "in time, reconcile itself to Taiwanese self-determination").

Like many who write on Sino-American relations, Bueler hopes to influence policy makers. One problem is that he wrote before Nixon's pilgrimage to China, so his advice is based on a situation that has changed. But, since the question of Taiwan remains unresolved, the volume is still worth attention for policy discussions as well as for the historical record.

Saying that, one must add a word of disappointment. The documentation is thin and, unfortunately, is confined almost entirely to American published materials. Even there, much is to be desired in newspaper and magazine coverage. Some pre-1949 material would have been welcome. And it is disturbing to find policy-making discussed with only the most incidental reference to the impact of economic, religious, political, and military forces, and without much consideration of other Asian and world developments.

FRED HARVEY HARRINGTON  
University of Wisconsin

JOHN MORTON BLUM, edited and with an introduction by. *The Price of Vision: The Diary of Henry A. Wallace, 1942-1946*. Boston: Houghton Mifflin Company. 1973. Pp. x, 707. \$15.00.

Henry A. Wallace has often been pictured as mystical and incomprehensible. Having coauthored a biography of the man, I have never thought this to be true. Blum's wartime diary of Wallace reinforces my impression. Wallace's ruminations reflect the same pattern of ideas consistently expressed either in public speeches and writings or privately in correspondence.



They revolve around his idealistic and progressive political philosophy based on the twin themes of domestic abundance and international cooperation. Formulated while a rural editor, they were implemented as secretary of agriculture and expanded and amplified as vice president, secretary of commerce, and a presidential candidate. While the fundamentals of this philosophy remained the same it was the milieu that changed—and this made for controversy. As secretary of agriculture he compromised his ideal of the ever-normal granary with the interests of Southern and Midwestern conservatives (i.e., the so-called marriage of corn and cotton) to achieve a workable farm program. But as vice president and secretary of commerce, and the diary reveals this very clearly, he became less realistic and more doctrinaire in direct proportion to his lack of expertise in the field of foreign affairs. The more difficulties encountered and the less workaday knowledge he possessed, the more prone Wallace became to deal in abstractions. International affairs finally appeared to him only in the two-dimensional framework of one world versus atomic destruction with no other viable alternatives.

Mr. Blum has edited about 25 per cent of the total diary on the basis of what he considered a representative sample. He has included a twenty-four page biographical sketch written in beautiful prose and based on recent authoritative books. Helpful are numerous explanatory footnotes that identify personages mentioned in the text. No attempt was made, however, to correlate the diary entries with other manuscript material, except for the "Columbia Oral Memoir," and with this only occasionally. The only really new information revealed might be designated as political gossip. Here we get a candid appraisal of the duplicity and maneuverings of Franklin D. Roosevelt, the corruption of the big city bosses, descriptions of political infighting, the conniving of presidential assistants, and suggestions of possible illegal campaign contributions. Apparently political skulduggery was not invented with Watergate.

Reactions to the diary will depend upon how one interprets the cold war era. Some will see Wallace as a prophet whose prescience was

amazing, while others will find him a man capable of unlimited self-delusion.

FREDERICK H. SCHAPSMEIER  
University of Wisconsin—  
Oshkosh

*Foreign Relations of the United States, 1947. Volume 3, The British Commonwealth; Europe.* (Department of State Publication 8625.) Washington: Government Printing Office. 1972. Pp. xiv, 1131. \$5.75.

A short review can only begin to express the wealth of this volume, most of which recounts American diplomacy with Britain, France, and Italy and the genesis of the European Recovery Plan during the economic crisis of 1947.

It is fitting that the documents on Britain's trade and currency-exchange crisis appear first, since the solution of this problem was essential to America's European policy. As the British themselves put it, if Britain failed she would no longer be "the differential gear" between the American economy and those of other countries (p. 47). That American officials agreed was shown by their willingness to accommodate the British in interpreting the Anglo-American Financial Agreement of 1945 and by the extension of a further credit of \$400 million at the end of 1947.

In early 1947 American policy makers also expected the precarious economic and political situations of France and Italy to worsen and possibly collapse, leaving the way to power open for the French and Italian Communist parties and stimulating a return to economic autarky by all the European countries. From Paris and Rome the communist danger was portrayed in abundant and knowledgeable dispatches by Ambassadors Jefferson Caffery and James C. Dunn. One should note, however, that Caffery and Dunn also dreaded extremists of the right. Their preference—and that of their superiors—was for a democracy of the center, reflecting still the hopes for Europe that Americans had nourished during the recent struggle against fascism.

In Washington those who conceived the European Recovery Program were concerned about both communism and economic nationalism in Western Europe, so that some interpreters of these documents may well perceive a capitalist, open-door strategy. That, however,

would not be a full reading. These materials also illustrate an American belief in multilateralism as a community value and a genuine concern for the material health of Western civilization. Otherwise, the sense of drama that still pervades the memorandums and dispatches would be inexplicable.

That drama is further explained by the luminous State Department of 1947. Marshall's precise authority, Under Secretary Dean Acheson's rigor and tenacity, and the thoughtfulness of the first Policy-Planning Staff enliven these pages. The department's *esprit* is preserved in Division Chief Charles P. Kindleberger's good-humored recollection of who deserves credit for the Marshall Plan (pp. 241-47). His account of how the secretary of state came, finally, to accept a Harvard degree is both amusing and indicative of Marshall's quiet decency.

Altogether, the story of America, Britain, and Europe in the critical year 1947 is remarkable and many-sided, one which does not fit very well into the "origins of the cold war" mold. Hence it may be fortunate, as well as ironical, that as the State Department archives open, the latter controversy has entered into a somewhat pedantic phase. Perhaps the records of 1947 may now be studied from less sectarian and more historical perspectives.

ROBERT E. BUNSELMAYER  
Yale University

*Foreign Relations of the United States, 1948. Volume 7, The Far East: China.* (Department of State Publication 8675). Washington: Government Printing Office. 1973. Pp. vi, 887. \$6.50 postpaid.

The most revealing aspect of this latest of the China volumes in the Foreign Relations series is its size: 887 pages, quite compact compared to the 2,963 pages for 1946 and the 1,477 pages for 1947 (see *AHR*, 79 [1974]: 249-50). Although its emissaries still struggled in the China tangle as the Red Hornet closed in, by 1948 Washington was shifting its attention back to Europe. Gone are the chapters and appendixes on loans, planes, ships, military equipment, taxes, property rights, transport agreements, and the countless other preoccupations of the White Man's Burden. What remains are Wagnerian final scenes with principals like

Ambassador Stuart, befuddled in Nanking, and Acting Secretary of State Lovett, exasperated in Washington. The supporting cast includes Consuls General Clubb (Peking) and Ward (Mukden) who stuck by their posts sturdily awaiting the People's Liberation Army.

The drama can be grasped neatly in miniature. Holed up in Mukden, Consul General Ward observed on December 31, 1947 that the situation in his bailiwick was "serious but not critical." When we last hear of him 848 pages later, on December 28, 1948, his colleague in Tientsin has said of Ward's headquarters: "Consulate General radio reported unharmed but guarded at all times by sentry. Sentries also guard office and residences." The sentries were Marshal Lin Piao's infantrymen.

Stuart must have driven Washington mad with dispatches, now piously upbeat, now sepulchral. On March 22, for no discernible reason, he claimed that "the friendliness [of the Chinese] towards our country and their trust in our intentions are almost universal and very deep. . . . We are helping this vast, amorphous population to adjust itself to modern conditions after the shattering of its ancient political and social patterns . . . (they) are in no sense a decadent race. . . . I have a sense of exhilarating expectancy despite all the questionable obstructions and uncertainties." By March 31 the situation was "desperate," and "either those of weak heart will prevail," or "those of stout heart will rally round a Generalissimo in some way to restore benevolence to his despotism. . . . We hope for [the] latter, but we fear [the] former."

Such swoops of mood, punctuated by tough advice to concentrate "our military and economic aid in all-out effort to assist the government . . . more bold and more imaginative measures are needed . . . rush shipment of arms and ammunition," etc., roused Acting Secretary Lovett to cable Stuart from Washington to knock it off. Lovett summarized a series of past Stuart communications that wittingly or not revealed KMT chaos, corruption, and desperation. Referring especially to the ambassador's recommendation to go "all-out," Lovett said: "In light foregoing appraisal, recommendations . . . seem inconsistent . . . and [fly] in face all previous experience US advisors in China."

Stuart's nutty moralizings and unreliability,

the somewhat stunned reportage from the northern consulates, and the bitter realism of Consul General Cabot in Shanghai ("The same old inefficient grafters are still occupying the same old positions") must be set in the wider context of Truman and Marshall who continued to enunciate a policy unrelated to circumstances. In his March 11 press conference, included in this volume, while the walls were falling around the ears of his legates in Manchuria, the president declared: "We did not want any Communists in the government of China or anywhere else if we could help it." On November 29, when the flight to Taiwan was already being hinted in dispatches, Marshall informed Stuart that the "Present National Government [is] the only instrument now available which has any capability of bringing about an independent stable China friendly towards the US. . . . A coalition government including the Communists would likewise be unsuitable as an instrument for achieving American objectives in China." (Later in this directive Marshall added "we have endeavored to impress upon the Chinese the advantage which we consider inherent in the American system of private enterprise.")

Along the edges of the main stage appear little comic turns: a Tibetan trade mission purveying yak tails shows up in Washington seeking an audience with President Truman. Cables flash back and forth between Nanking and Washington as the Chinese insist that Tibet, a mere particle of the Republic of China, "has no authority to conduct diplomatic negotiations with foreign governments." The puzzled Tibetans end up visiting with Secretary Marshall, leaving their white scarf and other momentos from Lhasa with Truman's chief of protocol.

Consul General Ward takes the new Communist mayor of Mukden on a tour of the USIS. The latter offers some "Commie" literature for the library. Ward, not in a good position to take a high hand, advises his chiefs that a "minority display" cannot hurt. Nanking and Washington say no. Shortly before the sentries arrive Ward cables back: "I feel that when formulating policy on USIS activity in China we should bear constantly in mind that with coming of [the] Communists the days of old China with foreign establishments enjoying

special privilege have in large measure passed."

JONATHAN MIRSKY  
Dartmouth College

WALTER JOHNSON, editor. CAROL EVANS, assistant editor. *The Papers of Adlai E. Stevenson*. Volume 3, *Governor of Illinois, 1949-1953*. Boston: Little, Brown and Company. 1973. Pp. xiii, 621. \$15.00.

The third volume of *The Papers of Adlai E. Stevenson* covers his period as governor of Illinois. In the losing battle that fact fights with history, these four years of our hero's career—the only years in which he held elected office—are always going to have to struggle for recognition. The years in the gingerbread mansion in Springfield do not follow neatly on the New Deal and United Nations curtain-raisers, nor do they prepare us for the more than national impact of the 1952 campaign and the emergence of the world-resonant voice of American liberalism. There is a disposition to see them as a necessary interlude—a sort of sop to the democratic Cerberus before the born leader can enter upon his real role.

Volume 3 exposes the falsity of such a view. It is a carefully documented record of a phase that Stevenson himself regarded as genuinely important in itself and not merely as an apprenticeship or a probation. Although the letters contain a fair amount of grunting at the fardels to be borne, they are also quite explicit about the enjoyment in the fight and the pleasure in the achievements that the governorship alone makes possible. Although he would not relax his own style and standards—"I simply can't use the conventional banalities of politicians, even if I should" (p. 306)—he did not duck any of the grind—"one day last week I spoke eight times between 10 a.m. and midnight in Chicago and suburbs" (p. 314). The real meat of the volume is the day-to-day stuff of state government: patronage problems, gasoline taxes, law enforcement, handshaking, and budget-paring. The sauce to this rather tough steak is provided by Stevenson's own infinite capacity for endowing every encounter with a touch of his own grace and humor. These *Papers*, like all such collections, carry their heavy quota of obligatory "bread and butter" and courtesy letters, but there can be few official correspondents who have imparted a comparable degree of felicity

and gaiety to this small change of public life.

Yet much of the gaiety is gloss. The subplot to this volume is the story of Stevenson's broken marriage, and the dateline for many of these items is "my lonely room in the deserted Mansion" (p. 258). Stevenson the man competes for interest in these pages with Stevenson the governor.

As in volumes 1 and 2 the quality of the editing is as high as could be wished, accurate in detail, well judged and informative—what, in another context in this volume, the governor himself endearingly describes as a *succes faux* (p. 418).

H. G. NICHOLAS  
New College,  
Oxford

JAMES T. PATTERSON. *Mr. Republican: A Biography of Robert A. Taft*. Boston: Houghton Mifflin Company. 1972. Pp. xvi, 749. \$12.50.

Americans have traditionally undervalued, even demeaned, political skills in their chosen leaders, revering most those individuals in public life who have seemed somehow above or apart from the world of partisan politics. Yet in the final analysis the stability of the American system has rested on the services of politically aware and adept officials who have excelled at their jobs. Such a man was Robert A. Taft, who rose to such prominence in his party and in the United States Senate that he earned the sobriquets "Mr. Republican" and "Mr. Congress," respectively.

James T. Patterson portrays Taft as an intelligent and purposeful leader, committed to principles more consistent than heretofore recognized. Taft's libertarian philosophy was based on the principles of equal justice under law, equality of opportunity, and human liberty. In foreign affairs he was motivated by faith in the example of American liberty before the world and by a consistent desire to limit United States commitments overseas. By demonstrating the consistency of these ideals Patterson has succeeded in bringing intellectual order to Taft's actions and beliefs. Yet, as the author admits, the senator was frequently "flexible." Clearly this flexibility often reflected partisan necessity of the moment, responsiveness to his urban-industrial Ohio constituency, and/or essential

maneuvering for his next quadrennial run for the presidency. Though Patterson avoids saying so, his own impressive evidence presents a strong case for Taft as a skillful politician. To admit this would detract nothing from Taft's historical reputation; that Patterson does not do so reflects his adherence to the common American tendency to view excellence and political skill as mutually exclusive.

There are few weaknesses, however, in this impressive study. If Patterson tends occasionally to take subtle and gratuitous swipes at certain of Taft's rivals (notably John W. Bricker, Thomas E. Dewey, and Dwight Eisenhower), the author explains exceptionally well Republican factionalism in the 1940s and 1950s. His analyses of the 1948 and 1952 conventions, particularly, suggest the interesting thesis that personal and geographical rivalries had at least as much to do with such factionalism as did ideological difference. Unfailingly attentive to the historical context and significance of his subject, Patterson describes Taft's life with intelligence, sensitivity, and grace. If the dual objectives of political biography are to evoke and explain the man and his times, then *Mr. Republican* is a model of its genre.

GARY W. REICHARD  
Ohio State University

ATHAN THEOHARIS. *Seeds of Repression: Harry S. Truman and the Origins of McCarthyism*. Chicago: Quadrangle Books. 1971. Pp. xi, 238. \$6.95.

How informative are book-jacket blurbs? The front flap of *Seeds of Repression* begins, "In this provocative new interpretation of the McCarthy years in America, Athan Theoharis lays the major responsibility for the Senator's rise at the doorstep of the White House during the Truman administration. He convincingly argues that President Truman helped create a political climate which made possible the McCarthy phenomenon in the early 1950's." The back flap predicts that the book "is certain to revise historical thinking and raise new questions about Harry S. Truman's accomplishments as President of the United States." *Caveat legens*: The book is provoking rather than provocative, unconvincing in its simplistic and poorly documented circumstantial case against Truman, and unlikely by itself to cause much revision of historical thinking.

First, McCarthy and McCarthyism are not the centerpieces of the book, and the reader ought not expect to learn very much about Senator McCarthy as a political personality or McCarthyism as a political force in American politics. They simply provide the author with symbols and a time frame to lay responsibility for the rise and intensification of the cold war on Truman. The assertion that Truman and his associates, by igniting and politically feeding upon fears of Soviet Communist expansion abroad and subversion in the United States, seeded a national climate of fear and suspicion which gave McCarthy his issue and made McCarthyism politically profitable is only instrumental to the primary theme that Truman broke with FDR's policy of diplomacy and detente with the Soviet Union and replaced it with an aggressive rhetoric but more moderate policy of saber-rattling abroad and witch-hunting at home.

Second, the book is unidimensional in its analysis. Theoharis sees American motives, actors, and actions, and he sees American images of Soviet motives, actors, and actions. But he does not incorporate their Soviet counterparts into his analysis of cause (origin) or assignment of blame. One need not be Hardhat Joe to recognize that the Soviet Union was more than a "bogie" manufactured by Truman and McCarthy; that there was a reality of Soviet motives (ideology, interests, strategy), actors (remember Stalin?), and actions; and that these have a bearing on sound historical interpretation of the era. The book would have been better informed and balanced, and more convincing, had Theoharis examined studies of Soviet domestic politics and foreign policy in this period, sought to capture the multifaceted dimensions of the decade's domestic and international politics, and provided the evidence and pauses that permit and invite the reader to make an independent judgment.

Third, Theoharis neither understands nor likes politics. The book is not informed or integrated by a coherent or realistic concept of politics. He accuses Truman of "oversimplified moralism" in his cold-war postures. But the author himself portrays the dramas of politics as morality plays and not complex scenarios of crisscrossing and conflicting currents of interests and ambitions in which mar-

ginal gains often are the results of massive efforts and skills of accommodation. Theoharis states, "A politician rather than a statesman, Truman was more attuned to short-term requirements than long-term results." Good grief! He contrasts FDR's "sophisticated" approach of negotiation and detente with the Soviet Union to Truman's reliance on "the primacy of power in international politics." Perhaps Truman, in the context of his presidency, was also "sophisticated." Arthur Schlesinger, jr.'s recent recollection in *Atlantic Monthly* of a Henry Wallace quip about FDR would apply equally well to Truman: He "could keep all the balls in the air without losing his own."

Fourth, Theoharis explains the absence in the text of source citations and the interpretations of other scholars by his hope to reach a "nonacademic audience." If his aim is to persuade the nonacademic reader that Truman was the principal creator of an unnecessary as well as cruel cold war (and McCarthyism), with its confrontations abroad and Red hunts at home, then ironically its impact will be just the opposite. The nonacademic reader will conclude from *Seeds of Repression* that Truman was not tough enough with Stalin abroad and his "agents" in Washington, and that McCarthy was right, after all. Theoharis, by presenting a one-dimensional and therefore exaggerated argument, presses his reader full circle in terms of its final verdict.

Finally, the sentence on Harry S. Truman, as it relates to the cold war and its internal security and foreign-policy components, and to his stature as president, which no doubt will be mixed, will, one hopes, necessarily rest upon more thorough, penetrating, and balanced scholarship.

HERBERT WALTZER  
Miami University

CONSTANCE MCLAUGHLIN GREEN and MILTON LOMASK. *Vanguard: A History*. (The NASA Historical Series.) Washington: Scientific and Technical Information Division, Office of Technology Utilization, National Aeronautics and Space Administration. 1970. Pp. xvi, 308. Paper \$3.00.

CONSTANCE MCLAUGHLIN GREEN and MILTON LOMASK. *Vanguard: A History*. Washington: Smithsonian Institution Press. 1971. Pp. xvi, 308. Cloth \$12.50.

This book takes its title from the name of that first entry by the United States in the space age rivalry. It is the history of the program that developed and launched the Vanguard sequence.

For the many persons who will have forgotten these events, let it be said again that rockets first commanded attention as the awesome weapon in the hands of the German army. Postwar developments in Russia and the United States built on this German foundation, as both nations strained to perfect the intercontinental missile. Space exploration was not rated as significant in this weapons race.

There were individuals in the military services and, of course, in the universities and scientific societies who were both interested and active. The creation of the National Science Foundation in 1950, financed with federal funds, and the acceptance of the International Geophysical Year (IGY) in 1952 allowed the development of plans for space exploration with purposes beyond the military. Not until 1955 was further funding available.

Despite the insistence that IGY was not secret and was scientific only, the undertone of rivalry with Russia ran through this program. Representatives of that country had announced Russia's entrance into a satellite program just five days after the American announcement. Americans had been told about, though few had noted, Russia's activity. Americans were not prepared for Sputnik.

The sight of that tumbling object, launched on October 4, 1957, produced close to panic. The press was hysterical, congressmen demanded action, and in the excitement the achievements of the American scientists were ignored. The first result was the breaking down of restrictions, which to date had limited military participation, to allow a joint crash program, and by midsummer the United States had not one but two satellites. More important was the enactment, in July 1958, of legislation providing for planned, continuing activity with national decision-making authority concentrated in NASA.

This volume, which gives essential background, is basically a history of the years 1955-58 and is itself a critically important background for studying NASA. The technical problems that the scientists faced were com-

plex; they involved agreement on the program's purposes, instrumentation, vehicle, launching site, and tracking system, with decisions interrelated. The effort of the authors to translate all this into the language of the layman was quite successful. The efforts to untangle the alphabetical agencies, bureaus, and committees and to describe the competitive bureaucratic ways in and around government were probably as difficult. The thoughtful student can learn much here on how government functions.

The first author, Mrs. Green, is the distinguished historian of Washington, D.C., and of several of its governmental institutions. Mr. Lomask is a writer and teacher in Washington whose special contribution to this volume is those sections dealing with field tests and the Cape. Charles A. Lindberg has contributed an informed and thoughtful foreword, reflecting in part his participation in some of these councils and committees. Dated in 1969, his addition still carries the touch of skepticism about press and politics that must have been widely shared by scientists in the fifties.

The Smithsonian Institution Press has given the volume an attractive dress, with generous and well-chosen illustrations and charts.

RAYMOND C. MILLER  
Wayne State University

JOSEPH C. GOULDEN. *Meany*. New York: Atheneum. 1972. Pp. 504. \$12.95.

This biography of George Meany is a superior piece of journalistic history. Produced at breakneck speed—two years from conception to publication for a meaty 500-page book must be some kind of record—Joseph C. Goulden's account inevitably suffers from the defects of the genre. The background is established from little more than textbook knowledge; the evidence is not carefully sifted and weighed (T. Harry Williams's *Huey Long*, which likewise depends heavily on oral sources, makes an instructive comparison here); and there is not much reflection on the underlying issues. But the virtues far outweigh the defects in this case, partly because of Goulden's skill and energy, even more so because of the nature of his subject. It would probably be impossible to write a first biography of George Meany in any other

way. The AFL-CIO president has (as Goulden observes) "assiduously avoided paper all his life" (p. 469). Even had the written record been fuller, this would not have adequately revealed the sources of Meany's stature within the labor movement nor his potency in national politics. No one but a working journalist could have developed Goulden's sure grasp of the realities of power and influence, the limits as well as the open ground, that explain Meany's role in American labor since World War II. Goulden was indefatigable in interviewing Meany, normally an inaccessible figure, and a long list of other union officers and staff men, many of whom demanded anonymity. To a large extent, in fact, Goulden's biography has the character of an oral history, but several cuts above the average, because Goulden was able to elicit pointed, detailed information usually lacking in the memoirs in oral history archives.

Goulden has given us a shrewd and informative biography of George Meany. It advances our present understanding of the modern labor movement that stems from the old AFL—its political evolution beyond Gompers's nonpartisanship (with Meany as Exhibit A), its cold-war mentality and consequent adventures (financed mostly by the U.S. government) into international politics, and the broadening out from tight craft unionism (from which Meany sprang) to accommodate to the demands of industrial unionism and New Deal social policy. The book will have a second career: it will provide labor historians with a treasure of oral history that otherwise probably never would have been committed to paper.

There is one cause for regret: the book ends in 1971 and so deprives us of an inside history of Meany's part in the 1972 election.

DAVID BRODY  
University of California,  
Davis

LOUIS M. KOHLMEIER, JR. *"God Save This Honorable Court!"* New York: Charles Scribner's Sons. 1972. Pp. x, 309. \$8.95.

This volume illustrates an old saw that most books should be articles. The core of the study is a perceptive account of the struggle to control appointments to the Supreme Court during the Johnson and Nixon administrations. But

the author, a reporter for the *Wall Street Journal*, wastes almost one-third of the space in unsuccessful efforts to place these events in historical context and the rhetoric of crisis. The result is an uneven work whose quality improves the shorter the historical sight.

The chapters devoted to setting the stage are too flat and oversimplified to serve as useful popular history. Despite an occasionally dramatic detail, the author misses the ebb and flow of judicial power in American history and the complex roles that judges play in the modern polity. Assuming racial equality to be the only issue that counts, he not only underestimates such tragic figures as Roger B. Taney and Abe Fortas, but the author overestimates the "Supreme Court crisis" provoked by President Nixon's Southern strategy. Nowhere is it demonstrated that Nixon appointees have forsaken minority rights or their own independence to the crisis point. Nor are facts faced that the Warren Court was losing public support and that court-packing is a classic method of harmonizing federal courts with popular tolerances.

It is fortunate that the author ignores his backdrop when recounting recent conflicts over Supreme Court succession. Though the coverage of the Fortas, Haynsworth, and Carswell nominations overlaps other popular accounts, Kohlmeier's treatment of the Carswell episode is fairer than Richard Harris's *Decision* (1971). The best chapter covers the appointments of Justices Powell and Rehnquist after the American Bar Association Standing Committee on the Federal Judiciary opposed Richard H. Poff, Herschel H. Friday, and Mildred L. Lillie for want of professional distinction. Kohlmeier argues effectively that these three candidates were neither decoys nor coin to demean the Supreme Court, as some observers feared, but rather the entrapment of Mr. Nixon in his own political criteria. Certainly the same criteria made G. Harrold Carswell's selection predictable.

A contemporary chronicle is rarely final. Though flawed by oversimplification and needless padding, this account of the Johnson and Nixon nominations to the Supreme Court illuminates the role of the American Bar Association in the politics of judicial selection and long-standing tensions among political and

professional standards for service on the nation's highest court.

J. WOODFORD HOWARD, JR.  
*Johns Hopkins University*

## CANADA

HAROLD MARTIN TROPER. *Only Farmers Need Apply: Official Canadian Government Encouragement of Immigration from the United States, 1896-1911*. Toronto: Griffin House. 1972. Pp. 192. \$8.95.

Immigration to Canada in the first decade of the twentieth century is generally thought of in terms of the wave of Southern and Eastern European peasants who literally flooded the vast reaches of the Canadian west. *Only Farmers Need Apply* deals with a lesser known but equally important immigration, that of over half a million American citizens and residents who crossed the border in search of cheap and fertile land. The subject has been examined before, notably by Paul Sharp and Karel Bicha; Harold Troper's contribution is a study of the organizational structure of the federal government's immigration branch, which was assigned the task of encouraging American farmers to come to Canada. Thoroughly researched and generally quite readable, this monograph unfortunately suffers from a lack of rigorous editing and is still too obviously a dissertation. Several chapters, for example, those describing the activities of government agents in various American communities, are needlessly detailed and include a good deal of relatively insignificant material. More effective are those in which the content of the publicity and the increasingly restrictive policies of the immigration branch are outlined. Troper shows how the branch's campaign was consciously designed to fit into the tradition of American land promotion. Americans were expected to understand that the prevailing image of western Canada as a frozen wasteland was because of the kind of misrepresentation the American West had suffered when it was known as the "Great American Desert." At a time when the supply of cheap land in the United States appeared to be running out, the Canadian west was pictured as the "Last Best West," really a continuation of the mov-

ing American frontier. Yet it was advertised as a continuation with a difference. North of the border was an orderly and peaceful society, by implication a sharp contrast to the supposedly lawless Western American society.

As Troper demonstrates, however, the border was not considered open to all Americans. Canada was looking for farmers, and, in particular, white farmers. No others' need apply. The myth that racial discrimination existed only south of the border has recently been challenged by Robin Winks in his history of the blacks in Canada, and Troper supplies additional evidence by describing how westerners pressured the immigration branch to close the border to Negroes. As a result the large-scale immigration from the United States during this period apparently included very few blacks.

Although the scope of this study is purposely limited to the function of a government agency, Troper reminds us of questions that remain concerning the nature and magnitude of this immigration. Perhaps the most important of these is the significance of this immigration in the shaping of western Canada.

GILBERT A. STELTER  
*University of Guelph*

## LATIN AMERICA

FRIEDRICH KATZ. *The Ancient American Civilizations*. Translated by K. M. LOIS SIMPSON. (History of Civilization.) New York: Praeger Publishers. 1972. Pp. xvi, 386. \$15.00.

The title of this book may mislead the average reader. Professor Katz has written an account of the Indian high cultures of the central Andes and Meso-America from their origins to the coming of the Europeans. The organization of the book reflects the dual areal base, for after a unified consideration of more general questions—such as sources for studying the pre-Conquest cultures, the origins of the American Indians, the much-vexed question of autochthonous invention of cultural traits, as against reception through diffusion—Professor Katz presents his analysis in parallel chapters. The major divisions of time for the two areas are essentially the same: formative or preclassical, classical, postclassical or urban or militaristic.



In both the central Andes and Meso-America, two remarkable Indian structures emerged in the fifteenth century, to have their courses truncated by the Spaniards in the sixteenth.

The entire book is the product of judicious reflection by an able mind fully abreast of a widely dispersed literature. Katz reviews, perhaps at too great length, old questions such as the origins of the American Indians and agriculture among them, but his basic interest is in the role of technology, religion, militarism, and similar clusters of traits in promoting change and in the nature and direction of the change. The function of irrigation in providing both social surplus for the support of upper classes and creating the need for a strong state apparatus (the Wittfogel thesis) is found especially true for the desert coast of the central Andes. In Meso-America, irrigation was important to a far lesser extent; the unique chinampa provided much of the production for urban life. The Maya with their advanced intellectual achievements resting upon an apparent exiguous base remain a puzzle. The framework for understanding that Katz brings to phenomena serves here only to emphasize how fragmentary and contradictory is our present knowledge.

Perhaps most of all Katz is interested in the movement toward larger, more complex social and political structures in possible parallel development to the Old World. The most original part of his book lies in his examination of the Triple Alliance and the Inca Empire in such terms. The Aztecs, he points out, ruled over a population held to heavy tribute but with little change in their socio-political structures; the Incas integrated subject peoples into the fabric of an essentially unified state. Yet Meso-America was in rapid change, and the question remains what would have emerged had not the Europeans annexed it with catastrophic rapidity to the Iron Age.

WOODROW EORAH  
University of California,  
Berkeley

BILLY JAYNES CHANDLER. *The Feitosas and the Sertão dos Inhamuns: The History of a Family and a Community in Northeast Brazil, 1700-1930*. (Latin American Monographs, Second

Series, number 10. Center for Latin American Studies, University of Florida.) Gainesville: University of Florida Press. 1972. Pp. xi, 178. \$7.50.

The perennial question as to whether New York City fairly represents the whole United States has its counterpart in the argument that no one knows the real Brazil if his experience is limited to the *café* and beaches. Mr. Chandler agrees, and *The Feitosas* is a useful corrective. This book also partially satisfies the lament of the brilliant historian J. H. Plumb, who writes that "historians have not studied family life very closely." But Chandler's work satisfies only in part, for it is totally unlike our family counseling books, and his "family" is more like the Medici or the Du Ponts or a Mafia family than the ever-provocative Loud family. The Feitosas do not all bear that last name, and the *parentela* includes clients and loyal employees, who all take part in the recurring *lutas* between power groups. They stand for whatever law there is in a remote and lawless region until, from time to time, the central government can infuse national standards or a rival family wins its way to the top. The government is forced to rely largely on the local VIP's and their autocratic power; outside authority is, or rather was, seldom exercised.

Chandler's monograph makes full use of Brazilian documents and historical writing—with the startling exception of Da Cunha—rather than the innovative techniques of the late Oscar Lewis.

This "narrative local history" begins with the rigorous natural characteristics of this part of Ceará and the original Portuguese land grants. It does not attempt to do much with national history, but it covers the efforts made to eliminate the Indian threat and the problem created by Negro slavery. This is part of the Brazilian reality, and Chandler does not claim that it is typical even of all isolated and poverty-stricken areas down to 1930.

The law of the books was less important than the unwritten law and private hate and vengeance. We, too, have examples of the nullification of law by the jury. Schools seldom functioned and the Church was insufficiently staffed. The droughts, or *secas*, occurring at intervals of less than ten years, caused the

death of many animals and the departure, temporary or permanent, of the *retirantes* (withdrawers), sometimes as much as ninety per cent of the population, and the destruction of a viable economy. In spite of all this, the author concludes with a recognition of the value of a society that is much more traditional than ours and less addicted to change. His treatment of this society is vastly informative and judicious.

W. REX CRAWFORD  
University of Pennsylvania

EDGAR GABALDÓN MÁRQUEZ. *El México virreinal y la "Sublevación" de Caracas, 1810*. (Biblioteca Venezolana de Historia, 13.) [Caracas:] Archivo General de la Nación. 1971. Pp. 576.

News of the discovery and publication of documents bearing upon an important historical event (as contained in the prologue of this work) is exciting. And, indeed, Gabaldón Márquez traveled to Mexico to study and photocopy an unedited file of documents consisting of 308 folios, which describe measures taken by the viceregal government of New Spain in reaction to the Caracas "rebellion" of April 10, 1810. The measures were secret because the authorities of New Spain wished to conceal the events in Venezuela from the Mexican population in order to prevent the "contagion" from spreading. The viceroyalty instructed officials under its jurisdiction to censor news and mail and to impose an embargo against the infected province, and it appealed to English authorities in Jamaica to take similar steps in order to help contain the revolt. The prophylactic measures that the viceregal government took were vigorous. However, Gabaldón Márquez does not present these facts in an orderly fashion, and one needs a great deal of patience in order to extract the information.

The book is essentially a philosophical-historical essay in which the author muses upon numerous thoughts that the documents and events provoke in his mind, the principal ones being secrecy in government ("Secrecy is a reflection of fear—fear on the part of a small group that it will lose advantages acquired at the expense of mankind" [p. 81]) and imperialism ("Is there anyone today who thinks

that Moby Dick . . . is a symbol of evil; no, absolutely not. . . . Moby Dick represents human liberty, pursued by the Ahabs, the whale-hunters, the hard-hearted profit-seekers, the island-hunters, the exploiters of other men, and the mortal enemies of the splendid whale-creatures" [p. 461]).

The author is erudite, wide-ranging, and amusing, but he also rambles and makes parenthetical statements within parenthetical statements. Like Melville, whom he admires, he is easily diverted. When he discusses the arrival of the news in Vera Cruz of the uprising in Caracas, he feels compelled to describe conditions in general in New Spain and to survey the prevailing knowledge there about Venezuela. When he treats the efforts of the viceroyalty of New Spain to suppress the news of the Caracas revolt, he adds a chapter about measures taken earlier in reaction to the Haitian revolt. And when he describes the appeals to the English in Jamaica, he writes at length about the English presence in the Caribbean and is carried away by his Anglophobia. This is legitimate, of course, but the work fails to fulfill the promise of the prologue and seems inappropriate for a publication of the Archivo General de la Nación. In short, the work is too eclectic for its own good.

CHARLES D. AMERINGER  
Pennsylvania State University

WILLIAM F. SATER. *The Heroic Image in Chile: Arturo Prat, Secular Saint*. Berkeley and Los Angeles: University of California Press. 1973. Pp. viii, 243. \$10.50.

Early in the War of the Pacific, Chile lost one of her decrepit warships, which was then under the command of Arturo Prat. In the clash Prat not only lost his ship but his life as well when he, along with only one other sailor, leaped upon the deck of the enemy vessel in a futile gesture to inflict some damage upon the Peruvians before the battle was lost. Prat became a hero to the Chileans and remains to this day one of his country's foremost historic figures. By examining attitudes toward Prat expressed in print over the subsequent seventy years the author of this book seeks to explain how and why a defeated commander becomes a great hero to his countrymen.

Prat's image shifted with events in Chile. During the remainder of the war he was used either as an antigovernment symbol, when some blamed military failures on political leaders, or as a means to strengthen morale when the population despaired of victory. Once the war ended successfully for Chile, the press and political and intellectual leaders displayed a lack of interest in the fallen hero. But when political problems developed and a civil war ravaged the nation in 1891, Prat was resurrected in the press. Following the civil war the new government was accused of corruption, and Prat was now used to emphasize virtue and morality. Later he became the inspiration for patriotism and nationalism and, as problems divided the nation, the symbol for national unity. At times political parties also made use of the naval hero. In the 1930s the Nazi party praised him and the socialists used him to dramatize the failures of the government. The military recalled Prat in its publications as higher budgetary appropriations were sought. Meanwhile, educators tried to instill in their students the qualities of self-sacrifice, duty, and discipline that ennobled Prat. In all of this the hero was immortalized not for his military action but rather for the human

qualities he displayed in battle, qualities that the nation hoped to re-establish.

Basing his study largely on the newspaper and periodical treatment of Prat and the Battle of Iquique, Sater succeeded in linking Prat's fame to political and social developments in Chile. When there was need for an example to dramatize some failing in the nation, Prat was hauled out to be held up for emulation. The hero in this case became the vehicle for propaganda.

This study, unfortunately, makes little note of the press treatment accorded other heroes such as Bernardo O'Higgins. Such a comparison and analysis would have placed the Prat phenomenon in a larger and perhaps more meaningful context. Nevertheless, this is an excellent book. The thesis is set forth precisely and logically amid succinct sketches of Chilean history that place Prat in his role as hero in proper perspective. The book is, in addition, exceptionally well written. The author obviously devoted a considerable amount of effort to the literary aspect of his history with the result that literary quality and scholarship are united in a most pleasant fashion.

JACK RAY THOMAS

*Bowling Green State University*

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## Communications

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*A communication will be considered only if it relates to an article or review published in this journal; publication of such a communication or of any part of it is solely at the editors' discretion. Limitations of space require that a communication concerning a review be no longer than the review to which it refers and in no case longer than 500 words. Communications concerning articles or review articles may be no more than 1,000 words, and the editors reserve the right to impose a lower limit. The schedule of publication and the time needed to send a communication to the author of the article or review in question for such reply as he may care to make virtually preclude the possibility of publication in the issue following that in which the original article or review appeared. Unless, in the editors' judgment, some major scholarly purpose is served, rejoinders will not be published.*

### TO THE EDITOR:

The Halsted Press Division of John Wiley and Sons, Inc. would like to inform your readers that V. D. Nabokov's *The Provisional Government*, which was reviewed in the April 1974 *AHR* (79: 546-49), has been withdrawn from sale in the United States.

WARREN SULLIVAN  
President, Halsted Press

### TO THE EDITOR:

In his article "Naturalism and Socialism in Germany" (*AHR*, 79 [1974]: 14-37), Vernon L. Lidtke holds the rigidity of Marxist theory accountable for the failure of German nat-

uralists to affiliate with the Social Democratic party in Imperial Germany. While this may be a valid judgment in respect to Otto Brahm, founder of the *Freie Bühne*, it is hardly an accurate assessment in the case of writers such as Karl Bleibtreu, author of the manifesto *Revolution der Literatur* (1885) and spokesman for the "Youngest Germany," or Michael Georg Conrad, founder of the Munich-based journal *Die Gesellschaft*. Professor Lidtke underestimates the nationalistic and antimaterialistic bias of the early naturalists. These writers sought to generate a literature commensurate with the grandeur of the German Empire. In Zola they admired the iconoclast who had successfully defied an outworn literary tradition in France. Bleibtreu and Conrad embraced naturalism as a means of overcoming the blandness of popular literature and the formalism of an older generation of writers. Description of social reality offered a means to shock the philistine out of his complacent materialism and indifference to art.

The concern of these writers with the social question in no way meant sympathy with the labor movement. They did not pose the problem in terms of how to correct economic imbalances but rather in terms of how to reduce the bitterness of class relations. In their view class warfare resulted from the materialistic attitudes to which both worker and bourgeois subscribed. They implicitly rejected the labor movement as one of the instigators of social conflict. Only a new idealism—a revival of heroic spiritual values shared by all—could in their view provide an adequate solution to the social problem.

To have accommodated Bleibtreu, Conrad, and the "Youngest Germany," the S.P.D. would

have had to cease being socialist in all but name, a condition met by the "national socialist" parties both before and after the First World War.

RODERICK STACKELBERG  
University of Massachusetts,  
Amherst

PROFESSOR LIDTKE REPLIES:

Roderick Stackelberg grossly oversimplifies the argument in my article and compounds his unfortunate misunderstanding by simply disregarding the evidence presented to demonstrate that many naturalists held views antithetical to Social Democracy. Stackelberg raises the case of Michael Georg Conrad with the suggestion that the article underestimates his nationalism and antimaterialism. This is a strange error for Stackelberg to make for I specifically pointed out that Conrad was a nationalist and that he vigorously attacked the most fundamental principles of Social Democracy, including materialism and internationalism. I did not and would not recommend that the Social Democrats should have accommodated themselves to a man like Conrad by abandoning their Marxism and adopting his chauvinistic beliefs. That would have been absurd. The issue is not the rigidity of Marxism, as Stackelberg wrongly maintains, but Marxism's intellectual superiority over anything the naturalists could produce and the resulting confidence this theoretical supremacy gave to the Social Democrats.

Stackelberg falls into other errors of oversimplification. Preoccupied with Conrad and Bleibtreu, he generalizes as if they spoke for the whole of the naturalist movement. That is hardly the case, as he must certainly know. The naturalist movement had its own diversity. Moreover, it should be emphasized that the social views of many naturalists underwent changes and did not remain static as Stackelberg seems to assume. If one takes into account both the personal diversity and the developmental factor then it is easier to understand the complexities of the relationship between socialists and naturalists.

VERNON L. LIDTKE  
Johns Hopkins University

TO THE EDITOR:

I do not wish to quibble over the motives behind American intervention in Cuba. I stick to the viewpoint set forth in my work, *The Spanish-Cuban-American War and the Birth of American Imperialism, 1895-1902*, despite the criticism in the review by Kenneth J. Hagan (*AHR*, 79 [1974]: 240-41). But since Professor Hagan asserts that I did not pay attention to the desire of the United States to assist Cuba in achieving independence from Spain, I would like to point out that I devote an entire chapter in the first volume ("The American People and Cuban Independence") to demonstrate that the majority of the American people distinctly favored and supported Cuban independence and that this desire was utilized by the McKinley administration to frustrate that independence. In the second volume I demonstrate the opposition in the United States to the Platt Amendment on the grounds that it deprived Cuba of any meaningful independence. If Professor Hagan wishes to identify the "Americans" as represented solely by the McKinley administration and other imperialists in this country, that is his privilege.

Professor Hagan also asserts that "racism" pervades my "discussion of American anti-imperialists," leaving the impression that I attribute the anti-imperialist impulse solely to the fear of adding more nonwhite population to the American nation. This is not borne out by the evidence in the books. It would, moreover, be strange that, having published a laudatory analysis of Mark Twain's anti-imperialist writings (see *Mark Twain: Social Critic* [1958]), I would view the anti-imperialist movement so narrowly. But the fact is that racism was deeply imbedded in American anti-imperialism at the turn of the century, although some anti-imperialists who started out as racists learned from their anti-imperialist activities how dangerous this view was to the welfare of their own country. Perhaps Robert L. Bessner put it best in his *Twelve Against Empire: The Anti-Imperialists, 1898-1900* ([1968], pp. 232-33): "They [the anti-imperialists] can be condemned as racists. Whether they used words like 'superior' and 'inferior' or 'civilized' and 'uncivilized,' they thought of peoples in the categories of racism. George

Hoar was generally an exception, but even he gave himself over to this kind of thinking on occasion. The unhappy fact is that few Americans were immune to the prevailing racism of the late nineteenth century. The anti-imperialists were no better or worse than their countrymen."

PHILIP S. FONER  
Lincoln University

PROFESSOR HAGAN REPLIES:

As I indicated in my review, Professor Foner's book is an impressive piece of research that lucidly traces the struggle in Cuba. My major reservation about the two volumes is that they interpret annexationists as being motivated almost exclusively by desire for material gain. Professor Foner advanced a similar thesis in volume 2 of an earlier work, *A History of Cuba and Its Relations with the United States* (1963). He argues that during the 1880s "the pernicious influence of big business" was pervasive in American government. He believes that "scarcely a week passed without some public disclosure of advantages and lawless concessions [being] granted to corporations, passed into law by bribed legislators, signed by corrupt executives, and approved by judges who were subservient tools of the corporate interests." In this environment Secretary of State Blaine's proposal for reciprocal trade was not a "liberal trade policy." It was merely "a device for the United States to dump surplus products on the Latin American market, and dominate that market for North American economic interests."

Corporate leaders did have great influence upon the United States in the late nineteenth century, but American foreign policy was more than a conspiracy to gain wealth through increasing exports. A widely held contemporary opinion contended that unless the United States increased its exports there might well be a social upheaval brought on by angry workers suffering the effects of reduced income as a result of decreased demand for industrial goods. Many Americans who had no personal vested interest in industrial exports therefore wanted to increase the volume of those shipments. Ultimately they accepted the economic rationale for imperialism.

The tendency to oversimplify interpretations

is also apparent in *The Spanish-Cuban-American War* when Professor Foner dismisses an analysis of American policy makers' fear of German penetration of the Caribbean as superficial. Yet, as Richard Challener and others have recently shown, throughout the period from 1900 to 1914 American naval strategists considered Germany the most likely enemy and the Caribbean the most probable theater of operations in any war involving the United States and Europe.

Similar points might be made about the racial attitudes of the late nineteenth century. Professor Foner's citation of *Twelve Against Empire* is most pertinent. But the fact that anti-imperialists held racist assumptions does not diminish the validity of their fear for the constitutional integrity of their country as it abandoned precedent and embarked on an overseas imperial adventure.

It is my hope that this exchange will spark a heightened interest in Professor Foner's work and a testing by others of my criticism. I look forward to a great deal more discussion of *The Spanish-Cuban-American War*.

KENNETH J. HAGAN  
United States Naval Academy

TO THE EDITOR:

After criticizing a number of details, Gordon L. Davies, in his review of my book *Charles Lyell. The Years to 1841: The Revolution in Geology* (*AHR*, 78 [1973]: 1461-62), writes: "I have been forced to conclude that Wilson understands neither the true nature of Lyell's geological beliefs nor the scientific milieu within which he operated."

To support such a sweeping assertion Dr. Davies offers only the criticism that I praise Lyell for his work on the origin of river valleys in Auvergne in 1828. When Lyell and Roderick Murchison were in Auvergne in May 1828 they made observations which demonstrated conclusively that the valley of the Sioule River was excavated, not just once, but repeatedly, over a long period of time by the water flowing in the river itself. Their observations destroyed the contrary theory of the Reverend William Buckland that the river valleys of Auvergne had been formed on a single occasion by the waters of a great deluge which had swept the whole country. Inasmuch as Lyell held in 1828 that

the Auvergne River valleys were formed by the rivers flowing in them, his ideas were in agreement with the earlier theories of Hutton and Playfair. As Lyell's ideas developed he attributed influence also to the action of waves along lines of seacoast in creating land forms. On page 378 of my book I describe Lyell's view in 1833 of the origin of the land forms of the Weald and the Chalk Downs. Lyell's and Murchison's work on river valleys was, however, a minor part of their work in Auvergne. Lyell was much more concerned with the Tertiary freshwater formations of Auvergne, with the picture that their fossils gave of conditions in the lakes of Auvergne during the geological past, and with the Auvergne volcanoes.

Does Dr. Davies himself understand Lyell's geological beliefs? He criticizes my failure to mention in greater detail Lyell's observations in 1818 of the effects of a flash flood in the Val de Bagnes. I do mention (pp. 62-63) Lyell's observations of the vast amount of sand brought down by this flood and add that Lyell "would always be aware that the occasional recurrence of such violent events as floods was part of the regular order of nature and helped to produce geological change." Dr. Davies refers to the flood of the Dranse as a "catastrophic debacle." It was, however, not a catastrophic debacle in the sense that the word "catastrophe" was understood in the nineteenth century—that is, an event of a magnitude many times greater than any geological change occurring in the world today. Although the flood of the Dranse was disastrous to the inhabitants of the Val de Bagnes it was an example of geological forces at work.

Dr. Davies simply ignores my extensive discussion of the development of Lyell's geological thought and creates the impression that my book gives only a picture of Lyell's everyday life. The supercilious tone of Davies's review and his contemptuous dismissal of the work of another are not only unjustified but are also incompatible with civilized historical discourse.

LEONARD G. WILSON  
*University of Minnesota*

TO THE EDITOR:

Since my late collaborator, Professor Edwin H.

Zeydel, is no longer in a position to defend himself against Allen Cabaniss's superficial and unfair review of our translation of Einhard's *Vita Karoli Magni: The Life of Charlemagne* (AHR, 78 [1973]: 1035), I feel called upon to defend our work most vigorously. Some of Mr. Cabaniss's criticisms can be dismissed easily, for he has plainly chosen to ignore what is clearly printed in our book for everyone to see and read: thus, our bibliography is intentionally called a "Selected Bibliography" and for this reason lists "only thirty-six books and articles" chosen from a flood of historical scholarship on Einhard and Charlemagne. As we expressly state in our preface, moreover, we have chiefly included recent German scholarship on Einhard and Charlemagne. Our footnotes, similarly, can hardly be called "thin," since they comprise almost twenty pages and elucidate a text only a little more than twice as long (forty-five pages). Whatever Mr. Cabaniss's editorial theory may be, it was not our intention to bury Einhard in footnotes.

As to Mr. Cabaniss's observation that "several debatable statements occur in the introduction," I should like to know of any scholarly book of which the same cannot be said. As a historian he ought to know that history is composed of "debatable statements," and that the study of history dies when the debate ceases. If, however, by "debatable" Mr. Cabaniss means "inaccurate" then he should have the courtesy of proving his point. Of his two so-called illustrations, the first—referring to an "apparent conflation" of Louis the Pious and Louis the German—apparently refers to the statement that "under Louis the Pious, it [the Imperial title] merged with a concept of Frankish unity, the sovereignty over all other hereditary Frankish rulers. But Louis the German was an opponent of the imperial office, so Einhard, looking to the future, perforce bowed to his feelings" (pp. 24-25). If this is indeed the passage to which Mr. Cabaniss refers, then the conflation must exist in Mr. Cabaniss's mind alone, since the two Louis's are clearly distinguished from each other. As to his second point, that we are wrong to claim that "in the eighth and ninth centuries marriages did not involve the Church," this is one of those debatable points on which any historical interpretation must be based. What Mr. Cabaniss should note in his review is that his

suggestion that marriages at this time did involve the Church is just as debatable. Furthermore, in the context it is clear that what is being presented here is a summary of historical scholarship and not an original interpretation on the part of the translators.

I object strongly to this shoddy treatment of a carefully thought-out introduction. But most strongly I object to Mr. Cabaniss's off-hand remarks about the quality of our translation. He does not seem to be aware of the difference between a translation and a trot. For readers such as himself we have provided the Latin text which can be glossed *ad libitum*. (My German translation, by the way, has received an excellent critical press and has been reprinted several times by Reclam.) It is easy enough to find deviations of a word-for-word kind in any good translation, and, indeed, word-for-word accuracy can be the most unfaithful way of bringing a literary text from one language into another. I do not wish to discourse here at length on the theory of translation, but it is my hope that this letter will provide me and my late collaborator—as well as innumerable translators who have been maltreated by reviewers as superficial as Mr. Cabaniss—with at least some, if still inadequate, redress for the damage done to us.

EVELYN SCHERABON FIRCHOW  
University of Minnesota

#### TO THE EDITOR:

One stands aghast at the irresponsibility of Bernard S. Bachrach's review of my book, *A Jewish Principedom in Feudal France 768–900* (*AHR*, 78 [1973]: 1440–41).

Bachrach offers a totally inaccurate summary of my method. I based my conclusions on all available and relevant eighth- and ninth-century records, not on "a highly selective use of *chansons* and other literary materials from the twelfth century and later."

For example, the epistle of Pope Stephen III of 768, responding "anguished to the point of death" to information supplied by Bishop Aribert of Narbonne, is not a later "fictional literary source(s)," in Bachrach's characterization. The bishop advised the pope that the Frankish kings had granted extensive territories in free allod to the Jews, in fulfillment of a

pledge. Stephen declares the kings can nullify their pledge since God Himself withdrew His promises to the Jews in consequence of the Crucifixion. What pledge? I draw the answer from ninth-century documents and later sources, which, I show, are based on Carolingian materials.

Thus, the thirteenth century *Gesta Karoli Magni* describes the Jews' surrender of Narbonne in return for a pledge of the Frankish king for a ruler of their own. Parts of the *Gesta* are based on older historical materials. I show that Pope Stephen's epistle complements and completes the *Gesta*. To Bachrach this means basing conclusions on "fictional literary sources."

I locate Carolingian fragments in the twelfth-century *Addendum* to the Hebrew *Sefer haQabbalah*. The writer describes the seal and briefly summarizes the contents of a capitulary issued by a "King Carolus" to the Jews of Narbonne, to be dated 791. This *Addendum* also reports a royal cession of hereditaments to Narbonne Jewry. It discloses the name of the scholar-prince, a scion of King David and former exilarch in Baghdad, who arrived in Frankia by royal invitation and established a dynasty of Jewish princes.

Bachrach charges that I "abandoned the canons of historical method." In fact, I demand that such canons be applied to the documents of the Carolingian age, especially when they derive from ecclesiastical sources and involve Jews. The ninth-century Goth monk who compiled the *Annals of Aniane* attributed to Goth residents of Narbonne its surrender to the Franks for a pledge to have their own law. Seventy-five years ago Pückert warned that this monk falsified fact with astounding audacity. I emphasize the incongruities in this text and the contradiction with historical reality, since Pepin banished the Goths on acquiring Narbonne but granted vast allodial lands to the Jews. To Bachrach such a method is "to accept as superior the evidential value of fictional literary sources . . . and to relegate documentary evidence of a more contemporary nature to inferior status."

I have searched for authentic documentation regarding Jews in numerous contemporary sources as, for example, the ninth-century *Poème* of Ermold. I demonstrate that Ermold's chronology for the siege of Barcelona is based on



the Jewish calendar. This successful assault involved Jewish forces led by their own prince.

ARTHUR J. ZUCKERMAN  
City College,  
City University of New York

PROFESSOR BACHRACH REPLIES:

While no one would question Rabbi Zuckerman's right to stand "aghast," the question of "irresponsibility" will remain for our readers to decide. Zuckerman is not the first nor will he be the last partisan of Jewish history to be misled by papal hyperbole or to be seduced by the theory of Christian conspiracy. The "Goth monk" and those other "ecclesiastical sources" that sought to deprive Saint William of his Davidic heritage have not fooled Zuckerman, because in the later Middle Ages various fictional sources relate the truth. William and his family had long noses, William ate no pork, and had several wives. Perhaps some learned Cadi in one of our emerging ethnic programs will soon honor us with a monograph entitled "An Arab Kingdom in the Carolingian Empire."

BERNARD S. BACHRACH  
University of Minnesota

*The following letters have been received in connection with the publication of John A. Garraty's "The New Deal, National Socialism, and the Great Depression," AHR, 78 (1973): 907-44.*

TO THE EDITOR:

I would like to claim some space to comment briefly on John Garraty's recent article on the Nazi and New Deal response to the depression of the 1930s. While I suspect that others will want to comment on what I consider some of the strained comparisons he makes between Germany and the United States, this is not the reason I take issue with the article. Like Professor Garraty I have no objection to anyone's recognizing good in the Nazis, so it is not from his revisionist attitude that my objections arise. Rather it is his deliberate use of the so-called comparative method that seems to call for comment. An examination of Garraty's article

can reveal quite clearly, I think, the limitations as well as the advantages of cross-national comparisons.

Professor Garraty's article makes evident that it is when differences are uncovered that comparison makes its principal contribution to our understanding or analysis of the past. It is true that a cross-national comparison which reveals similarities increases our understanding. Usually what we learn is that a given phenomenon had similar results in two or more different societies, suggesting further that the two societies are not as different as we had hitherto been led to believe. Thus the uncovering of similarities can correct erroneous assumptions about the differences between two countries or societies. But in this case Professor Garraty has been quite specific in denying that he is showing that the New Deal was more fascistic than we thought or that the Nazi regime was more democratic. In fact, at the beginning of his article he states his purpose as an attempt to "demonstrate that Nazi and New Deal antidepression policies displayed striking similarities. Since the two systems, seen in their totality, were fundamentally different, these similarities tell us a great deal about the depression and the way people reacted to it." The proposition, I believe, is a non sequitur, for if the two societies are "fundamentally different" what do the similarities reveal to us that we could not have learned without a comparison between Germany and the United States? The evidence which Professor Garraty adduces, for example, to show that the Nazis had a concern for the unemployed or that they used propaganda to advance their antidepression policies did not require a comparison with the New Deal. Nor was a comparison of Germany and the United States necessary in order to show that Hitler and Roosevelt were charismatic leaders.

My point, however, is not that Garraty's article is without contribution. The point rather is that his contribution to our understanding comes only when he moves away from a comparison between Germany and the United States and looks, however briefly, at the depression in England and France. Then the German and American comparison spring into life, telling us "a great deal about the depression and the way people reacted to it." For now we learn that not all societies reacted to the depression

in the same way, and that those which differed may have been of the same political order. Thus political democracies like England and France had a quite different response, as Professor Garraty shows, from that in another political democracy, the United States. In sum, the comparison with Germany did not tell us anything about the depression in the United States and the reaction to it that we could not have learned better from a comparison with England or France. About all, on a conceptual level, the German comparison seems to add to our understanding is that the differences between the response of the New Deal and the French Popular Front, for example, could not be ascribed to the differences between a New World and a European country.

My object here, however, is not to suggest the various hypotheses one might fruitfully examine, on the basis of Professor Garraty's comparative matrix of four countries, but rather simply to point out that differences, not similarities, are the significant elements in cross-national comparisons of this sort. Consequently, I would respectfully submit that Professor Garraty would have done us a much greater service if he had elaborated his English or French comparison and reduced, if not eliminated, his German. Then, I think, some rather striking avenues for future research and study in regard to the impact of the depression would have been opened up. The German comparison as it worked out suggests only one, and that one has been excluded by the author from the outset—namely, that the Nazis and the New Deal were more alike than we had suspected.

CARL N. DEGLER  
*Stanford University*

TO THE EDITOR:

I found John Garraty's article fascinating. I agree with the similarities he finds between Hitler's Germany and Roosevelt's America and would also accept his comparisons and contrasts with Great Britain. I do not feel, however, that the pages devoted to Léon Blum are quite as good. In the first place, I noticed no

reference to the fact that Blum was Jewish. To be a Jewish président du Conseil in France in 1936 was probably a greater handicap than being Austrian-born (Hitler), having a Harvard accent (FDR), or being old (Chamberlain).

More important is the factor of the interplay of France's parliamentary system and the multiplicity of parties. Let us not speak of Hitler who, once in power, destroyed all legal opposition and only had to fear a coup d'état. Roosevelt, elected in 1932, knew he had four full years and, after 1936, that he had four more years. True, Chamberlain was responsible to Parliament, but British parties are usually well disciplined and he could presumably count on Tory support for five years. In fact, due to the war, the Parliament elected in 1935 lasted ten years. Admittedly he could be and was eventually replaced by another Conservative.

Now let us look at Blum. He was at the head of one of three parties in a coalition. Professor Garraty has himself shown that Blum's own socialists were divided. In addition, he had to keep simultaneously the support of the Radical Socialists (who, as most people know, were neither radicals nor socialists) and the Communists. Perhaps Blum should have tried for more profound reforms but his position was not easy: contrary to FDR, Hitler, and Chamberlain, Blum might be overthrown any day in the Chamber or in the Senate. The loss of either of his coalition parties would be fatal. In addition, Blum did not want to be a Kerensky, always a possibility. I would dispute Garraty's sentence: "Yet Blum's efforts were pitifully inadequate, no better or worse than those of uninspired premiers who preceded and followed him." I wonder, incidentally, how many AHA members could even name the premier immediately before Blum in 1936 or his successor in 1937.

On page 938, Mr. Garraty says the franc was worth about five cents in 1935. If my memory is correct, one got fifteen francs for the dollar, which would come to nearly seven cents for a franc.

BERNARD SINSHEIMER  
*University of Maryland,  
European Division*

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## Recent Deaths

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J. SALWYN SCHAPIRO, emeritus professor of history at the City College of New York, died in New York on December 30, 1973, at the age of ninety-four. Born December 19, 1879, at Hudson, New York, he was graduated from City College in 1904. He returned to the college as a tutor in history and served there for forty years until 1947, when he was awarded the City College Townsend Harris Medal for outstanding alumni achievement. He received his doctorate from Columbia University in 1909.

Schapiro's first scholarly book was his dissertation, *Social Reform and the Reformation*, which appeared in 1909, the year he was promoted to instructor. From then on a series of scholarly works and textbooks flowed from his pen. His *Modern and Contemporary European History* was for several decades the most popular textbook in its field and dominated the national market. *Liberalism and the Challenge of Fascism* (1948), one of his finest works, emphasized the great and lasting values of the liberal state. His two paperbacks, *Liberalism: Its Meaning and History* (1958) and *Movements of Social Dissent in Modern Europe* (1962), were perennial best sellers. *The World in Crisis: Political and Social Movements in the 20th Century* (1950) was widely quoted as a model of historical analysis.

He had a deep-rooted sense of social justice. At the age of eighteen he was a founder of Madison House, a New York City settlement house at the Downtown Ethical Culture Society under the leadership of Dr. Henry Moskowitz and others. Schapiro's aim was "to welcome the refugees and the immigrants for whom the Gay Nineties were less than gay, and to strengthen the home ties between immigrant parents and American-bred children."

In 1934 Schapiro was elected head of the City

College chapter of the American Association of University Professors. During World War II he worked for the war department's historical service. From 1939 to 1941 he was acting chairman of his department. He was a visiting professor at Columbia University, the New School, the Universities of Southern California, Colorado, and Montana, and the North Carolina College for Women. He was an active member of the board of editors of the *Journal of the History of Ideas*.

Schapiro was the true intellectual. A gentle man, rich in intellect, a scholar who combined erudition with compassion, he was a rare human being who had a strong impact on teachers and students alike. His friendship was firm and giving. He had no ulterior motives. All who knew him agree that he was—unconsciously to be sure—the apotheosis of the great academician.

LOUIS L. SNYDER  
City College,  
City University of New York

CLIFFORD KENYON SHIPTON knew more about the lives and thoughts of eighteenth-century Harvard graduates than any man who lived at that time. In a little over forty years he wrote fourteen volumes of *Biographical Sketches of Those who Attended Harvard College* in which he revived the memory of more than 2,000 men from the classes of 1690 through 1771. Born August 5, 1902, in Pittsfield, Massachusetts, he was a member of the Harvard class of 1926. In 1930 he was engaged by Samuel Eliot Morison to continue for the Massachusetts Historical Society this biographical work which John Langdon Sibley had begun in 1859. Shipton received his Harvard Ph.D. in 1933. Although

he held other posts that would have been full-time occupations for an ordinary man, he continued work on this project for the rest of his life. Through great industry in research, combined with a beguiling literary style, he constantly brought the obscure dead to life. Any volume of Shipton's Sibley is as good bedside reading as John Aubrey's lives. Happily Shipton was more prolific than his seventeenth-century predecessor. For readers without access to the whole series, the Harvard University Press in 1963 published an anthology of "Shipton's Lives" under the title *New England Life in the 18th Century*.

In 1938 he was put in charge of the Harvard University Archives, a post that he continued to fill after he went to Worcester in 1940 as librarian of the American Antiquarian Society. He was director of that society from 1959 until he retired in 1967. At Worcester, aside from being responsible for many other important bibliographical works, he edited 166 boxes of microcards that reproduced all American imprints through the year 1800. In 1967-68 he was president of the Society of American Archivists, and from 1961 until November 1973 president of the Colonial Society of Massachusetts. Although he retired from the Harvard Archives in 1969, he was working there on a Sibley biography the day before his death on December 4, 1973.

In addition to Sibley and his great bibliographical works, Shipton found time to write

biographies of Roger Conant and Isaiah Thomas, to take an active part in the town affairs of Shirley, Massachusetts, and to be endlessly helpful to his colleagues and students. He received an honorary doctorate of letters from Harvard in 1965 and from Clark University in 1969.

WALTER MUIR WHITEHILL  
*Boston Athenaeum*

Other members of the association who have died recently include: Andre A. Beaumont of New York; Francis Benjamin of Emory University in Atlanta, Georgia; Donald Bradeen of the University of Cincinnati, Ohio; James Brewer of the University of North Carolina in Chapel Hill, North Carolina; Ralph Burton of Detroit, Michigan; Catherine Crary of Scarsdale, New York; Florence Crofut of Hartford, Connecticut; Charles A. Culotta of Bryn Mawr College, Pennsylvania; T. J. Curry of Atlanta, Georgia; C. P. Foulke of Philadelphia, Pennsylvania; Paul M. Kendall of Lawrence, Kansas; Kathryn J. Lee of Chattanooga, Tennessee; Harold Manakee of Baltimore, Maryland; Rev. Charles H. Metzger, S. J., of Colobiere College in Clarkston, Michigan; Amory O. Moore of Fort Atkinson, Wisconsin; Edward H. Phillips of Charleston, South Carolina; E. S. Phinney of Joplin, Missouri; Donald H. Sheehan of Whitman College in Walla Walla, Washington; and John Paul Yoder of Doylestown, Pennsylvania.

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## Association Notes

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For the past twenty-five years Professor Leland H. Carlson of the University of Southern California has compiled the British Commonwealth and Ireland list for the Recently Published Articles section of the *AHR*. Professor Carlson recently retired and will no longer serve as a section editor. Because of his skill and dedication, the *AHR* has provided comprehensive bibliography for an important and broad field of study. The *AHR* staff and the profession owe a debt of gratitude to Professor Carlson for his outstanding service.

Mrs. Janet Hearne, who has served as an assistant editor of the *AHR*, has been appointed editor of the *AHA Newsletter*, replacing Mrs. Janet Hayman, who has resigned from that position. Miss Maryann Lesso is joining the staff of the *AHR* as an editorial assistant.

## Festschriften and Miscellanies

These volumes, recently received in the *AHR* office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other *Festschriften* and similar volumes that are amenable to reviewing will be found in the review section.

BLANE, ANDREW, editor, and BIRD, THOMAS E., associate editor. *Russia and Orthodoxy: Essays in Honor of Georges Florovsky*. Vol. 3, *The Ecumenical World of Orthodox Civilization*. The Hague: Mouton. 1974. Pp. 250. 80 gls.

GEORGE H. WILLIAMS, Baptismal Theology and Practice in Rome as Reflected in Justin Martyr. HARRY A. WOLFSON, The Identification of *ex nihilo* with Emanation in Gregory of Nyssa. T. F. TORRANCE, The Relation of the Incarnation to Space in Nicene Theology. JOHN MEYENDORFF, Free Will (γνώμη) in Saint Maximus the Confessor. PETER CHARANIS, Church-State Relations in the Byzantine Empire as Reflected in the Role of the Patriarch in the Coronation of the Byzantine Emperor. WILHELM KAHLE, Die Tränen der Frommen in der Gottesbegegnung (Ein Beitrag zur oekumenischen Spiritualität). ERNST BENZ, Sophia—Visionen des Westens. ROBERT STUPPERICH, Protestantismus und Orthodoxie im Gespräch. STEVEN RUNCIMAN, The British Non-Jurors and the Russian Church. YVES M.-J. CONGAR, O.P., Église de Pierre, Église de Paul, Église de Jean: Destin d'un Thème Oecuménique. N. A. NISSIOTIS, An Orthodox View of Modern Trends in Evangelism. C. J. DUMONT, O.P., La Levée des Anathèmes de 1054 (7 Décembre 1965) et sa Signification dans la Conjoncture Oecuménique Contemporaine. CHARLES MOELLER, Nouveaux Aspects de L'Oecuménisme.

FRITZ, PAUL, and WILLIAMS, DAVID, editors. *City & Society in the 18th Century*. Toronto: Hakkert. 1973. Pp. 301. \$12.00.

RONALD GRIMSLEY, Rousseau's Paris. JAMES CLIFFORD, Some Aspects of London Life in the Mid-18th

Century. ALEXANDER G. MCKAY, Piranesi's Impressions of Rome. DAVID RINGROSE, Madrid & Spain, 1560-1860: Patterns of Social & Economic Change. CHARLES F. MULLETT, Community & Communication. ROGER EMERSON, The Enlightenment & Social Structures. NICHOLAS PHILLIPSON, Towards a Definition of the Scottish Enlightenment. PAUL HENRY LANG, Music & the Court in the 18th Century. ERIC FORBES, Tobias Mayer (1723-1762): A Case Study in the Interaction between Cartography, Astronomy, & Navigation During the 18th Century. GEORGE GRINNELL, Newton's Principia as Whig Propaganda. GERALD FINLEY, The Encapsulated Landscape: An Aspect of Gilpin's Picturesque. JAMES LEITH, The Hope for Moral Regeneration in French Educational Thought 1750-1789. ARTHUR SHEPES, Ideological Immigrants in Revolutionary America. ALLAN EVANS, The Shadow of Edward Gibbon. DOUGLAS G. CREIGHTON, A Genevan Reaction to Diderot's *Pensées philosophiques*: Jacques-François de Luc. WILLIAM KINSLEY, Varieties of Infernal Experience: Pope's *Dunciad* & Dante's *Inferno*.

HEARDER, H., and LOYN, H. R., editors. *British Government and Administration. Studies Presented to S. B. Chrimes*. Cardiff: University of Wales Press. 1974. Pp. 250. £5.

H. R. LOYN, The Hundred in England in the Tenth and Early Eleventh Centuries. C. H. KNOWLES, The Justiciarship in England, 1258-1265. ANTHONY STEEL, The Collectors of the Customs in the Reign of Richard II. A. L. BROWN, The English Campaign in Scotland, 1400. J. GWYNFOR JONES, Government and the Welsh Community: the North-east Borderland in the Fifteenth Century. R. A. GRIFFITHS, Patronage, Politics, and the Principality of Wales, 1413-1461. NICHOLAS PRONAY, The Chancellor, the Chancery, and the Council at the End of the Fifteenth Century. GLANMOR WILLIAMS, Prophecy, Poetry, and Politics in Medieval and Tudor Wales. LIONEL WILLIAMS, The Crown and the Provincial Immigrant Communities in Elizabethan England. IVAN ROOTS, Lawmaking in the Second Protectorate Parliament. C. D. CHANDAMAN, The Financial Settle-

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sannes d'Ancien Régime. TAMÁS HOFFMANN, Faillite et culture de la paysannerie (Hongrie—XIX<sup>e</sup> siècle). IMRE KATONA, L'organisation et les communautés temporaires des ouvriers saisonniers instables en Hongrie (1848-1945). PIERRE BARRAL, Le monde agricole français vers 1950. PÉTER GUNST, L'agriculture hongroise dans la première moitié du XX<sup>e</sup> siècle. MAGDA SOMLYAI, Le partage des terres. LÁSZLÓ KOMLÓ, Révolution agraire et industrialisation du complexe alimentaire hongrois. VICTOR-L. TAPIÉ, Résultats et conclusions.

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*Cloud* 24-25 mai 1967. Paris: Mouton. 1973. Pp. 269. 64 fr.

P. LÉVÊQUE, Les différenciations sociales au sein de la démocratie athénienne du V<sup>e</sup> siècle. C. MOSSÉ, Les classes sociales à Athènes au IV<sup>e</sup> siècle. P. VIDAL-NAQUET, Les esclaves étaient-ils une classe? C. NICOLET, Essai d'histoire sociale: l'ordre équestre à la fin de la République romaine. A. CHASTAGNOL, Classes et ordres dans le Bas-Empire. J. BATANY, Le vocabulaire des catégories sociales chez quelques moralistes français vers 1200. P. MICHAUD-QUANTIN, Le vocabulaire des catégories sociales chez les canonistes et les moralistes du XIII<sup>e</sup> siècle. J. BATANY; P. CONTAMINE; B. GUENÉE; and J. LE GOFF, Plan pour l'étude historique du vocabulaire social de l'Occident médiéval. J. LE GOFF, Le vocabulaire des catégories sociales chez saint François d'Assise et ses biographes du XIII<sup>e</sup> siècle. J. DELUMEAU, Mobilité sociale: riches et pauvres à l'époque de la Renaissance. P. GOUBERT, Remarques sur le vocabulaire social de l'Ancien Régime. J. C. PERROT, Rapports sociaux et villes au XVIII<sup>e</sup> siècle. J. DUPAQUIER and J. JACQUART, Les rapports sociaux dans les campagnes françaises au XVIII<sup>e</sup> siècle: quelques exemples. M. BOULOISEAU, Inspiration. rédaction et vocabulaire des doléances rurales pour les États Généraux de 1789. J. GEORGELIN, Ordres et classes à Venise aux XVII<sup>e</sup> et XVIII<sup>e</sup> siècles. A. J. TUDESQ, Les survivances de l'Ancien Régime: la noblesse dans la société française de la première moitié du XIX<sup>e</sup> siècle. A. DAUMARD, Les fondements de la société bourgeoise en France au XIX<sup>e</sup> siècle. J. LÉONARD, L'exemple d'une catégorie socio-professionnelle au XIX<sup>e</sup> siècle: les médecins français. J. BRUHAT, Le mouvement ouvrier français au début du XIX<sup>e</sup> siècle et les survivances d'Ancien Régime. A. SOBOUL, Survivances "féodales" dans la société rurale française au XIX<sup>e</sup> siècle. C. E. LABROUSSE, Conclusion.

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moderna. MARTIN VAN DEN BRUWAENE, Influence d'Aratus et de Rhodes sur l'oeuvre philosophique de Cicéron. KENNETH DOUGLAS WHITE, Roman Agricultural Writers I: Varro and his Predecessors.

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Michael E. Moody, University of Denver

Frederic A. Youngs, Jr., Louisiana State University, Baton Rouge

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Lynn M. Case and Margaret Klein,  
University of Pennsylvania

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C. J. Bishko, University of Virginia

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Emiliana P. Noether, University of Connecticut

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Charles Morley and Carole Rogel, Ohio State University

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David E. Gardinier, Marquette University

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## A GUIDE TO THE ABBREVIATION OF JOURNAL TITLES

GENERAL RULES: Proper names are spelled out with the exception of adjective forms and names of countries used to identify place of publication. Whenever possible, articles, conjunctions, and prepositions are deleted. Exceptions are relatively short titles such as *Past & Present* or *The Americas*.

<i>abt</i>	Abteilung	<i>bol</i>	boletim, boletín
<i>acad</i>	academia, academy	<i>boll</i>	bollettino
<i>accad</i>	accademia	<i>brandenburg</i>	brandenburgisch
<i>adm</i>	administration, administrative	<i>bras</i>	Brasil, brasileira, Brazilian
<i>aff</i>	affairs	<i>braunsch</i>	braunschweigisch
<i>afric</i>	africain, African, Africana, africanum	<i>Braz</i>	Brazilian
<i>afrik</i>	afrikaanse	<i>brem</i>	bremisches
<i>agrar</i>	agrarisches	<i>Brit</i>	British
<i>agric</i>	agricultural, agriculture	<i>bull</i>	bulletin
<i>AHR</i>	<i>American Historical Review</i>	<i>bus</i>	business
<i>aikakausk</i>	aikakauskirja	<i>byz</i>	byzantine
<i>akad</i>	Akademie		
<i>Ala</i>	Alabama	<i>cah</i>	cahiers
<i>Alas</i>	Alaska	<i>Calif</i>	California
<i>alemann</i>	alemannisches	<i>Can</i>	Canada, Canadian
<i>allg</i>	allgemein	<i>Carib</i>	Caribbean
<i>altertumsk</i>	Altertumskunde	<i>cath</i>	catholic
<i>alttest</i>	alttestamentliche	<i>cent</i>	century
<i>Am</i>	American, Americana, Amerikas	<i>cercet</i>	cercetări
<i>an</i>	anales, annalen, annales, annali, annals, annua, annuaires, annual, annuarium, anuarul	<i>českoslov</i>	československý
		<i>chron</i>	chronicles, chronique
<i>anc</i>	ancien, ancient	<i>circ</i>	circle, circular
<i>annot</i>	annotation, annotator	<i>civil</i>	civilization
<i>anthol</i>	anthologica, anthology	<i>class</i>	classica, classical, classique
<i>anthropol</i>	anthropological, anthropologie, anthropology	<i>co</i>	county
		<i>coll</i>	college
<i>antiq</i>	antiquarian, antiquarisch, antiquarischen, antiquité, antiquities, antiquity	<i>collect</i>	collection, collections
		<i>Colo</i>	Colorado
<i>antol</i>	antologia	<i>com</i>	comité, committee
<i>antropol</i>	antropologiczny	<i>comm</i>	commerce
<i>anz</i>	anzeiger	<i>comp</i>	comparate, comparative, comparée
<i>appenzell</i>	appenzellische	<i>compil</i>	compilation, compiled, compiler
<i>arch</i>	archiven, archives, archivio, archivo, archiv, archivum	<i>concl</i>	conclusion
		<i>conf</i>	conference
<i>archaeol</i>	archaeolog, archaeologia, archaeology	<i>cong</i>	congress
<i>archäol</i>	archäologie, archäologische, archäologischer	<i>Conn</i>	Connecticut
		<i>cont'd</i>	continued
<i>archeol</i>	archeologia, archeologica, archeologie, archeology	<i>contemp</i>	contemporaine, contemporánea, contemporary
			contributed, contribution, contributor
<i>Ariz</i>	Arizona	<i>contrib</i>	contribution
<i>Ark</i>	Arkansas	<i>corp</i>	corporation
<i>ark</i>	arkiv	<i>corr</i>	correspondence
<i>arq</i>	arquivos	<i>c. r.</i>	comptes rendus
<i>arqueol</i>	arqueológico	<i>crit</i>	critica, criticism
<i>ari</i>	article	<i>cuad</i>	cuaderno
<i>assoc</i>	association	<i>cult</i>	cultura, cultural, culture
<i>assyriol</i>	assyriological, assyriologie, assyriology		
<i>at</i>	atti	<i>D.C.</i>	District of Columbia
<i>Atl</i>	Atlantic	<i>Del</i>	Delaware
<i>AUMLA</i>	<i>Journal of the Australasian Universities Language and Literature Association</i>	<i>demog</i>	demografie, demographische, demography
<i>autobiog</i>	autobiography	<i>Den</i>	Denmark
		<i>dept</i>	department
<i>b</i>	buch (compounds only)	<i>deux</i>	deuxième
<i>balt</i>	Baltic, baltisch	<i>dev</i>	developing, development
<i>bayer</i>	bayerisch	<i>dig</i>	digest
<i>Beitr</i>	Beitrag, Beiträge	<i>dipl</i>	diplomatic, diplomatique
<i>Ber</i>	Bericht	<i>doc</i>	documentation, documents
<i>bibl</i>	bibliotek, bibliotheca, bibliothèque	<i>dok</i>	dokuments
<i>bibliogr</i>	bibliografice, bibliographical, bibliography	<i>drev</i>	drevnei
<i>bijd</i>	bijdragen	<i>dsch</i>	deutsche, deutschen, deutsches
<i>biog</i>	biography		
<i>bk</i>	book	<i>e</i>	east, eastern
<i>bl</i>	Blatt, Blätter	<i>ec</i>	economics, économique, economy
<i>BMGN</i>	<i>Bijdragen en Mededelingen betreffende de Geschiedenis der Nederlanden</i>	<i>eccles</i>	ecclesiastical
		<i>ecles</i>	eclesiástico
		<i>ed</i>	edited, edition, editor

<i>educ</i>	education	<i>int</i>	internacional, international, internationale, internazionale
<i>EEH</i>	<i>Explorations in Economic History</i>	<i>interdisc</i>	interdisciplinary
<i>EHR</i>	<i>English Historical Review</i>	<i>intern</i>	internal
<i>ELH</i>	<i>English Literary History</i>	<i>introd</i>	introduced, introduction
<i>Eng</i>	English	<i>ist</i>	istorii, istorijski, istoriski
<i>epig</i>	epigraphik, epigraphy	<i>istruz</i>	istruzione
<i>Epis</i>	Episcopal	<i>ital</i>	Italian, italiana, italienisch
<i>estud</i>	estudios		
<i>et</i>	études		
<i>ethnog</i>	ethnographisch	<i>j</i>	journal
<i>ethnol</i>	ethnological, ethnology	<i>jb</i>	Jahrbuch, Jahrbücher
<i>etnol</i>	etnologia	<i>JEGP</i>	<i>Journal of English and Germanic Philology</i>
<i>Eur</i>	Europäische, Europas, Europe, European, européennes	<i>jugoslov</i>	jugoslovenski
<i>ev</i>	evangelisch	<i>jur</i>	juridical, juridiceski, juridique
<i>explor</i>	explorations		
		<i>kan</i>	kanonistisch
<i>fac</i>	faculté, faculty	<i>Kans</i>	Kansas
<i>facs</i>	facsimile	<i>kath</i>	katholik, katholisch
<i>fak</i>	Fakulte	<i>kd</i>	Kunde
<i>fil</i>	filosofia, filozofski, filozofskog	<i>kl</i>	Klasse
<i>filol</i>	filología	<i>Ky</i>	Kentucky
<i>Fla</i>	Florida		
<i>for</i>	foreign	<i>La</i>	Louisiana
<i>försch</i>	Forschung, Forschungen	<i>landesk</i>	Landeskunde
<i>fr</i>	français, France, French	<i>lang</i>	language
<i>francisc</i>	franciscanos, franciscanum	<i>lett</i>	letter, letterário, letteratura, literature, lettre
<i>fränk</i>	fränkische	<i>lib</i>	library
<i>frankf</i>	frankfurter	<i>Lib Cong</i>	U.S. Library of Congress
<i>fries</i>	friesisches	<i>libr</i>	librarian
		<i>ling</i>	linguistics, linguistique
<i>g</i>	giornale	<i>lit</i>	literary, literatur, literature, literary, littérature
<i>Ga</i>	Georgia	<i>lübeck</i>	lübeckische
<i>gaz</i>	gazette	<i>lüneburg</i>	lüneburger
<i>gen</i>	general, général		
<i>geneal</i>	genealogy	<i>mag</i>	magasin, magazine
<i>geog</i>	geografi, geográfico, geographic, geographical, géographique, geographischen, geography	<i>marit</i>	maritime
	German	<i>Mass</i>	Massachusetts
<i>Ger</i>	germanistisch	<i>Md</i>	Maryland
<i>germ</i>	Gesellschaft	<i>Me</i>	Maine
<i>ges</i>	Geschichte, geschichtliche	<i>med</i>	medieval, médiévale, medievals, medievalia
<i>gesch</i>	gospodarczych	<i>meded</i>	mededelingen
<i>gos</i>	government	<i>Mediterr</i>	Mediterranean
<i>gout</i>	graduate	<i>mél</i>	mélanges
<i>grad</i>	Grafschaft	<i>mém</i>	mémoires, memorial, memorie
<i>grafsch</i>		<i>mennonit</i>	mennonitische
		<i>Mex</i>	Mexican
<i>h</i>	hefte (compounds only)	<i>Mich</i>	Michigan
<i>hamburg</i>	hamburgisch	<i>mid</i>	middle
<i>hann</i>	hannoversche	<i>midcont</i>	midcontinental
<i>hell</i>	hellenic, hellénique, hellenistic	<i>mil</i>	militaire, militarisch, military
<i>helvet</i>	helvetian	<i>Minn</i>	Minnesota
<i>hess</i>	hessisch	<i>misc</i>	miscelánea, miscellany
<i>Hi</i>	Hawaii	<i>Miss</i>	Mississippi
<i>hisp</i>	hispanic, hispánicos, hispanique	<i>mitt</i>	Mitteilung, Mitteilungen
<i>hist</i>	histoire, historiae, historialinen, historical, historická, historický, histórico, historicum, historique, historisch, history, historyczne	<i>Mo</i>	Missouri
	hohenzollerische	<i>mod</i>	modern, moderna, moderne
<i>hohenzoll</i>	holsteinisch	<i>mond</i>	mondiale
<i>holstein</i>		<i>Mont</i>	Montana
<i>iaz</i>	iazyka	<i>monum</i>	monumenta
<i>Ida</i>	Idaho	<i>movim</i>	movimento
<i>Ill</i>	Illinois	<i>mt</i>	mountain
<i>illus</i>	illustrated	<i>mus</i>	musée, musei, museum
<i>ind</i>	industrial, industry		
<i>Inda</i>	Indiana	<i>n</i>	north, northern
<i>individ</i>	individual	<i>nac</i>	nacional
<i>inscr</i>	inscription	<i>nass</i>	nassauische
<i>inst</i>	institut, institute, institution, instituto, institutului	<i>nat</i>	national

<i>nationalok</i>	nationaløkonomie, nationalökonomisk	<i>rep</i>	report, reporter
<i>naz</i>	nazionale	<i>repub</i>	republic, republicii
<i>N.C.</i>	North Carolina	<i>res</i>	research
<i>N.D.</i>	North Dakota	<i>rev</i>	revolution
<i>ne</i>	northeast	<i>rhein</i>	rheinisch
<i>Nebr</i>	Nebraska	<i>R.I.</i>	Rhode Island
<i>neutest</i>	neutestamentliche	<i>ric</i>	ricerche
<i>Nev</i>	Nevada	<i>rocz</i>	roczniki
<i>newslett</i>	newsletter	<i>röm</i>	römische
<i>N.H.</i>	New Hampshire	<i>roman</i>	romanische
<i>niedersächs</i>	niedersächsisch	<i>roy</i>	royal
<i>N.J.</i>	New Jersey		
<i>N.M.</i>	New Mexico	<i>s</i>	south, southern
<i>no</i>	number	<i>S.C.</i>	South Carolina
<i>nos</i>	numbers	<i>Scand</i>	Scandinavia, Scandinavian
<i>Nor</i>	Norway	<i>sch</i>	school
<i>nord</i>	nordisk	<i>schles</i>	schlesisch
<i>norm</i>	normale	<i>schr</i>	Schrift
<i>numis</i>	numismatic, numismatique	<i>schweiz</i>	schweizerisch
<i>nw</i>	northwest	<i>sci</i>	science, scientiarum, scientific, scientist, scienze
<i>N.Y.</i>	New York	<i>S.D.</i>	South Dakota
<i>obit</i>	obituary	<i>se</i>	southeast
<i>oesterr</i>	oesterreichisch	<i>sec</i>	sectio, section
<i>ok</i>	økonomic	<i>ser</i>	série, series
<i>Okla</i>	Oklahoma	<i>slaw</i>	slawistik
<i>Ore</i>	Oregon	<i>soc</i>	social, societatis, society
<i>organ</i>	organization	<i>sociog</i>	sociographiques
<i>orient</i>	oriental, orientale, orientalia, orientalistyczny	<i>sociol</i>	sociologia, sociological, sociology
<i>österr</i>	österreichisch	<i>solothurn</i>	solothurnische
<i>osth</i>	Osthefte	<i>sozial</i>	sozialistischen
		<i>soziol</i>	Soziologie
<i>Pa</i>	Pennsylvania	<i>Span</i>	Spanish
<i>Pac</i>	Pacific	<i>späl</i>	Spølecznuch
<i>pädagog</i>	pädagogik, pädagogisch	<i>stat</i>	statistical, statistics, Statistik
<i>paedagog</i>	paedagogica	<i>stift</i>	stiftung
<i>pap</i>	papers	<i>stor</i>	storia, storici, storico
<i>papyrol</i>	papyrologie	<i>stud</i>	studi, studia, Studien, studies, studium
<i>parl</i>	parlementaire, parliament	<i>sup</i>	superiore
<i>pfälz</i>	pfälzische	<i>suppl</i>	supplement
<i>phil</i>	philosophical, philosophique, philosophy	<i>sw</i>	southwest
<i>philol</i>	philology	<i>Swed</i>	Sweden, Swedish
<i>photo</i>	photograph	<i>symp</i>	symposium
<i>pol</i>	political, político, politics, Politik, politique, politische	<i>tech</i>	technisch
<i>pop</i>	popular	<i>technol</i>	technology
<i>port</i>	portuguesa, portuguese	<i>Tenn</i>	Tennessee
<i>pres</i>	president, presidential	<i>test</i>	testament, testamentum
<i>Presb</i>	Presbyterian	<i>Tex</i>	Texas
<i>preuss</i>	preussisch	<i>theol</i>	theological, theologie, theology
<i>probl</i>	problems	<i>tids</i>	tidskrift, tidsskrift
<i>proc</i>	proceedings	<i>tijd</i>	tijdschrift
<i>prot</i>	protestant, Protestantismus	<i>tr</i>	translated, translation, translator
<i>prov</i>	provençe, provinces	<i>trans</i>	transactions
<i>PSQ</i>	<i>Political Science Quarterly</i>	<i>trav</i>	travail, travaux
<i>psych</i>	psychology		
<i>pt</i>	part	<i>u</i>	und
<i>pts</i>	parts	<i>U</i>	Universitătii, university
<i>publ</i>	publication, publishing	<i>unter</i>	Unterricht
<i>q</i>	quaderni, quarterly	<i>Va</i>	Virginia
<i>quel</i>	quellen	<i>vaterl</i>	vaterlandisch
<i>r</i>	review, revista, revue, rivista	<i>ver</i>	Verein, vereinigung, Vereins
<i>rass</i>	rassegna	<i>verh</i>	Verhandlungen
<i>rdsch</i>	Rundschau	<i>veröff</i>	Veröffentlichungen
<i>rec</i>	record	<i>vesn</i>	vesnik
<i>rech</i>	recherches	<i>vest</i>	vestnik
<i>regist</i>	register	<i>volksk</i>	volkskunde
<i>relig</i>	religieuse, religion, religiöse, religious	<i>vopr</i>	voprosy
<i>rend</i>	rendiconti	<i>vrem</i>	vremennuk
		<i>Vt</i>	Vermont

<i>w</i>	west, western	<i>yrbk</i>	yearbook
<i>Wash</i>	Washington		
<i>westf</i>	westfälisch	<i>z</i>	Zeitschrift, Zeitschriften
<i>wirtsch</i>	Wirtschaft, wirtschaftlich	<i>zeitgesch</i>	Zeitgeschichte
<i>Wis</i>	Wisconsin	<i>zgodov</i>	zgodovinski
<i>wiss</i>	Wissenschaft, wissenschaftlich	<i>zhurn</i>	zhurnal
<i>WMQ</i>	<i>William and Mary Quarterly</i>		
<i>württemb</i>	württembergisch		
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**MEMBERSHIP:** Persons interested in historical studies, whether professionally or otherwise, are invited to membership. The present membership is about 18,000. Members elect the officers by ballot.

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**PUBLICATIONS AND SERVICES:** The *American Historical Review* is published five times a year and sent to all members. It is available by subscription to institutions. The Association also publishes its *Annual Report*, the *AHA Newsletter*, a variety of pamphlets on historical subjects, and bibliographical and other volumes. To promote history and assist historians, the Association offers many other services, including publication of the *Employment Information Bulletin* four times a year. It also maintains close relations with international, specialized, state, and local historical societies through conferences and correspondence.

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**CORRESPONDENCE:** Inquiries should be addressed to the Executive Director at 400 A Street, S.E., Washington, D.C. 20003.

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The *AHR* is sent to all members of the American Historical Association; information concerning membership will be found on the preceding page. The *AHR* is also available to institutions by subscription. There are two categories of subscription:

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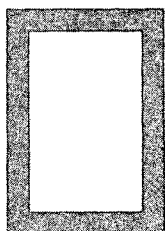
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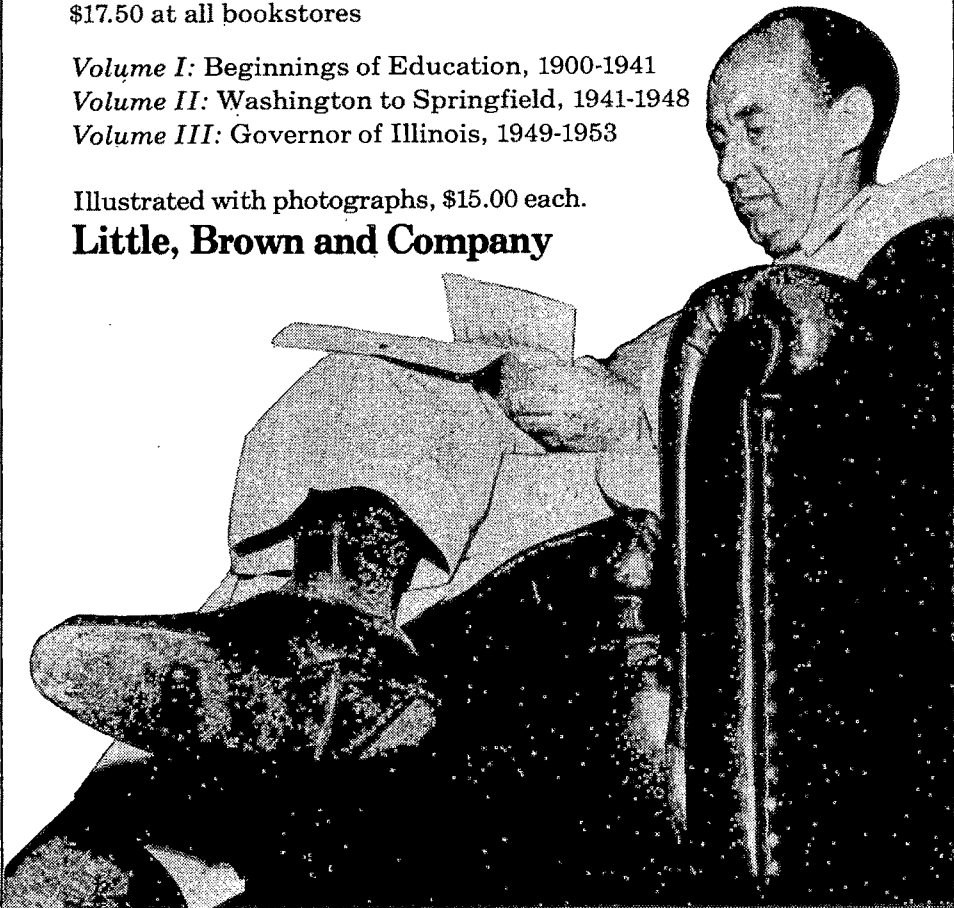
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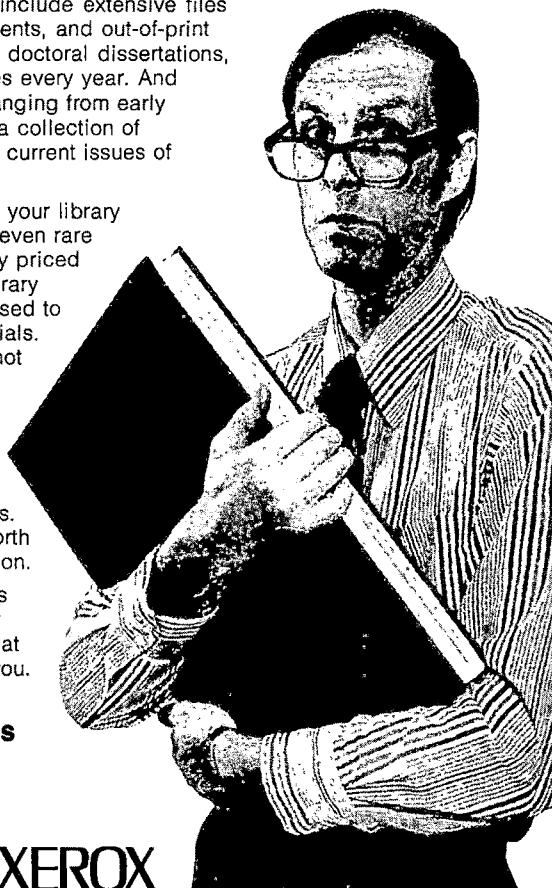
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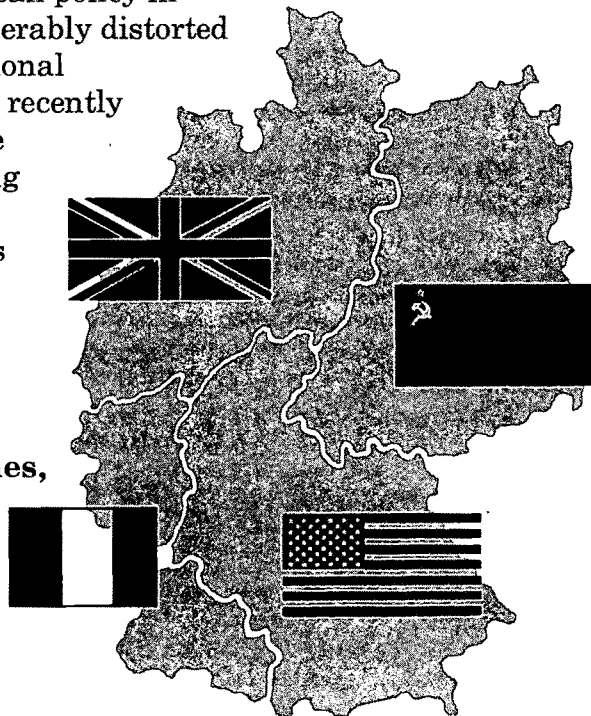
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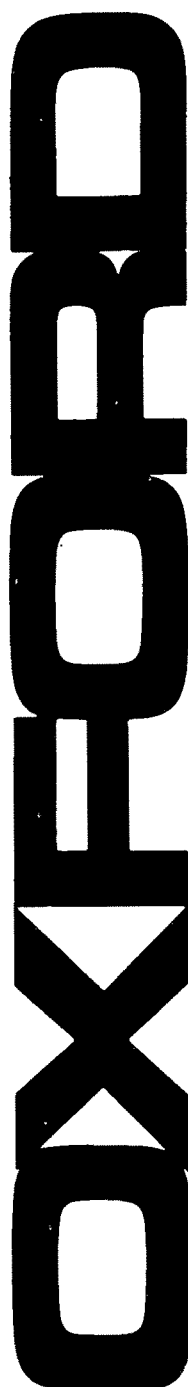
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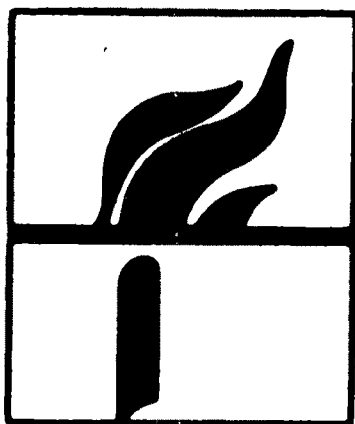
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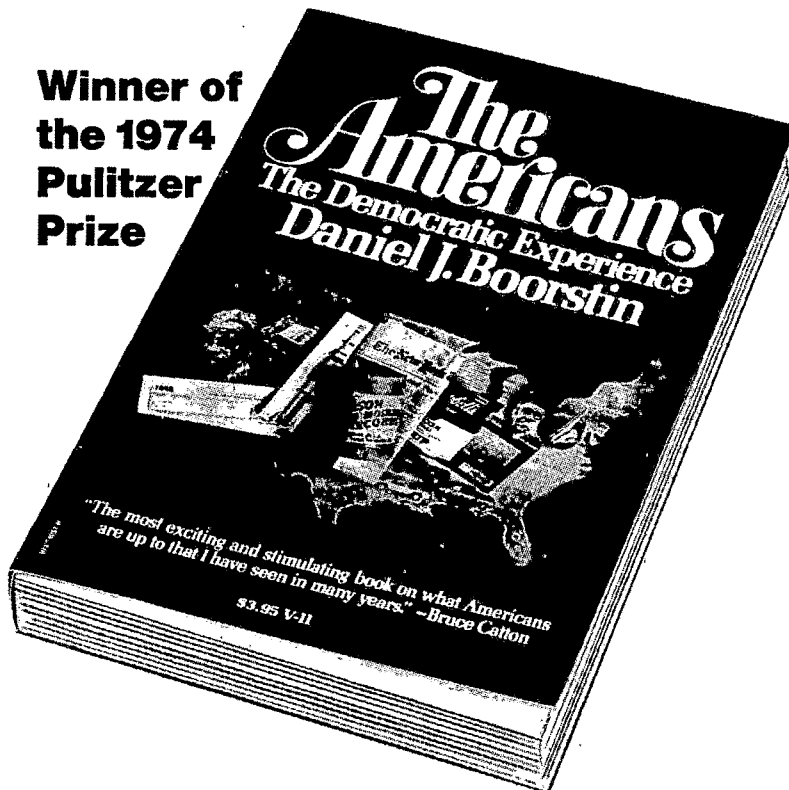
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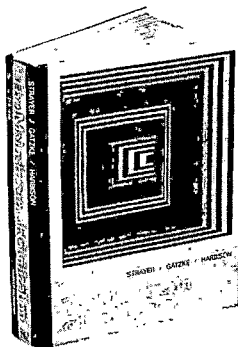
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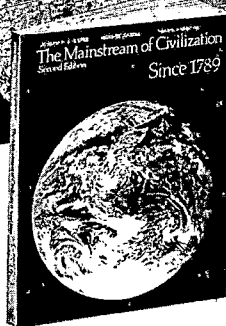
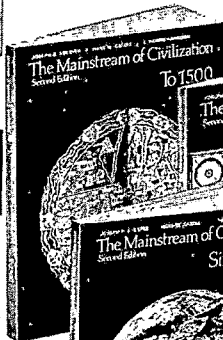
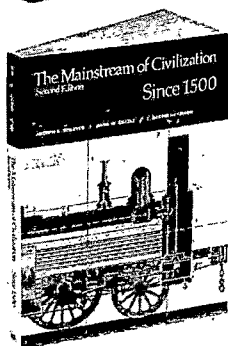


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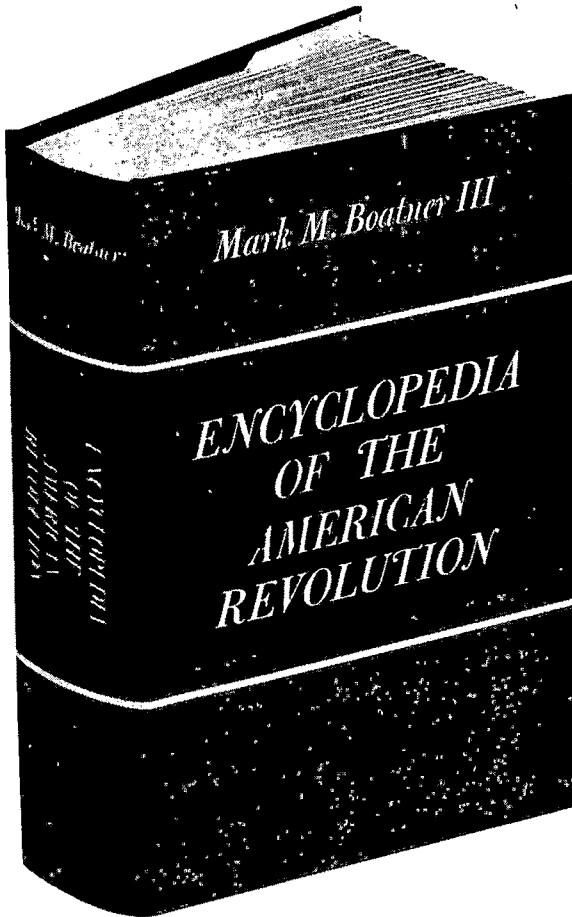
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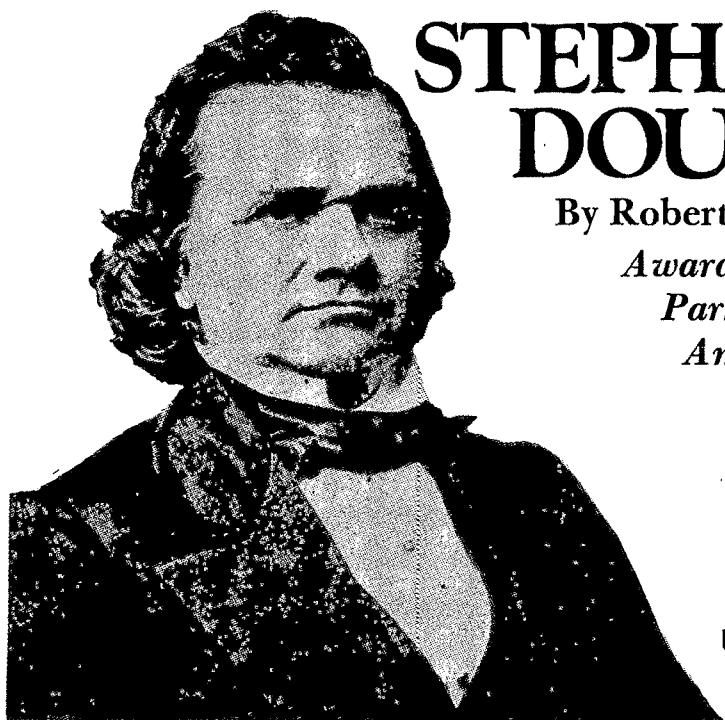
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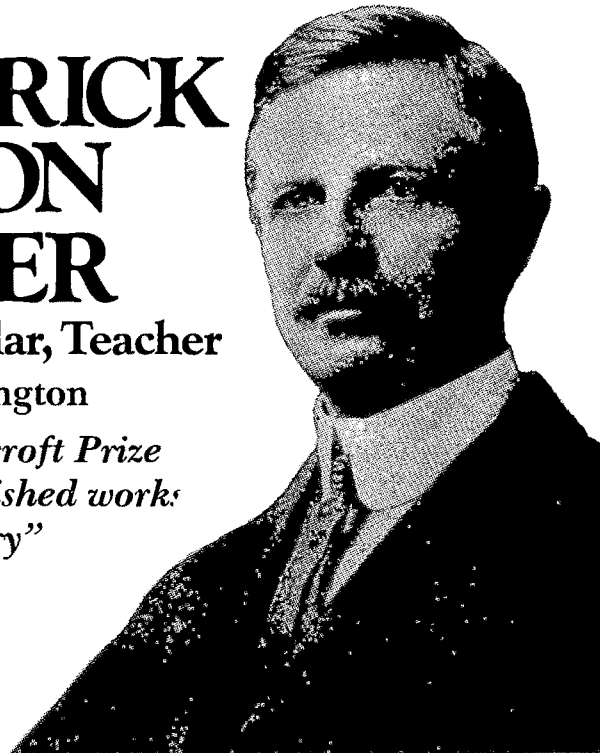
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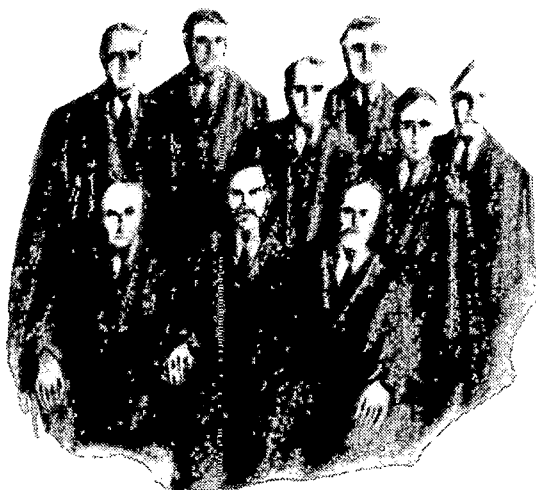
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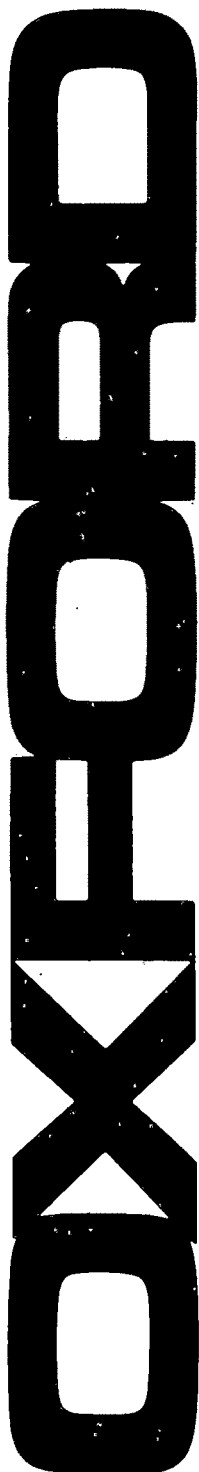
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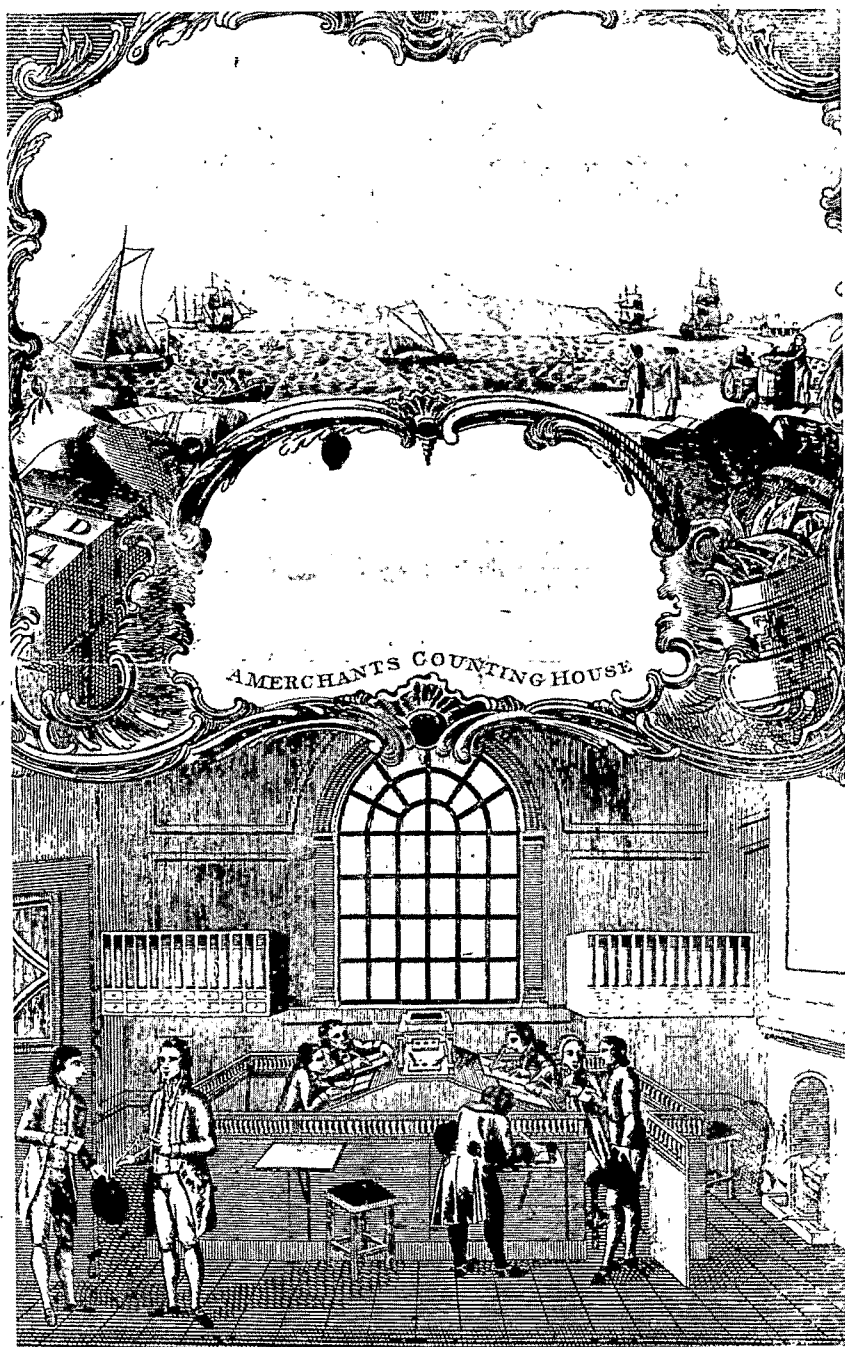
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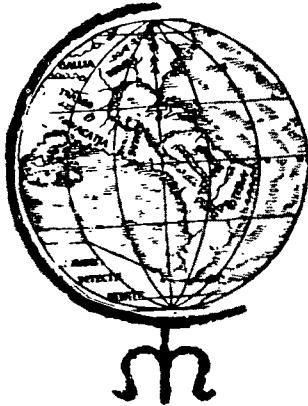


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THOMAS C. COCHRAN is Benjamin Franklin Professor Emeritus of the University of Pennsylvania and Senior Scholar in Residence at the Eleutherian Mills-Hagley Foundation, where he works with students from the University of Delaware. His most recent book is *The Uses of History* (Wilmington, 1974), and he is currently working on a history of management in the United States in comparison with that of other industrial nations.

CARL N. DEGLER is Margaret Byrne Professor of American History at Stanford University. His books include *Out of Our Past: the Forces that Shaped Modern America* (New York, rev. ed., 1970), *Neither Black Nor White: Slavery and Race Relations in Brazil and the United States* (New York, 1971), and *The Other South: Southern Dissenters in the Nineteenth Century* (New York, 1974). His principal field of interest is American social history and the history of the American South. His graduate work was done at Columbia University under the directorship of Richard B. Morris. At the present time he is working on a history of women and the family in the United States.

JOEL COLTON, professor of history at Duke University and from 1967 to 1974 chairman of the department, is currently on leave serving as director for Humanities at the Rockefeller Foundation. He is principally interested in twentieth-century Europe with special emphasis on modern France. His volume *Léon Blum: Humanist in Politics* (New York, 1966) has been issued in paperback (Cambridge, Mass., 1974) and has also been translated into French. His present research interests include a study of the Great Depression in Western Europe.

JACK P. GREENE, a professor of history at the Johns Hopkins University, is presently a fellow of the Woodrow Wilson International Center for Scholars. He received his doctorate from Duke University, where he studied under Professor John R. Alden. He is the author of *The Quest for Power: The Lower Houses of Assembly in the Southern Royal Colonies 1689-1776* (Chapel Hill, 1963) and is primarily interested in the sociology of early modern European colonization. He is now engaged in a comparative study of the material culture of Virginia, Jamaica, and South Carolina during the seventeenth and eighteenth centuries.



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# The Business Revolution

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THOMAS C. COCHRAN

SCHOLARS CHIEFLY INTERESTED in economics and technology have called a series of physical and social changes in Western society that occurred between 1750 and 1850 the Industrial Revolution. In keeping with this language the focus of attention has been on the gradual growth of machinery using nonmanual power. When such machines became economically productive they were housed in factories that, in turn, drew workers from other activities, produced new business forms for distribution, created new opportunities for the use of capital, and greatly raised the standard of living. Exemplifying this approach, David Landes writes:

The heart of the Industrial Revolution was an interrelated succession of technological changes. The material advance took place in three areas: . . . substitution of mechanical devices for human skills; . . . inanimate power—in particular steam—took the place of human and animal strength; . . . a marked improvement in the getting and working of raw materials.<sup>1</sup>

Although not always so explicitly stated this general or classic approach assigns a primary and basic function to technology.

Yet most of the same writers would agree that certain social-structural conditions were, and still are, essential to a rapid growth of production by power machinery.<sup>2</sup> Even those who adhere closely to measurable economic factors admit that something has to accelerate growth in the existing or traditional economy.<sup>3</sup> New insights may come, therefore, from reversing the traditional approach of focusing on technology and assuming the social structure necessary for its economic use and from seeing instead technological advances as following the demands of new elements in the business-political-social system.

A general model of the progress from relatively fixed or traditional patterns

<sup>1</sup> *The Unbound Prometheus: Technological Change and Industrial Development in Western Europe from 1750 to the Present* (Cambridge, 1970), 1.

<sup>2</sup> See Peter Mathias, *The First Industrial Nation: An Economic History of Britain 1700–1914* (New York, 1969), 10 ff.; Phyllis Deane, *The First Industrial Revolution* (Cambridge, 1965), 54–68; and David Landes, *Prometheus*, 543–47.

<sup>3</sup> Kazushi Ohkawa and Henry Rosovsky, *Japanese Economic Growth: Trend Acceleration in the Twentieth Century* (Stanford, 1973), 12.



of social structure to new adjustments promoting higher productivity runs as follows. Such social change requires as an initial condition the existence of an entrepreneurial group enjoying relative security and freedom of action and also access to requisite knowledge. Given this condition an increase in the demand for goods so substantial as to exceed the capacity of the existing system of production and exchange will induce business entrepreneurs to enlarge their scale of operations. The process of enlargement, in turn, entails, and indeed depends upon, specialization of a number of functions that were previously performed by one individual businessman. Specialization, an economy of scale, increases the efficiency of the system as a whole, much as division of labor does in production. For example, a better capital market and faster turnover increases the input of this classic factor, while each specialized unit enhances the operating efficiency of the labor and management factors. Although the dynamics of the model have been described, an additional element, efficiency in the relations between specialized units, determines the over-all performance of the system. Among the institutions strongly affecting this efficiency are those of the government, generally regarded as external to the business system.

This approach, through social structure, can readily be substantiated from the history of Britain, as shown by Peter Mathias, or Holland, as described by Charles Wilson, but the interrelationships affecting business are both more rapid and clearly defined in the United States.<sup>4</sup> Here changes that had been gradual in England, stretching over three-quarters of a century, occurred within a generation, with developments in business structure clearly preceding the use of new technology.<sup>5</sup>

Beyond dealing with a different nation and period the present discussion also places more emphasis on human planning and behavior than has been done by economic historians. No one to my knowledge has stressed business organization rather than technology or capital as the leading sector in bringing about economic growth.<sup>6</sup> Even if the concept of a leading sector is dismissed the decisions to use existing technology for new purposes had to originate with businessmen who had, in turn, to be socially conditioned to perceive the specific opportunities for improvement. In the longest possible view the United States did excellently when advance depended on social structure and roles, as used in early business, less well in the later period when administration, science, and technology became more important.

The early American development is made more dramatic by the fact that the coming of the new politico-business system was accompanied by what

<sup>4</sup> Charles Wilson, "Transport as a Factor in the History of Economic Development," *Journal of European Economic History*, 2 (1973): 327-30.

<sup>5</sup> See Thomas C. Cochran, *Business in American Life* (New York, 1972), 61-87. In the opening paragraph, written some half dozen years ago, I am still repeating the classic emphasis on technology.

<sup>6</sup> Business organization is not what most economists define as "human capital." The latter is usually meant to represent expenditures for education and training.

now appears to be the most rapid rise in the standard of living of any contemporary nation in the world. It is this phenomenon, recognized in the work of a number of distinguished economists, that so strongly underlines the role structure of entrepreneurship in finance, trade, and service in creating the modern industrial world.

A reconsideration of priorities was strongly suggested by Robert Gallman's estimates that in 1840 the United States had a gross national product per capita 40 to 65 per cent larger than that of Continental European leaders such as France and approaching that of Great Britain.<sup>7</sup> Since "value added by all manufacture," manual as well as power, stood at under \$250 million in the census of 1840, and steam and iron machinery, aside from steamboats, was just coming into use, clearly some other potent factors had been operative in the rapid advance.

Another economist, Paul A. David, was meanwhile carrying out a detailed study of national income (GNP) from 1800 to 1840, the results of which first reached the scholarly public in 1967. The arresting conclusion is that between 1800 and 1840 real per capita domestic product increased from between 55 to 62 per cent.<sup>8</sup> Since David also finds a gain in the nonagricultural labor force, from 17 per cent of the total in 1800 to 37 per cent in 1840, and a rate of increase in agricultural productivity per worker lower than in non-farm work, the question must be: what nonagricultural factors were causing this great upswing?

In 1973 a book by the geographer Allan R. Pred organized much of the evidence needed for an answer.<sup>9</sup> Linking information flows to volumes of trade and types of business transactions Pred for the first time assembles some of the chief dimensions and characteristics of the expanding American business system. His maps and statistics emphasize, as never before, that upward change in the rate of economic growth depends more on a society that under certain conditions fosters improvements in the business structure for better utilizing land, labor, capital, and entrepreneurship than it does on the particular local resources. Put another way, neither available resources nor technology can by themselves cause change and growth; they require a social system that produces the knowledge and roles necessary to make use of the latent factors.

In the present discussion I will not attempt an analysis of the underlying characteristics of the American society and culture that produced types of businessmen and politicians particularly suited to realizing the opportunities

<sup>7</sup> National Bureau of Economic Research, "Gross National Product 1834-1909," *Output, Employment and Productivity in the United States after 1800* (New York, 1969), 5-7.

<sup>8</sup> "New Light on a Statistical Dark Age: U.S. Real Product Growth Before 1840," *American Economic Review*, 57 (1967): 294-306. The article is reprinted in Peter Temin, ed., *New Economic History* (Baltimore, 1973), 44-60. See table, 50-51. Robert Gallman thinks these figures slightly high, but he would only reduce them about 10 per cent.

<sup>9</sup> *Urban Growth and the Circulation of Information: The United States System of Cities 1790-1840* (Cambridge, Mass., 1973).

of the period from 1750 to 1850. No matter how favorable the national character and social structures were, they had to operate economically through a business system, and, therefore, we will concentrate on the latter. The theme here is to present the immediate conditions that rapidly established the business structure and entrepreneurial roles that were most successful in stimulating economic growth in the world of the early nineteenth century.

To trace the specific evolution in the United States after 1790, the general model needs descriptive elaboration. Increasing trade, both foreign and domestic, created a demand that expanded the scale of mercantile operations, leading, in turn, to more specialization of business processes and hence more division of labor. Governmental power was drawn upon for promotional, as distinct from regulatory, purposes, and state assistance plus higher business profits made it possible to invest the relatively large sums needed for improved transportation and faster flows of commercial information. The increasing flows of capital led to still greater specialization and efficiency in the financial sector; brokers, for example, organized stock exchanges, while service auxiliaries, such as dealers in commercial paper, investment bankers, and specialized law firms, appeared. And finally, all of these factors combined to create a faster moving and more economical world of business with decisions based on better information.

Once the processes of improved business specialization, communication, and transportation were underway, new business emerged most rapidly at the already leading commercial centers. The new local demand generated by mercantile business produced more job opportunities of all types, from banking to domestic service. Pred writes: "Any new or enlarged manufacturing whether of the *entrepôt*, commerce serving or local market variety, generates a secondary multiplier effect and touches off a subsidiary feedback process."<sup>10</sup> By "manufacturing" Pred does not exclude purely manual operations. Just as urban size generates specialization in the administrative functions of business, it leads to bigger units and more specialization in manual fabrication, often creating "factories" that are merely collections of specialized craftsmen. Furthermore, the short-term payoff in less-roundabout hand processes was considerably larger relative to investment than in the case of capital sunk into experiments in pioneer utilization of advanced technology.<sup>11</sup>

ALTHOUGH THE RELATIONS OF BUSINESS CHANGE to the application of more machine technology in Europe are less distinct in time than in the United States, it seems wise for comparative purposes briefly to recount the well-

<sup>10</sup> *Ibid.*, 191-92.

<sup>11</sup> See Gene Cesari, "Technology in the American Arms Industry, 1790-1860" (Ph.D. dissertation, University of Pennsylvania, 1970), for the unprofitability of Ely Whitney's and other early attempts at mass production.

known history of such evolution in Britain. In the eighteenth century a great increase in the volume of water-borne trade, particularly with overseas colonial areas, created new demands for both English goods and for the marketing of colonial products. While France and the Low Countries shared in this commercial upsurge, the United Kingdom was the chief beneficiary. The demand for more goods for export, for better means of inland transportation, for more ships to carry the goods, and for more capital to carry on all the operations led to essential business advances that preceded the major technological changes that are commonly referred to as the Industrial Revolution. By the early nineteenth century, when iron and steel machinery, powered by steam, began to come into use in Britain, the Business Revolution had already occurred.

The close connection of world trade, generated by the overseas empire, with initiation of the new business developments is illustrated by Peter Mathias's observation about eighteenth-century England: "Enormous wealth and capitals were piling up there exactly in response to the redirection of trade encouraged by the operation of the Navigation Acts."<sup>12</sup> These laws made the chief United Kingdom ports—Bristol, Glasgow, Liverpool, and London—the European distribution points for most British colonial exports. The Navigation Acts may have been burdensome to the North American colonies, but they were an innovating force in the mother country.

As described in our model the Business Revolution in Britain was based on separating the many functions of the old merchant capitalist into specialties such as banking, insurance, and transportation, great improvements in the distribution of business information and goods, and the systematic organization of handicraft manufacture. In the process legal forms were altered either by acts of Parliament or interpretations of the common law. Participation in the new types of enterprise were, of course, not limited to large merchant capitalists. Producers such as clothiers or hosiers with small capitals might organize cottage industry by supplying raw materials and leasing inexpensive hand-operated machines, like Kay's flying shuttle, to household workers. Similarly, so many small capitalists went into country banking that by mid-century among brewers alone fifty had started banks.<sup>13</sup> Meanwhile, marine and fire insurance were made routine specialties by such merchant-financiers as Lloyds of Birmingham, a development that relieved the prospective shipper of uncertainty, as well as the loss of time involved in shopping around for insurance.

<sup>12</sup> Mathias, *Industrial Nation*, 93. For a more extended discussion of available capital, see Mathias, "Capital, Credit and Enterprise in the Industrial Revolution," *Journal of European Economic History*, 2 (1973): 121–44. Both Deane and Mathias also point out (*Industrial Revolution*, 24–35, and *Industrial Nation*, 190–95) that population increase was a concomitant rather than an initial cause of business change. For more detailed discussion see E. A. Wrigley, ed., *An Introduction to English Historical Demography from the Sixteenth to the Nineteenth Centuries* (London, 1966).

<sup>13</sup> Mathias, *Industrial Nation*, 170.

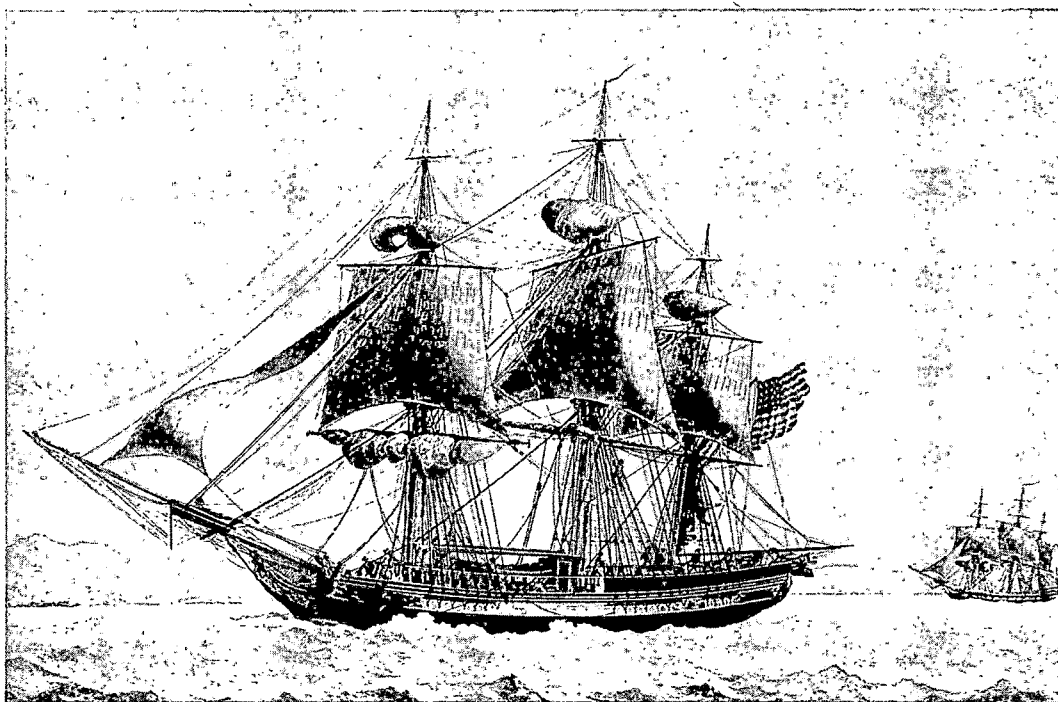


Fig. 1. The *Zulema*, a transatlantic merchant ship, originally owned by Daniel Mann of Philadelphia. Painting by Antoine Roux, 1801. Courtesy Historical Society of Pennsylvania.

Of all the developments created by increased demands on business, ship-building and inland transportation involved the largest amounts of capital, and while in the former operation the capital was to a large extent rotated from ship to ship, in canal and turnpike construction there was frequently little or no return for the long period it took to complete construction. Unlike the later American states the British government left the financing of these works to private enterprise, but Parliament helped by creating essentially modern chartered corporations for this purpose. From the mid-eighteenth century on, but particularly in the last quarter, Parliament chartered many of these transportation companies with limited liability and transferable shares. The latter gradually transformed the London Stock Exchange from a stodgy market for government bonds into an active resource for private capitalists.<sup>14</sup>

A charter for general purposes, however, was still very difficult to get through Parliament, and the Bubble Act of 1720 imposed discouraging limitations on nonchartered joint-stock companies. This difficulty in achieving flexibility of investment and limited liability for participants in varied private ventures was not overcome until the early nineteenth century, when

<sup>14</sup> *Ibid.*, 116–18, and W. H. B. Court, *A Concise Economic History of Britain* (Cambridge, 1964), 85.

lawyers found that partnerships, by acting as equitable trusts, could facilitate changes in membership and secure limitations of liability for contractual obligations.

By this time the basic external business structure necessary to manage the larger scale trade and production of a society using power machinery had emerged. Beyond increasing the rate of turnover or capital-profit ratio in commercial operations such changes in the social structure of any nation created new regional business communities and conditioned people and governments to the idea of institutional change. They created a lively atmosphere in which innovations were more welcome and tradition lost some of its hold. Enterprises appeared in the countryside, not dependent on ancient guild or licensing practices. Businessmen traveled more readily from city to city and thought in terms of broader markets. In England the nobility and gentry who ran the central government were only slightly involved, but one could classify their general attitude as benevolent in contrast to the distrust of, or aversion to, business change that existed on the Continent.

WHILE THE IMPERIAL YOKE rested lightly on the colonies, the fact that they were largely dependent on England for both commercial capital and approval of mercantile practices discouraged American innovation. Trade regulations were designed to keep the colonies as producers of raw material, which they would chiefly have been in any case, and while some types of manufacturing for export, such as finished iron products, were prohibited, it is doubtful if output could have expanded much in the face of high inland transportation costs and British competition. Continuous wars at sea, in which the colonists were necessarily belligerents, may also have been a retarding factor. The chief restraints on colonial American progress, however, seemed to have involved a lack of capital not tied to Britain, which prevented provincially financed improvements in transportation, while poor inland transportation made for locally isolated urban business communities and self-sufficient farmers with small purchasing power.

Independence presented business challenges and freed pent-up entrepreneurial energies that soon generated a Northeastern business community capable of winning state assistance for commercial development. While some business forms now appeared in America in more modern guises than in the United Kingdom, rapid development, as men like Alexander Hamilton could clearly see, was still held back by lack of capital. In 1790 and 1791 Hamilton and his supporters partially met this need through funding the national debt into bonds salable at home and abroad and chartering the First Bank of the United States. These two measures created over \$50 million worth of high-grade securities available as a base for further credit.

But in the long-run more capital was supplied fortuitously by wars in Europe, which lasted intermittently from 1793 to 1815. Soon the nation

became the major neutral ocean carrier. While piracy was a menace in certain areas, and raiders commissioned by warring governments often disregarded national flags, these risks greatly increased freight rates and profits for the successful. Some American merchants made fortunes of over a million dollars, profits unheard of from trade in the colonial period. By the time Jefferson's embargo of 1808 began a period of interference with ocean shipping, American mercantile houses, such as those of Brown, Girard, Astor, or Derby, had grown greatly in size, and specialization of functions had increased strikingly in trade, finance, and handicraft manufacture. Meanwhile, promotional policies by government had actively furthered these developments. Consequently, while theoretically separable, in the actual history the first two steps in the descriptive paradigm were so closely parallel that there is no sharp division in time between them.

Specialization was accompanied by far more systematic ways of conducting business. Of accounting, A. Dunsire writes: "The eighteenth century was an era of revolution in this field—as much as in methods of manufacture."<sup>15</sup> Depletion of capital, inputs, and outputs made their way into the old mercantile bookkeeping. Meanwhile, as the volume and complexity of trade increased, double entry and profit and loss accounts became the rule in large mercantile firms. In Philadelphia, in 1796, William Mitchell published the first American text on accounting, which was followed in 1800 by that of Thomas Turner in Portland, Maine.<sup>16</sup> By 1810 Robert Oliver of Baltimore maintained a system of accounts vastly more detailed and meticulous than had been used in earlier decades.<sup>17</sup>

A number of competing cities within the framework of the same national market appear to have been a strongly stimulating factor for business development. Four of the Northeastern states—Maryland, Pennsylvania, New York, and Massachusetts—felt their general welfare to be dependent on the promotion of their major seaports. Consequently, governments joined in the competition to attract capital and trade to a degree not duplicated in similar areas abroad. From independence until 1837, at least, the mercantile community of each of the major ports became increasingly afraid of having trade diverted to their rivals, and, consequently, state legislators became anxious to assist business development in all practical, and some impractical, ways. A common state device for mobilizing private capital and promoting business was the chartered corporation.

The states were willing to delegate wide powers to private business corporations, a move they justified by claiming corporate development would advance the general welfare. In one sense, from Roman times on, this had

<sup>15</sup> A. Dunsire, *Administration: The Word and the Science* (New York, 1973), 40.

<sup>16</sup> Roy J. Sampson, "American Accounting Education, Textbooks and Public Practice Prior to 1900," *Business History Review*, 34 (1960): 460-61.

<sup>17</sup> Stuart Bruchey, "Success and Failure Factors: American Merchants in Foreign Trade in the Eighteenth and Early Nineteenth Centuries," *Business History Review*, 32 (1958): 278-79.

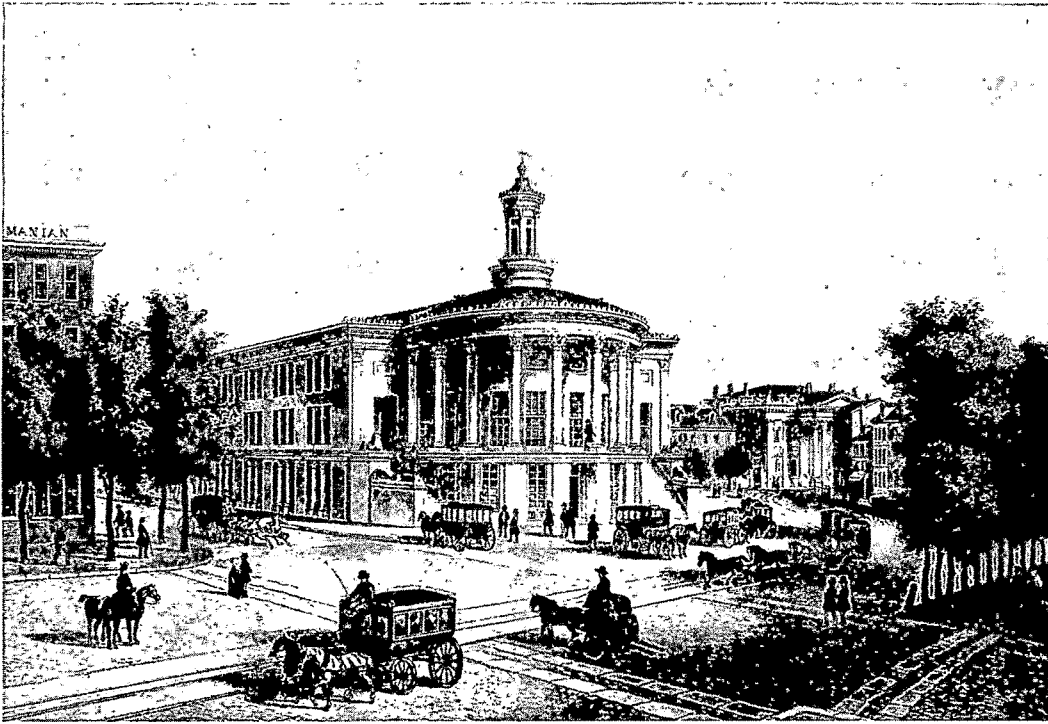


Fig. 2. The Merchant's Exchange, Philadelphia, was completed in 1834. Courtesy Historical Society of Pennsylvania.

always been the reason for such delegation of power. The American innovation was expansion of the definition of general welfare to include practically all local economic growth. While colonial governments had chartered political, educational, and religious corporations, they had maintained grave doubts about charters for business purposes. Only seven survived from before the Revolution.<sup>18</sup> In contrast the legislators of the young states took an enthusiastic view of corporations for banking, insurance, transportation, public works, and manufacturing, often subscribing substantial state funds for corporate stocks, and, in turn, selling state securities to the growing banks and insurance companies.

Prior to 1800 neither England nor France had satisfactory arrangements for incorporation, yet, all in all, the biggest advantage in this area accruing to business in the United States appears to have come from incorporation by competing state jurisdictions, rather than by a conseil d'état or national parliament.<sup>19</sup> The pressure for state development meant that legal adjust-

<sup>18</sup> Curtis P. Nettels, *The Emergence of a National Economy 1775-1815* (New York, 1962), 290. For a more specialized account of American business at this time see Elisha P. Douglass, *The Coming of Age of American Business* (Chapel Hill, 1971).

<sup>19</sup> See Judah Adelson, "The Early Evolution of Business Organization in France," *Business History Review*, 31 (1957): 226-45; Charles E. Freedman, "Joint Stock Business Organization in France, 1807-1867," *Business History Review*, 39 (1965): 187-91; Mathias, *Industrial Nation*, 33-38; and Joseph S. Davis, *Essays in the Earlier History of American Corporations*, 2 (Cambridge, Mass., 1917).



ments to new demands of business units selling securities to the public were made rather quickly in the United States and only slowly brought about in England and France.<sup>20</sup> In 1800, when neither Britain or France had more than about a score of the modern type of corporations, the United States, with only a small fraction of the population of the two European states, had incorporated over three hundred such private enterprises.<sup>21</sup>

The mature corporate form, as Europe was to learn, had a number of advantages over any partnership. A corporation could be given various monopoly rights; issue many different types of securities, representing either equity or debt; set face values on the securities low enough to encourage wide sale; enjoy eternal life, unless limited by its charter; lease other properties or lease itself to other owners; limit the liability of its stockholders to the assets of the company; operate in receivership when it could not meet its obligations; be transferred to the control of its creditors by bankruptcy; and separate managerial control from ownership. The early corporation was probably most important as a device for mobilizing the savings of many small businessmen. While one could not afford, for reason of liability, if no other, to join a partnership in a place too remote for careful supervision of its operations, the approval of the state, monopoly privileges in some cases, and the requirements for voting and record keeping, particularly, made it seem safe to buy a few shares in a bank or water or turnpike company located outside one's own locality.

The various possibilities of the corporations, however, were realized more by the continuous experiments of businessmen that resulted in litigation and judicial decision than through initial definition in charters. It took a generation, for example, to establish limited liability as the rule when there was no contrary provision in the charter, and the right to do business in other states, where not specifically prohibited, was only affirmed by the Supreme Court in 1839.<sup>22</sup> The possibilities of preferred stock and various types of bonds, together with corporate leasing, were not fully realized until a later age. The

<sup>20</sup> State and federal aid to economic growth are well summarized in Stuart Bruchey, *The Roots of American Economic Growth 1607-1861: An Essay on Social Causation* (London, 1965), 95-138. For more detailed studies of Northeastern states see John W. Cadman, Jr., *The Corporation in New Jersey: Business and Politics 1791-1875* (Cambridge, Mass., 1949), 33-37; Nathan Miller, *The Enterprise of a Free People: Aspects of Economic Development in New York State during the Canal Period, 1792-1838* (Ithaca, 1962), 14, 25-26; Oscar and Mary Handlin, *Commonwealth, A Study of the Role of Government in the American Economy: Massachusetts, 1774-1861* (New York, 1947), 113-43; and Louis Hartz, *Economic Policy and Democratic Thought: Pennsylvania, 1776-1860* (Cambridge, Mass., 1948), 37-103. The Handlins' and Hartz's volumes were planned by the Committee on Research in Economic History as parts of a systematic survey of business and government in sample states. The Cadman volume, started independently, fitted into the series. The Miller volume, also conceived independently on the particular subject of canals, was sponsored by the Beveridge Fund of the American Historical Association.

<sup>21</sup> Davis, *Corporations*, 2, 20 ff.

<sup>22</sup> See Eric Monkkonen, "Bank of Augusta v. Earle: Corporate Growth v. States' Rights," *Alabama Quarterly*, 34 (1972): 113-30.

rise of salaried corporate managers, owning little or no stock, was a gradual evolution from the 1820s on, as transportation, insurance, and banking companies grew larger.

Ironically, the first direct beneficiary of the early wave of incorporation was the hard-pressed government of the Confederation. Even before the war was over Philadelphians, in 1780, brought mercantile capital to the aid of the nation by chartering the Pennsylvania Bank, which they superseded a year later by the federally and state chartered Bank of North America.<sup>23</sup> In contrast to English and European banks, which had grown as family partnerships, incorporated American banks could raise their capital from the whole business community through the sale of stock and could issue notes bearing the stamp of state government approval. That the Bank of North America was quickly followed by dozens of others, particularly in the flush years from 1793 to 1808, demonstrated the attractiveness and business utility of chartered banks. Since working capital was the largest credit requirement of most early business, these numerous competing banks, each making short-term but renewable loans, were a strong aid to expansion and growth. Commenting on Britain, Mathias says: "The most important single development lay in the progressive efficiency and the expansion of conduits and institutions serving the short-term end of the money markets."<sup>24</sup> Much of what appeared as short-term lending on the ledgers of banks was, in fact, continuing and never-demanded investment in enterprises. Such enduring loans facilitated the investment of profits in the expansion of fixed capital. Another source of capital, largely through mortgages, came from the institutionalization of insurance. This had earlier become a specialized business in Philadelphia, and its rapid spread to other cities was hastened by use of the corporate form. By 1800 thirty-two marine insurance companies were facilitating the boom in ocean shipping. This rapid adoption of the corporation for a wide variety of enterprises in the United States within the course of a single generation must be regarded as one of the most important developments of the Business Revolution.

The division of tasks in business or office operations has probably been as important as division of labor in manufacturing plants for raising levels of efficiency. Wholesalers, for example, became more departmentalized within their offices, as well as more specialized. Some dealt in foreign imports that were passed on to dealers whose chief connections were with the back country, others specialized in facilitating the Southern cotton trade, a few concentrated on still more limited activities, such as auctioneering, while many became various types of commission merchants.<sup>25</sup>

<sup>23</sup> For details of this complicated history see F. Cyril James, "The Bank of North America and the Financial History of Philadelphia," *Pennsylvania Magazine of History and Biography*, 64 (1940): 56-96.

<sup>24</sup> Mathias, "Capital, Credit and Enterprise," 129.

<sup>25</sup> Bruchey, "Success and Failure Factors," 280-84.

Because of the profits from the 1793–1808 boom the exporting and importing merchants became the people to approach for all risks that would not be financed by a bank, and even when they were so financed, the local merchants had almost invariably subscribed the capital for the bank. Wholesalers financed the great upswing in handicraft production, as well as the beginnings of mechanized textile manufacture; they provided the goods and buildings needed to establish new retailers in both the city and its surrounding back country; they invested in construction and real estate enterprises for both business and residential use, putting the profits into new facilities for trade and manufacture; they financed the rapidly growing cotton trade; and at all times they invested heavily in improvements in transportation.<sup>26</sup>

Increasing capital flows and the creation of auxiliary businesses were also stimulated by the trade boom of the 1790s. Issues of government, bank, insurance, and transportation company stocks or bonds led some erstwhile merchants to specialize in security brokerage. By the middle of the decade brokers met at fixed locations for trading in both New York and Philadelphia and formed exchanges. As time went on some merchants, like the Browns of Baltimore, came to specialize in foreign exchange and securities, while others, like Stephen Girard of Philadelphia, became private bankers prepared to undertake the initial distribution of securities. By the 1830s note brokers, various mercantile associations, and specialized law firms added to the array of institutions involved in the East Coast flow of information, capital, and credit.

By 1800 the signs of rapid change were obvious in all the big Northern seaports. Business information was being supplied by a score of urban dailies primarily devoted to commercial news and advertising. Printing and publishing became not only one of the principal industries in each large city, but the very nervous system of business. Building, representing the largest source of urban employment, was going on everywhere, and profits from construction were being put into expanding ocean and inland trade. Handicraft shops were frequently outgrowing their confines. In order to provide more space in the now crowded business sections the craftsmens' families were moved elsewhere and the erstwhile homes converted into a mixture of shops for producing and selling.

Rapid commercial development in this period before 1840 was largely confined to the Northeast coast. In 1790 a hundred-mile-wide coastal strip from Maryland to southern Maine had a population of about 2.5 million people, with under 5 per cent living in cities of over 10,000; by 1820 the population had doubled and by 1840 tripled, while, during the same period, the number of people living in cities of over 10,000 rose to 10 and 15 per

<sup>26</sup> See Glenn Porter and Robert Livesay, *Merchants and Manufacturers: Studies in the Changing Structure of Nineteenth Century Marketing* (Baltimore, 1971), for a general discussion of the wholesaler. As Mathias notes, most capital investment was (and continued to be) in buildings not in production goods in the form of machines. "Capital, Credit and Enterprise," 123.

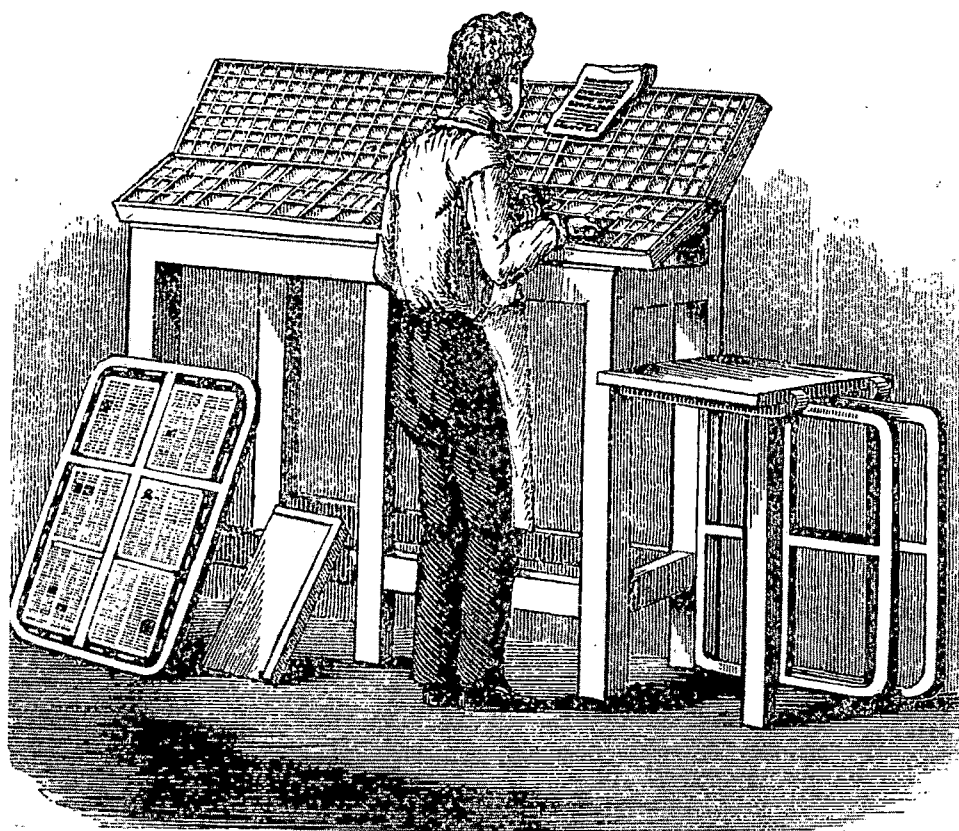


Fig. 3. A printer at his trade. Engraving by C. T. Hinckley from *Godey's Lady's Book*, Philadelphia, 1852. Courtesy Historical Society of Pennsylvania.

cent respectively. Thus the total major city population rose from 125,000 to roughly 1,000,000. This area in which the Business Revolution occurred can be treated, therefore, as a generating center of activity separated from the Southern coast and the trans-Appalachian interior by the high cost of transportation and the time intervals that more than equaled those between the nations of Europe. Along this fertile and accessible coastal plain there was a rapidly growing agricultural population, within reach of markets, that sent people looking for work, as well as food, to the growing cities. Thus, the favorable conditions present in England for the distribution of produce were to some extent duplicated; by contrast, the northern coastal plain of France suffered from overconcentration of business energies in Paris, with a lack of other large urban centers, and Germany and the Low Countries lacked such a coastal plain under a single central government.

HAVING BRIEFLY DESCRIBED, first, the specialization of business and labor activities brought about by a massive increase in trade and, second, some of the importance of government aid, I will now turn to improvements in

transportation and communication, in which government investment in public works and improved mail service also promoted mercantile enterprise.

In 1790 an isoline indicating the distance of five-days' travel from Philadelphia would extend southward only to the head of the Chesapeake Bay, westward to Lancaster, Pennsylvania, and northward to New York and Brooklyn. In 1817 the same line included Richmond, Virginia, mid-Pennsylvania cities, and New London, Connecticut. By this time, however, a similar isoline, centered on New York, stretched from Norfolk, Virginia, to Portsmouth, New Hampshire, and westward nearly to Lake Ontario.<sup>27</sup> These simple measures not only help explain the Business Revolution, but also tell the story of the victory of New York over Philadelphia in the great upsurge of trade: from the port of New York one could reach more places in the United States more quickly than from Philadelphia, thereby making New York the best distributing point, at which merchants could turn over their inventories more rapidly. Quicker transport was one of the major factors in reducing the very large amount of working capital required by early enterprise.<sup>28</sup>

Along the coastwise axis, improvement in the time of travel up to 1820 was primarily a matter of better highways. In 1818 the statistical analyst Adam Seybert said that the great increase in the number and length of post roads "demonstrates the rapid improvement of our country."<sup>29</sup> The economic importance of the reduction in time was due to the fact that more than three-quarters of communication over the turnpike was on business matters, whether in the form of commercial travelers, newspapers, or mail. Newspapers devoted 75 to 90 per cent of their space to business concerns, and few besides businessmen would send letters at twenty-five cents to a dollar a page. Thus information flows and the speed and efficiency of business were intimately related. It must be remembered in this connection that the buyer had to wait for the transmission of an order for goods as well as for the merchandise to be sent back. The decrease in total time necessary to receive and fill orders led to a much more rapid turnover of inventory, or in other words more profit from the same quantity of working capital. In 1790, for example, it took a minimum of about two weeks to transmit an order between Boston and Philadelphia; by 1836 the mail moved regularly between the two points in thirty-six hours.<sup>30</sup>

Before 1835 the forms of transportation mechanized by steam had only a slight effect on the carriage of mails (including newspapers). Total postal carrier movements in that year were 16.9 million miles by stage, 7.8 million on horseback, under 1 million by steamboat, and about a third of a million by train. At that time railroads alone could not carry mail between any of the major cities.<sup>31</sup>

<sup>27</sup> Pred, *Urban Growth*, 37, 44, 45.

<sup>28</sup> Mathias, "Capital, Credit and Enterprise," 127.

<sup>29</sup> *Statistical Annals* (Philadelphia, 1818), 374.

<sup>30</sup> Pred, *Urban Growth*, 86.

<sup>31</sup> *Ibid.*, 85.

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Fig. 4. Advertisement for the New York stage, from the *U.S. Gazette*, Apr. 27, 1819. Courtesy Historical Society of Pennsylvania.

For heavy goods the completion of canal systems along the urban axis was highly important. Even in 1840 the necessity of frequent transfers of freight limited carriage of bulky or heavy goods on railroads to metropolitan regions. The Erie Canal, completed in 1825, the Union (Philadelphia to the Susquehanna) 1828, the Chesapeake and Delaware, 1830, and the Delaware and Raritan, 1834, provided protected water connections from New London to Baltimore and inland to Buffalo and the cities in the Susquehanna Valley. While in theory travel to the West, either by canal and rail from Philadelphia, or by river and canal from New York, was now possible, its volume remained small. In 1835 more than four-fifths of all freight revenue on the Erie Canal was from goods shipped within New York State.<sup>32</sup> Hence, even by the time the panic of 1837 temporarily checked rapid development the new high levels of business activity were still confined to the Northeast coast.

All these elements of faster transportation, more rapid capital turnover, and the resulting increase in the tempo of business activity led to the operation of the final, and ultimately most important, factor in the model: entrepreneurial decisions based on better information, including, particularly,

<sup>32</sup> Ronald W. Filante, "A Note on the Economic Viability of the Erie Canal, 1825-1860," *Business History Review*, 48 (1974): 96.

more up-to-date knowledge of the state of the market. For overseas information the daily newspapers were meeting fast sailing packets outside the harbors and rushing foreign news to press by racing schooners, while overland mail between major centers was being speeded by relays of horses. It is not hard to imagine the difference in tempo and business alternatives that came about during this period.<sup>33</sup>

In addition to these well-documented feedbacks from trade and transportation, differentiation of all kinds, encouraged by big urban centers, such as New York and Philadelphia, with hundreds of thousands of people, led specialists to congregate, talk, and find better ways of doing things. While patent records are unreliable for the period prior to 1837, the four big East Coast centers appear to have had per capita rates of application up to eight or nine times as high as the rest of the nation.<sup>34</sup> Large-scale urbanization also meant that the capitalist could work with better newspapers, more able lawyers, and more astute financial advisers, and was better able to externalize risks through use of outside agencies, access to private or commercial bankers, quick transfer of inventory to dealers, or tighter control over suppliers.

Although along the East Coast these factors worked to build the major ports into bigger and bigger centers of domestic trade there were some limitations on their growth and prosperity. Since none of them had fast flowing streams within the city, mills and factories using water power were located elsewhere. To some extent this would also have been dictated by higher urban wages. Thus a fringe of factory industry came, particularly after 1825, to surround the major centers at distances of ten to thirty miles.

Distance, however, was a simpler problem than adequate management. Entrepreneurs needed not only information, but the ability to put it to use, and here again the mercantile community was of primary importance. Management of the industries using power machinery, which by 1840 involved large factories only in the case of textiles, became a part of the specialization of mercantile functions. From 1792 on, following the pioneer efforts of Almy and Brown of Providence, the successful enterprises were based on mercantile experience in finance, supply, and marketing, leaving only plant supervision in the hands of technicians. By 1809 Almy and Brown alone, through aggressive interurban marketing, had underwritten the success of a majority of the operative New England spinning mills.<sup>35</sup> Thus, in the first three decades of the nineteenth century, merchants had organized a modern textile industry with large plants and adequate marketing. This represented an especially important example of alert, experienced general entrepreneurs, with access to working capital, quickly taking advantage of business opportunities to use new technology. The same essential pattern was followed in

<sup>33</sup> See Robert G. Albion, *The Rise of New York Port, 1815-1860* (New York, 1939).

<sup>34</sup> Fred, *Urban Growth*, 268.

<sup>35</sup> Douglass, *American Business*, 88.

steam engines, hardware, and a few minor industries that were partially mechanized in 1840. By then the Business Revolution had taken place in the United States. The nation now had a commercial structure capable of adopting machine technology, whether originating locally or abroad, as fast as it could be developed. American businessmen, as attested to by both natives and foreigners, were particularly attuned to machine processes and were prepared both to innovate in and to manage mass production. In all, no matter what the level of value in dollars added by mechanized industry in 1840, the necessary social and structural changes associated with modern industrialism had occurred and, with twenty years of stable government, expansion would follow as a matter of course. In actuality the census figures show that the decade 1839 to 1849 marked the most rapid rate of increase in "value added by manufacture" in American history.

WHILE THIS BRIEF ACCOUNT of the Business Revolution has dealt chiefly with the United States, the logic of the development in America and Britain inspires confidence that further research will reveal much the same patterns in the nations that were slower to develop. The American experience was especially rapid because of a social structure permeated by a nearly universal interest in business, because of federal and state governments staffed by men interested in economic development, because of immigrants who were perhaps more likely than immobile people to innovate, because of a group of entrepreneurs sharpened by the experiences of the Revolutionary War and new national opportunities, and, finally, because of a shared language and trade connections with the world's business leader. The experience also illustrates that the ability to produce goods is only a part of the creation of social utility. That such favorable factors need not always be present in the same form or proportions is indicated by the much later case of Japan, yet there, as in America, it was the national culture and business structure, rather than resources or technology, that initiated the rapid advance.<sup>36</sup>

This line of reasoning has a host of corollaries. If economic growth lags at a later stage in development, for example, the governmental or business systems should be assumed, until proven otherwise, to be at fault.<sup>37</sup> If, on the other hand, business in a nation is able to elicit valuable help from government, this social-structural element may well be far more important than deposits of iron ore. Similarly, the ability to interpret correctly the large masses of information that flow to management in the twentieth century may be more important than economies of scale. One could go on at length with such a list, but the conclusion should now be clear: the pervasive values,

<sup>36</sup> Ohkawa and Rosovsky, *Japanese Economic Growth*, 217-32.

<sup>37</sup> See Martin Wolfe, "French Interwar Stagnation Period," in Charles K. Warner, ed., *From the Ancient Regime to the Popular Front—Essays in Honor of Shepard B. Clough* (New York, 1969), 159-80.



interests, and attitudes of a society and the business structure resulting therefrom constitute causal forces that shape the nature of both executive action and the markets in which companies operate.

The proposition that social structure and cultural values underlie economic change has been widely accepted in principle by leading economists, but it has too frequently been regarded as a given from the standpoint of theory.<sup>38</sup> Almost equally well accepted is the hypothesis that technological change or invention accelerates in response to effective demand. What I have tried to illustrate by the early American experience is that these factors are not only the given, or platform from which economic growth takes off, but that by shaping and reshaping business forms and relationships they are continuously active forces in daily economic operations and in the longer-run processes of economic growth. This conclusion has far-reaching consequences. If structure and culture are, in fact, major influences on the behavior of men dealing in markets, or making decisions involving attempts at marginal balancing of inputs and the adoption of new machines or processes, such factors must be given a greater weight in most of the propositions of economic growth theory, as well as in economic history.<sup>39</sup> It must not be forgotten that the relations between business forms and practices and the market are reciprocal.

Yet, so far, the force of these sociocultural considerations is not subject to quantification and can only be observed empirically in situations that always involve some differing variables. Consequently, history interpreted on the basis of the still highly general propositions of social theory has to serve as the guide to major economic change. History, or, in this particular case, business history, is not, therefore, a branch of applied economic theory, but rather an autonomous social discipline.

<sup>38</sup> For long standing examples see Albert O. Hirschman, *The Strategy of Economic Development* (New Haven, 1958), 9; James S. Buchanan and Howard S. Ellis, *Approaches to Economic Development* (New York, 1955), 406; or Simon Kuznets, *Modern Economic Growth: Rate Structure and Spread* (New Haven, 1966), 491.

<sup>39</sup> Culture is used here in the anthropological sense of a series of implied understandings as to what constitutes normal thought, attitudes, and behavior.

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## What Ought To Be and What Was: Women's Sexuality in the Nineteenth Century

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CARL N. DEGLER

AS EVERY SCHOOLGIRL KNOWS, the nineteenth century was afraid of sex, particularly when it manifested itself in women. Captain Marryat, in his travels in the United States, told of some American women so refined that they objected to the word "leg," preferring instead the more decorous "limb." Marryat also reported seeing this delicacy carried to extremes in a girls' school where a school mistress, in the interest of protecting the modesty of her charges, had dressed all four "limbs" of the piano "in modest little trousers with frills at the bottom of them!"<sup>1</sup> Women's alleged lack of passion was epitomized, too, in the story of the English mother who was asked by her daughter before her marriage how she ought to behave on her wedding night. "Lie still and think of the Empire," the mother advised.

This view of Victorian attitudes toward sexuality is captured in more than stories. Steven Marcus, writing about the attitudes of English Victorians toward sexuality, and Nathan Hale, Jr., summarizing the attitudes of Americans on the same subject, both quote at length from Dr. William Acton's *Functions and Disorders of the Reproductive Organs*, which went through several editions in England and the United States during the middle years of the nineteenth century.<sup>2</sup> Acton's book was undoubtedly one of the most widely quoted sexual-advice books in the English-speaking world. The book summed up the medical literature on women's sexuality by saying that "the majority of women (happily for them) are not very much troubled with sexual feelings of any kind. What men are habitually, women are only ex-

<sup>1</sup> Captain Frederick Marryat, *A Diary in America, with Remarks on Its Institutions* (London, 1839), 2: 244-47. The story of the trousers on piano legs is taken seriously in John Duffy, "Masturbation and Clitoridectomy: A Nineteenth Century View," *Journal of the American Medical Association*, 186 (1963): 246; G. Rattray Taylor, *Sex in History* (New York, 1954), 203; and Peter T. Cominus, "Innocent Femina Sensualis in Unconscious Conflict," in Martha Vicinus, ed., *Suffer and Be Still: Women in the Victorian Age* (Bloomington, 1972), 157.

<sup>2</sup> William Acton, *The Functions and Disorders of the Reproductive Organs in Youth, in Adult Age, and in Advanced Life: Considered in Their Physiological, Social, and Psychological Relations* (1857; 2d ed., London, 1858; expanded American ed., Philadelphia, 1865). For references to Acton's writings, see Steven Marcus, *The Other Victorians: A Study of Sexuality and Pornography in Mid-Nineteenth-Century England* (New York, 1966), ch. 1; and Nathan G. Hale, Jr., *Freud and the Americans: The Beginnings of Psychoanalysis in the United States, 1876-1917* (New York, 1971), 36-37.

ceptionally."<sup>3</sup> Theophilus Parvin, an American doctor, told his medical class in 1883, "I do not believe one bride in a hundred, of delicate, educated, sensitive women, accepts matrimony from any desire for sexual gratification; when she thinks of this at all, it is with shrinking, or even with horror, rather than with desire."<sup>4</sup>

Modern writers on the sexual life of women in the nineteenth century have echoed these contemporary descriptions. "For the sexual act was associated by many wives only with a duty," writes Walter Houghton, "and by most husbands with a necessary if pleasurable yielding to one's baser nature; by few, therefore, with any innocent and joyful experience."<sup>5</sup> Writing about late-nineteenth-century America, David Kennedy quotes approvingly from Viola Klein when she writes that "in the whole Western world during the nineteenth century and at the beginning of the twentieth century it would have been not only scandalous to admit the existence of a strong sex urge in women, but it would have been contrary to all observation."<sup>6</sup> Nathan Hale, Jr. sums up his review of the sexual-advice literature at the turn of the century with a similar conclusion: "Many women came to regard marriage as little better than legalized prostitution. Sexual passion became associated almost exclusively with the male, with prostitutes, and women of the lower classes."<sup>7</sup> Most recently Ben Barker-Benfield has argued that male doctors were so convinced that women had no sexual interest that when it manifested itself drastic measures were taken to subdue it, including excision of the sexual organs. "Defining the absence of sexual desire in women as normal, doctors came to see its presence as disease. . . . Sexual appetite was a male quality (to be properly channelled of course). If a woman showed it, she resembled a man."<sup>8</sup>

<sup>3</sup> Acton, *Functions and Disorders* (1865), 133.

<sup>4</sup> Theophilus Parvin, "Hygiene of the Sexual Functions," *New Orleans Medical and Surgical Journal*, n.s. 11 (1883-84): 607. Parvin also quotes at length from Acton's book.

<sup>5</sup> Walter E. Houghton, *The Victorian Frame of Mind, 1830-1870* (New Haven, 1957), 353. Marcus presents a portrait of Victorian attitudes toward sex similar to that of Houghton, but he disclaims to be talking about behavior: "We need not pause to discuss the degree of truth or falsehood in these assertions. What is of more immediate concern is that these assertions indicate a system of beliefs." *Other Victorians*, 32. Yet it is not clear what point there is in detailing a system of beliefs unless it has some behavioral consequences. Peter T. Cominos also relies upon Acton, in "Late Victorian Sexual Respectability and the Social System," *International Review of Social History*, 8 (1963): 18-48, 217-50. E. M. Sigsworth and T. J. Wyke doubt the pervasiveness in Victorian England of Acton's conception of women's sexuality. They write: "Victorian opinion on the innate sexuality of women was cloudy and divided"—a view about which more will be said in this article. "A Study of Victorian Prostitution and Venereal Disease," in Vicinus, *Suffer and Be Still*, 83.

<sup>6</sup> Viola Klein, *The Feminine Character: History of an Ideology* (1946; reprint, Urbana, 1972), 85, as quoted in David M. Kennedy, *Birth Control in America: The Career of Margaret Sanger* (New Haven, 1970), 56-57.

<sup>7</sup> Hale, *Freud and the Americans*, 31. Elsewhere Hale sums up the medical view as he sees it: "By 1906 . . . some physicians regarded the asexual female as the norm: 'It may be offered that the sexual appetite in the majority of American females is evoked only by the purest love. In many the appetite never asserts itself and, indeed, the only impulse thereto is in the desire to gratify the object of affection'" (pp. 39-40; quotation from Ferdinand C. Valentine, "Education in Sexual Subjects," *New York Medical Journal*, Feb. 10, 1906, p. 276).

<sup>8</sup> Ben Barker-Benfield, "The Spermatic Economy: A Nineteenth Century View of Sexuality," *Feminist Studies*, 1 (1972): 54.

Despite the apparent agreement between the nineteenth-century medical writers and modern students of the period, it is far from clear that there was in the nineteenth century a consensus on the subject of women's sexuality or that women were in fact inhibited from acknowledging their sexual feelings. In examining these two issues I shall be concerned with an admittedly limited yet significant population, namely, women of the urban middle class in the United States. This was the class to which the popular medical-advice books, of which William Acton's volume was a prime example, were directed. It is principally the women of this class upon whom historians' generalizations about women's lives in the nineteenth century are based. And though these women were not a numerical majority of the sex, they undoubtedly set the tone and provided the models for most women. The sources drawn upon are principally the popular and professional medical literature concerned with women and a hitherto undiscovered survey of married women's sexual attitudes and practices that was begun in the 1890s by Dr. Clelia D. Mosher.

LET ME BEGIN with the first question or issue. Was William Acton representative of medical writers when he contended that women were essentially without sexual passion? Rather serious doubts arise as soon as one looks into the medical literature, popular as well as professional, where it was recognized that the sex drive was so strong in woman that to deny it might well compromise her health. Dr. Charles Taylor, writing in 1882, said, "It is not a matter of indifference whether a woman live [*sic*] a single or a married life. . . . I do not for one moment wish to be understood as believing that an unmarried woman cannot exist in perfect health for I know she can. But the point is, that *she must take pains for it*." For if the generative organs are not used, then "some other demand for the unemployed functions, must be established. Accumulated force must find an outlet, or disturbance first and weakness ultimately results." His recommendation was muscular exercise and education for usefulness. He also described cases of women who had denied their sexuality and even experienced orgasms without knowing it. Some women, he added, ended up, as a result, with impairment of movement or other physical symptoms.<sup>9</sup>

Other writers on medical matters were even more direct in testifying to the presence of sexual feelings in women. "Passion absolutely necessary in woman," wrote Orson S. Fowler, the phrenologist, in 1870. "Amativeness is created in the female head as universally as in the male. . . . That female passion exists, is as obvious as that the sun shines," he wrote. Without woman's passion, he contended, a fulfilled love could not occur.<sup>10</sup> Both sexes enjoy the

<sup>9</sup> Charles Fayette Taylor, "Effect on Women of Imperfect Hygiene of the Sexual Function," *American Journal of Obstetrics*, 15 (1882): 175-76, 168-71, italics in original.

<sup>10</sup> Orson S. Fowler, *Sexual Science; Including Manhood, Womanhood, and Their Mutual Interrelations, etc. . . . as Taught by Phrenology* (Philadelphia, 1870), 680.

sexual embrace, asserted Henry Chevasse, another popular medical writer, in 1871, but among human beings, as among the animals in general, he continued, "the male is more ardent and fierce, and . . . the desires of the female never reach that high [sic] as to impel her to the commission of crime." Woman's pleasure, though it may be "less acute," is longer lasting than man's, Chevasse said. R. T. Trall, also a popular medical writer, counseled in a similar vein. "Whatever may be the object of sexual intercourse," he wrote, "whether intended as a love embrace merely, or as a generative act, it is very clear that it should be as pleasurable as possible to *both parties*."<sup>11</sup>

If one can judge the popularity of a guide for women by the number of its editions, then Dr. George Napheys's *The Physical Life of Woman: Advice to the Maiden, Wife, and Mother* (1869) must have been one of the leaders. Within two weeks of publication it went into a second printing, and within two years 60,000 copies were in print. Napheys was a well-known Philadelphia physician. Women, he wrote, quoting an unnamed "distinguished medical writer," are divided into three classes. The first consists of those who have no sexual feelings, and it is the smallest group. The second is larger and is comprised of those who have "strong passion." The third is made up of "the vast majority of women, in whom the sexual appetite is as moderate as all other appetites." He went on to make his point quite clear. "It is a false notion and contrary to nature that this passion in a woman is a derogation to her sex. The science of physiology indicates most clearly its propriety and dignity." He then proceeded to denounce those wives who "plume themselves on their repugnance or their distaste for their conjugal obligations." Napheys also contended that authorities agree that "conception is more assured when the two individuals who co-operate in it participate at the same time in the transports of which it is the fruit." Napheys probably had no sound reason for this point, but the accuracy of his statement is immaterial. What is of moment is that as an adviser to women he was clearly convinced that women possessed sexual feelings, which ought to be cultivated rather than suppressed. Concerning sexual relations during pregnancy he wrote, "There is no reason why passions should not be gratified in moderation and with caution during the whole period of pregnancy." And since his book is directed to women, there is no question that the passion he is talking about here is that of women.<sup>12</sup>

In 1878 Dr. Ely Van de Warker of Syracuse, a fellow of the American Gynecological Society, described sexual passion in women as "the analogue of the subjective copulative sensations of man, and that the acme of the sexual orgasm in woman is the sensory equivalent of emission in man, observ-

<sup>11</sup> P. Henry Chevasse, *Physical Life of Man and Woman: or, Advice to Both Sexes* (1866; reprint, New York, 1897), 291-92; quotation from Trall in Michael Gordon, "From an Unfortunate Necessity to a Cult of Mutual Orgasm: Sex in American Marital Education Literature, 1830-1940," in James M. Henslin, ed., *Studies in the Sociology of Sex* (New York, 1971), 58, my italics.

<sup>12</sup> George H. Napheys, *The Physical Life of Woman: Advice to the Maiden, Wife, and Mother* (1869; Philadelphia, 1871), 74-75, 180.

ing the distinction necessarily implied between the sexes—that in woman it is psychic and subjective, and that in man it has also a physical element and is objective,” that is, it is accompanied by seminal emission. The principal purpose of Van de Warker’s article was to deplore the fact that some women lacked sexual feeling, a state which he called “female impotency.”<sup>13</sup> What is striking about his article is that he obviously considered such lack of feeling in women abnormal and worthy of medical attention, just as impotency in a man would cause medical concern.

Van de Warker’s remarks, as well as his use of the word, make it evident that physicians were well aware that normal women experienced orgasms. Lest there be any doubt that their meaning of the word was the same as ours today, let me quote from a physician in 1883 who described in some detail woman’s sexual response. He began by describing the preparatory stage, which, he said,

may be reached by any means, bodily or mental, which, in the opposite sex, cause erection. Following upon this, then, is a stage of pleasurable excitement, gradually increasing and culminating in an acme of excitement, which may be called the stage of consummation, and the analogue of which in the male is emission. This is followed in both sexes by a degree of nervous prostration, less marked, however, in the female, and . . . by a relief to the general congestion of all the genital organs which has existed, and perhaps increased, from the beginning of the preparatory stage.<sup>14</sup>

All of this evidence, it seems to me, shows that there was a significant body of opinion and information quite different from that advanced on women’s sexuality by William Acton and others of his outlook.—Now it might be asked how widespread was this counter-Acton point of view? Was it not confined primarily to physicians writing for other physicians? Not at all. Napheys, Chevasse, and Fowler, to name three, were all writing their books for the large lay public that was interested in sexual matters. As we have seen, many of these marriage manuals, particularly Napheys’s and Fowler’s, were printed in several large editions.

Yet, in the end, there is a certain undeniable inconclusiveness in simply raising up one collection of writers against another, even if their existence does make the issue an open one, rather than the closed one that so many secondary writers have made it. It suggests, at the very least, that there was a sharp difference of medical opinion, rather than a consensus, on the nature of women’s sexual feelings and needs. In fact there is some reason to believe, as we shall see, that the so-called Victorian conception of women’s sexuality was more that of an ideology seeking to be established than the prevalent view or practice of even middle-class women, especially as there is a substantial amount of nineteenth-century writing about women that assumes

<sup>13</sup> Ely Van de Warker, “Impotency in Women,” *American Journal of Obstetrics*, 11 (1878): 47.

<sup>14</sup> J. Milne Chapman, “On Masturbation as an Etiological Factor in the Production of Gynec Diseases,” *American Journal of Obstetrics*, 16 (1883): 454.

the existence of strong sexual feelings in women. One of the historian's recognized difficulties in showing, through quotations from writers who assert a particular outlook, that a social attitude prevailed in the past is that one always wonders how representative and how self-serving the examples or quotations are. This is especially true in this case where medical opinion can be found on both sides of the question. When writers, however, assume the attitude in question to be prevalent while they are intent upon writing about something else, then one is not so dependent upon the tyranny of numbers in quoting from sources. For behind the assumption of prevalence lie many examples, so to speak. Such testimony, moreover, is unintended and therefore not self-serving. This kind of evidence, furthermore, helps us to answer the second question—to what extent were women in the nineteenth century inhibited from expressing their sexual feelings? For in assuming that women had sexual feelings, these writers are offering clear, if unintended, testimony to women's sexuality.

Medical writers like Acton may have asserted that women did not possess sexual feelings, but there were many doctors who clearly assumed not only that such feelings existed but that the repression of them caused illness. One medical man, for example, writing in 1877, traced a cause of insanity in women to the onset of sexuality. "Sexual development initiates new and extraordinary physical changes," he pointed out. "The erotic and sexual impulse is awakened."<sup>15</sup> Another, writing ten years later, asserted that some of women's illnesses were due to a denial of sexual satisfaction. "Females feel often that they are not appreciated," wrote Dr. William McLaury in a medical journal, "that they have no one to confide in; then they become morose, angular, and disagreeable as a result of continual disappointment to their social and sexual longings. Even those married may become the victims of sexual starvation when the parties are mentally, magnetically, and physically antagonistic."<sup>16</sup> Henry Chevasse, writing for a popular audience, was also impressed by the need for sexual outlets for women. There may be some individuals "of phlegmatic temperament," he conceded, who are not injured by celibacy, but "absolute continence in the sanguine and ardent disposition predisposes to the gravest maladies." His listing of the resulting maladies, of which nymphomania was one, makes it clear that he was referring to women as well as men. These maladies, he went on, "are born as well of extreme restraint as of extreme excess. . . . Females seem to suffer even more than males . . . perhaps because their continence is more complete." (Presumably he was referring here to the absence of nocturnal emissions in women.) As a result, he continued, nunneries were notorious as places of fanaticism. "Hence the old proverb, 'The convent and the confessional are the cradles of hysteria and nymphomania.'"<sup>17</sup>

<sup>15</sup> Montrose S. Pallen, "Some Suggestions with Regard to the Insanities of Females," *American Journal of Obstetrics*, 10 (1877): 209.

<sup>16</sup> William M. McLaury, "Remarks on the Relation of the Menstruation to the Sexual Functions," *American Journal of Obstetrics*, 20 (1887): 161.

<sup>17</sup> Chevasse, *Physical Life of Man and Woman*, 372-73.

To Dr. Van de Warker women's sexuality was so obvious that he assumed men required it in order to achieve full sexual satisfaction for themselves. In marriage, he wrote, the husband

not only demands pleasure and satisfaction for himself, but he requires something much more difficult to give—the appearance, if not the real existence, of satisfaction and pleasure in the object of his attentions. Unhappiness and suspicion are often the result of the absence of this pleasure [in women], and are sure to work to the material disadvantage of the weaker party. To show that this is really the case, I need but to remind physicians how often they are approached by husbands upon this subject; yet further, how often the coldness and indifference of wives are alleged as the excuse for conjugal infidelity.<sup>18</sup>

What is striking in this passage is that husbands complained to doctors about their wives' coldness, a fact that makes it quite evident that passion in wives was not only desired by men, but expected—why, otherwise, would they complain of its lack? Van de Warker, it is worth pointing out, was writing for his fellow physicians, who were in a position to verify his assertions from their own experience with patients.

Van de Warker's explanation for "impotency" in women is revealing, too. Ascribing it to "sexual incompatibility," he went on to say that "so far as my own observation extends, the husband is generally at fault. The more common cause is acute sexual irritability on the part of the husband."<sup>19</sup> Dr. William Goodell, writing in 1887, also asserted that mutual pleasure was essential to successful marital intercourse. In Goodell's mind, as in Van de Warker's, that meant men must recognize women's interests and sexual rhythm. "Destroy the reciprocity of the union," Goodell cautioned, "and marriage is no longer an equal partnership, but a sensual usurpation on the one side and a loathing submission on the other."<sup>20</sup> Another medical writer who also acknowledged women's pleasure in the sex act made the same point as Goodell and Van de Warker. Men must not force themselves upon women or "overpersuade, but await the wife's invitation at this time [during ovulation], when her husband is a hero in her eyes." In this way the husband "would enjoy more and suffer less," the physician predicted.<sup>21</sup> These writers, in short, were not only testifying to their knowledge that women possessed sexual feelings, they were also explaining how those feelings were sometimes denied legitimate satisfaction by inept husbands.

The assumption that women had sexual feelings which required satisfaction also comes through in the course of discussions about contraception. Generally, physicians and other writers on this subject in the nineteenth century strongly opposed contraception, though all recognized that it was

<sup>18</sup> Van de Warker, "Impotency in Women," 38–39.

<sup>19</sup> *Ibid.*, 41. Today the complaint is called premature ejaculation.

<sup>20</sup> William Goodell, *Lessons in Gynecology* (Philadelphia, 1887), 567, as quoted in Hale, *Freud and the Americans*, 40.

<sup>21</sup> McLaury, "Remarks on the Relation of the Menstruation," 161.



widely practiced. One of the methods in common use was *coitus interruptus*, or withdrawal by the male prior to ejaculation. This method was condemned for a variety of reasons, but for our purposes it is significant that among the objections was its harmful effects upon women. This method, wrote Henry Chevasse, is "attended with disastrous consequences, most particularly to the female, whose nervous system suffers from ungratified excitement."<sup>22</sup> Dr. John Harvey Kellogg, a popular writer on medical matters, also warned against the method because of its effects upon women. He quoted at length from a French authority. Whenever this method is practiced, the authority wrote, all of women's genital organs "enter into a state of orgasm, a storm which is not appeased by the natural crisis; a nervous super excitation persists" after the act. The authority then compared the unreleased tension to that evoked in presenting food to a "famished man" and then snatching it away. "The sensibilities of the womb and the entire reproductive system are teased to no purpose." It is evident that in the minds of both writers women were assumed to have sexual feelings that were normally aroused during sexual intercourse.<sup>23</sup> Dr. August Gardner, writing in 1870 also for a popular audience, quoted from the same French authority and for the same purpose as Kellogg.<sup>24</sup>

Anyone who has looked into the sexual history of the nineteenth century is immediately struck by the deep and anxious concern physicians as well as other people felt about masturbation. Although it is often thought that boys were the principal objects of that concern, the fact is that girls were just as much fretted about. That there were such concerns about girls' masturbating is in itself a sign and measure of the recognition of sexual feelings in women. In fact in 1871 one popular medical writer on women defined masturbation as "the mechanical irritation of the sexual organs in order to excite the same voluptuous sensations attendant upon natural intercourse."<sup>25</sup> Mary Wood-Allen, a leader in the Women's Christian Temperance Union and a writer of advice books for young women, had no doubt that girls could be led into self-abuse. Even girls who would not use any mechanical means "to arouse sexual desire," she pointed out, nevertheless permitted themselves to fantasize or to have mental images that "arouse the spasmodic feelings of sexual pleasure."<sup>26</sup> Indeed from Wood-Allen's book one receives the message that women's sexual feelings were not only present but dangerously easy to arouse.

Discussion about masturbation in women reveals in another way how widely accepted was the idea that women possessed sexual desires. One physician, in the course of an article on the subject, said that the worst thing about masturbation in women was that a climax and resolution of

<sup>22</sup> Chevasse, *Physical Life of Man and Woman*, 424-25.

<sup>23</sup> John Harvey Kellogg, *Plain Facts for Old and Young* (1879; Burlington, Iowa, 1881), 252.

<sup>24</sup> Augustus K. Gardner, *Conjugal Sins against the Laws of Life and Health and Their Effects upon the Father, Mother, and Child* (New York, 1870), 98.

<sup>25</sup> Chevasse, *Physical Life of Man and Woman*, 33.

<sup>26</sup> Mary Wood-Allen, *What a Young Woman Ought to Know* (Philadelphia, 1905), 155.

tension were generally not achieved; hence the vice was persisted in. In response another doctor agreed that masturbation indeed gave rise to all the physical harm alleged in the article. But he disagreed with the assertion that in a woman sexual excitation could stop short of orgasm. "A commencement of the act, either of masturbation or coition," the letter writer contended, "*naturally* leads to its consummation, viz., an orgasm." Furthermore, he persisted, if "in the *healthy* female, an orgasm is not produced in the act of coition, she is not satisfied, and either will continue the act herself or with her coadjutor till such consummation does take place."<sup>27</sup>

Women's sexuality is also assumed in another class of medical-concerns. When Dr. J. Marion Sims, the "founding father" of American gynecology, published *Clinical Notes on Uterine Surgery* in 1866, conception was only dimly understood. In explaining how it took place Sims revealed, in passing, that most people took for granted that women experienced sexual feelings. "It is the vulgar opinion, and the opinion of many savants," Sims remarked, "that, to ensure conception, sexual intercourse should be performed with a certain degree of completeness, that would give an exhaustive satisfaction to both parties at the same moment." This sounds like twentieth-century ideas on optimum sexual performance, for Sims then went on to note, again in passing, that husbands and wives strove for such simultaneity and were unhappy when they failed to have simultaneous orgasms. "How often do we hear husbands complain of coldness on the part of the wives; and attribute to this the failure to procreate. And sometimes wives are disposed to think, though they never complain, that the fault lies with the hasty ejaculation of the husband."<sup>28</sup> Sims's point, of course, was that conception did not depend upon either sexual arousal or satisfaction in the woman. The important point for us, however, is that Sims, the medical readers he was addressing, and the patients he treated, all believed women were naturally capable of sexual feelings. Napheys in his popular book of advice for women also alluded to the prevalent idea that conception and pleasure were connected. He said that many people erroneously believed that conception could be known from the "more than ordinary degree of pleasure" on the part of the woman during the sexual act.<sup>29</sup>

In the course of discussing other kinds of women's illnesses, physicians often made it clear that they not only recognized the existence of sexual feelings in women but expected them in normal women. As we have observed already, Dr. Van de Warker considered the lack of sexual feelings in a woman as an abnormality to be cured. He called such women "impotent," just as one would denominate a man who failed to have adequate sexual responses.

<sup>27</sup> Chapman, "On Masturbation"; letter from S. E. McCully, *American Journal of Obstetrics*, 16 (1883): 844, my italics.

<sup>28</sup> James Marion Sims, *Clinical Notes on Uterine Surgery* (London, 1866), 369.

<sup>29</sup> Napheys, *Physical Life of Woman*, 104-05. This belief, which other writers also speak of, may well have affected some women's attitudes toward orgasm, for if a woman, under this view, could repress pleasure or climax, conception could be prevented.

To Van de Warker, women had to learn how to dislike sex; enjoyment of it was natural.<sup>30</sup> Napheys, too, saw frigidity as abnormal; its removal, he thought, was "so desirable."<sup>31</sup> One physician in 1882, in discussing a case of excessive masturbation, wrote that during an examination his female patient experienced "the most intense orgasm that I have ever witnessed,"<sup>32</sup> implying that he had witnessed others. Another physician listed among the pathological symptoms of one patient "an absence of all sexual desire"<sup>33</sup>—as if its presence were the normal condition of a woman. One medical doctor, in trying to show how intense was the pain a married patient experienced during intercourse, said that both partners had given up sexual relations "although both had unusually violent animal passions."<sup>34</sup> In arguing against birth control Dr. August Gardner told of a wife who, fearing pregnancy since she had borne seven children in seven years, was "otherwise very ardent."<sup>35</sup>

During the 1880s and 1890s, as surgeons became more skillful and antiseptics made abdominal operations safer, a number of doctors sought to alleviate otherwise incurable or obscure pelvic pains and nervous conditions in women through the removal of ovaries. This medical development is a complex one, especially as to the attitudes it might reveal on the part of doctors and society in general. This is not the place to pursue that question, however. It serves to explain, though, why ovariectomies were a subject of considerable interest among gynecologists. One consequence of that interest was a report in 1890 by a surgeon who had removed forty-six pairs of ovaries. Significantly, he related that "the sexual instinct was always preserved. Three patients, virginal before operation, married later and lived in happy wedlock. The passions persist particularly when the operation is performed early on young persons," he concluded.<sup>36</sup> For us the significance of this report is not whether it is accurate; in fact I suspect that it is not. For as Dr. Van de Warker remarked on a different occasion, many women who suffered the pain or nervousness that caused them to submit to the operation in the first place probably had never felt any sexual pleasure. Consequently, to ask them after the operation whether there was any diminution in sexual feeling generally brought a denial. Moreover, the removal of the ovaries may well have reduced or eliminated hormonal secretions that may contribute to normal sexual feelings in women. In short, the physician's

<sup>30</sup> Van de Warker, "Impotency in Women," 39.

<sup>31</sup> Napheys, *Physical Life of Woman*, 86.

<sup>32</sup> Horatio R. Bigelow, "Aggravated Instance of Masturbation in the Female." *American Journal of Obstetrics*, 15 (1882): 437.

<sup>33</sup> "A Case of Excision of Both Ovaries for Fibrous Tumors of the Uterus, and a Case of Excision of the Left Ovary for Chronic Oöphoritis and Displacement," reported by Dr. E. H. Trenholme in *Canada Lancet*, July 1876, *American Journal of Obstetrics*, 9 (1876-77): 703.

<sup>34</sup> "Case of Vaginismus," reported by Dr. George Pepper, *American Journal of Obstetrics*, 3 (1871): 322-24.

<sup>35</sup> Gardner, *Conjugal Sins*, 97.

<sup>36</sup> Summary of paper by Dr. Keppler, "The Sexual Life of the Female after Castration," given at the 10th International Medical Congress, *American Journal of Obstetrics*, 23 (1890): 1155-56.

report suffers from his clear wish to put his series of operations in a good light. But that very wish is revealing, for what it tells us is that women were expected to have sexual feelings and it was undesirable for a surgeon or, presumably, anyone else, to eliminate or even reduce those feelings.

IN THE LIGHT of the foregoing it is difficult to accept the view that women were generally seen in the nineteenth century as without sexual feelings or drives. The question then arises as to how this widely accepted historical interpretation got established? Part of the reason, undoubtedly, is the result of the general reticence of the nineteenth century in regard to sex. The excessive gentility of the middle class has been read by historians as a sign of hostility toward sexuality, particularly in women. The whole cult of the home and women's allegedly exalted place in it was easily translated by some historians into an antisexual attitude.<sup>37</sup> But a good part of the explanation must also be attributable to the simple failure on the part of historians to survey fully the extant sources. The kind of statements quoted from medical writers in this article, for example, was either overlooked or ignored. Another important part of the explanation is that the sources that were surveyed and quoted were taken to be descriptive of the sexual ideology of the time when in fact they were part of an effort by some other medical writers to establish an ideology, not to delineate an already accepted one. In other words, the medical literature that was emphasized by Steven Marcus, Oscar Handlin, or Nathan Hale, Jr. was really normative or prescriptive rather than descriptive.

This misinterpretation was easy enough to make since much nineteenth-century medical literature was often descriptive in form even though in fact it was seeking to set a new standard of sexual behavior. Sometimes, however, the normative concerns and purposes showed through the ostensible description. A close reading, for example, of William Acton's second edition of *The Functions and Disorders of the Reproductive Organs* reveals in several places his desire to establish a new and presumably "higher" standard of sexual attitude and behavior. After pointing out that publicists strongly condemn sexual relations outside marriage, he asks, "But should we stop there? I think not. The audience should be informed that, in the present state of society, the sexual appetites must not be fostered; and experience teaches those who have had the largest means of information on the matter, that self-control must be exercised." So far, he continues, no one has "dared publicly to advocate . . . this necessary regulation of the sexual feelings or training to continence." Or later, when he discusses women in par-

<sup>37</sup> Not all historians, it should be noted, have assumed that nineteenth-century concerns about sex meant hostility toward women's sexuality. In tracing the history of the social-purity movement after 1870, David J. Pivar is careful to distinguish between a concern with the exploitation of women's sexuality and an opposition to women's sexual feelings. See his *Purity Crusade, Sexual Morality, and Social Control: 1868-1900* (Westport, 1973).

ticular, it is evident that he is arguing for a special attitude, not merely describing common practice. "The *best* mothers, wives, and managers of households know little or nothing of sexual indulgence. Love of home, children, and domestic duties are the only passions they feel," he writes.<sup>38</sup>

American writers of the time who followed the lead of Acton as well as quoting him display a similar mixture of prescription and description. Take Dr. John Kellogg's *Plain Facts for Old and Young*, which sold over 300,000 copies by 1910 and went through five editions. Kellogg, like Acton, made it clear that he thought sex was too dominant in the thoughts of people. As we look around us today, he wrote, "it would appear that the opportunity for sensual gratification has come to be, in the world at large, the chief attraction between the sexes. If to these observations," he continued, "we add the filthy disclosures constantly made in police court and scandal suits, we have a powerful confirmation of the opinion."<sup>39</sup> It was this excess that he warns against, drawing upon quotations from Acton to support his arguments. He is at pains to show, too, that continence, especially in men, is not deleterious to health, as some contended. He admits that the medical profession is not in agreement on the amount of sexual indulgence permitted in marriage. "A very few hold that the sexual act should never be indulged except for the purpose of reproduction, and then only at periods when reproduction will be possible. Others, while equally opposed to the excesses . . . limit indulgence to the number of months in the year." Human beings, he advised, should take their cue from animals, who have intercourse only for procreation and then at widely spaced intervals. Instead of heeding this counsel, he writes, loosely quoting from Acton, "the lengths to which married people carry excesses is perfectly astonishing."<sup>40</sup>

Kellogg's reference to the behavior of animals as a worthy guideline for human behavior was echoed by other writers who sought to control sexuality. William Acton and Orson S. Fowler, for example, also used that standard of sexual behavior. Kellogg even went so far as to make an overt defense of the analogy. He carefully explained to his readers that in the modern age of biology these analogies were extremely helpful in getting at nature's purpose. "It is by this method of investigation," he remarked, "that most of the important truths of physiology have been developed; and the plan is universally acknowledged to be a proper and logical one." Then he launched into a condemnation of those men who use their wives as harlots, "having no other end but pleasure." For it was clear that among animals the end was reproduction only and then only at those one or two times a year when reproduction was possible. But by the time Kellogg reached the place in his book where he defended the analogy with animals he had already revealed

<sup>38</sup> Acton, *Functions and Disorders* (1858), 8-9; (1865), 134, my italics.

<sup>39</sup> Kellogg, *Plain Facts*, 178. Hale gives the figures on Kellogg's sales in *Freud and the Americans*, 37.

<sup>40</sup> Kellogg, *Plain Facts*, 206, 209, 247, 225-26. Kellogg also quoted Acton.

that his purpose in invoking the analogy was reformist and normative, not simply scientific and logical. For in the early pages of his book, in making a different normative point—the need to protect children from premature sexuality—he told of a parent whose adolescent children often played games in the nude. When admonished for permitting this practice, the parent replied that it was only natural. “Perfectly harmless; just like little pigs!” Kellogg quoted the parent as saying. Kellogg’s comment, however, was quite different from that which he would advise later in his book: “as though pigs were models for human beings!”<sup>41</sup>

In the end Kellogg himself virtually admitted that his “plain facts” were hardly facts at all, but prescriptions and hopes. “There will be many,” he wrote, “the vast majority, perhaps, who will not bring their minds to accept the truth which nature seems to teach, which would confine sexual acts to reproduction wholly.” And so he was prepared to offer a compromise, that is, a method of contraception. It was not a very effective method, as he admitted—the so-called safe period—but again what is important is his frank recognition that only a minority among his readers confined their sexual activities to reproduction and that he hoped he would be able to induce more to do so.<sup>42</sup>

It would be a mistake, in short, to accept the prescriptive or normative literature, like that of Acton, Kellogg, and others,<sup>43</sup> as revealing very much about sexual behavior in the Victorian era. It may be possible to derive a sexual ideology from such writers, but it is a mistake to assume that the ideology thus delineated is either characteristic of the society or reflective of behavior. On the contrary, it is the argument of this article that the attitudes and behavior of middle-class women were only peripherally affected by that ideology. Not only did many medical writers, as we have seen, encourage women to express their sexuality, but there is a further, even more persuasive reason for believing that the prescriptive literature is not a reliable guide to either the sexual behavior or the attitudes of middle-class women. It is the testimony of women themselves.

ANY SYSTEMATIC KNOWLEDGE of the sexual habits of women is a relatively recent historical acquisition, confined to the surveys of women made in the 1920s and 1930s and culminating in the well-known Kinsey report.<sup>44</sup> Until

<sup>41</sup> *Ibid.*, 217, 221–25, 118.

<sup>42</sup> *Ibid.*, 265–66.

<sup>43</sup> It is true that some of the advice and medical literature that recognized women's sexual feelings and from which I have been quoting was also prescriptive rather than merely descriptive. But for convenience and economy of words in subsequent pages when I refer to “prescriptive or normative literature” I mean only that which minimized or denied women's sexuality.

<sup>44</sup> Among the largest and most significant of such surveys were Katharine B. Davis, *Factors in the Sex Life of Twenty-two Hundred Women* (New York, 1929); Robert Latou Dickinson and Lura Beam, *A Thousand Marriages: A Medical Study of Sex Adjustment* (Baltimore, 1931); and Alfred C. Kinsey et al., *Sexual Behavior in the Human Female* (Philadelphia, 1953). The first

recently no even slightly comparable body of evidence for nineteenth-century women was known to exist. In the Stanford University Archives, however, are questionnaires completed by a group of women testifying to their sexual habits. The questionnaires are part of the papers of Dr. Clelia Duel Mosher (1863–1940), a physician at Stanford University and a pioneer in the study of women's sexuality. Mosher began her work on the sexual habits of married women when she was a student at the University of Wisconsin prior to 1892. That year she transferred for her senior year to Stanford, where she received an A.B. degree in 1893 and an M.A. in 1894. In 1900 she earned an M.D. degree from Johns Hopkins University. After a decade of private practice she joined the Stanford faculty as a member of the department of hygiene and medical adviser to women students. Her published work dealt with the physical capabilities of women; she was a well-known advocate of physical exercise for women. Mosher's questionnaires are carefully arranged and bound in volume 10 of her unpublished work, "Hygiene and Physiology of Women." Mosher, however, apparently never drew more than a few impressionistic conclusions from the highly revealing questionnaires. She did not even publish the fact of their existence, and so far as can be ascertained no use has heretofore been made of this manuscript source. Yet the amount and kind of information on sexual habits and attitudes of married women in the late nineteenth century contained in these questionnaires are unique.

The project, which spanned some twenty years, was begun at the University of Wisconsin when Mosher was a student of biology in the early 1890s. She designed the questionnaire when asked to address the Mother's Club at the university on the subject of marriage. In later years she added to her cases and used the information when giving advice to women about sexual and hygienic matters.<sup>45</sup> This initiative, as well as the kind of questions she asked, reveals that Mosher was far ahead of her time. She amassed information on women's sexuality that none of the many nineteenth-century writers on the subject studied in any systematic way at all.

The questionnaire itself is quite lengthy, comprising twenty-five questions, each one of which is divided into several parts. Much of the questionnaire, it is true, is taken up with ascertaining facts about the parents and even the grandparents of the respondents, but over half of the questions deal directly with women's sexual behavior and attitudes.<sup>46</sup> The informa-

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chapter of Robert Latou Dickinson and Lura Beam, *The Single Woman* (Baltimore, 1934), concerns the sexual life of working girls in the 1890s, but it is based on forty-six cases, the typical patient being born "soon after 1870." I am indebted to David M. Kennedy of Stanford University for this reference.

<sup>45</sup> Mosher, "Hygiene and Physiology of Women," 10: xv, Mosher Papers, Stanford University Archives.

<sup>46</sup> The principal questions dealing with women's sexual habits are: number of conceptions; number of conceptions by choice and by accident; frequency of intercourse; whether intercourse is participated in during pregnancy; whether intercourse is "agreeable"; whether an orgasm occurs; what effects from orgasm, or from failure to have one; purpose of intercourse; the ideal

tion contained in the questionnaires not only supports the interpretation of women's sexuality that already has been drawn from the published literature, both lay and medical, but it also provides us with a means of measuring the degree to which the prescriptive marriage literature affected women's sexual behavior.

Since the evidence in this questionnaire, which I call the Mosher Survey, has never been used before, it is first worthwhile to examine the social background of the women who answered the questionnaires. All told there are forty-six useable questionnaires, but since two of the questionnaires seem to have been filled out by the same woman at an interval of twenty-three years, the number of women actually surveyed is forty-five.<sup>47</sup> In the aggregates that follow I have counted only forty-five questionnaires. The questionnaires, it ought to be said, were not administered at the same time, but at three different periods at least; moreover the date of administration of nine questionnaires cannot be ascertained. Of those that do provide that information, seventeen were completed before 1900, fourteen were filled out between 1913 and 1917, and five were answered in 1920.

More important than the date of administration of the questionnaires are the birth dates of the respondents. All but one of the forty-four women who provided their dates of birth were born before 1890. In fact thirty-three, or seventy per cent of the whole group, were born before 1870. And of these, seventeen, or slightly over half, were born before the Civil War. For comparative purposes it might be noted that in Alfred Kinsey's survey

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habit of sexual relations; whether there is desire for intercourse other than during pregnancy; whether contraception is used and method employed; whether wife sleeps in same bed with husband; knowledge of sexual physiology prior to marriage; and the character of menses: age of onset, pain, and amount.

<sup>47</sup> The small number of women queried in the Mosher Survey may cause some readers to discount almost entirely the significance of any conclusions drawn from it. While such a response may be understandable as a first reaction, in the end I think it would be unwise. So far as I know, this is the only survey of sexual attitudes and practices in the nineteenth century; historians' standard conception of women's sexual practices and attitudes in the nineteenth century has been derived from no previous survey at all. Certainly the systematic questioning of forty-five women at considerable length and their rationales for their answers ought to be at least as significant in shaping historians' conceptions of women's sexuality as the scraps of information from interested writers at the time, novels, and recollections, which have been the bases of our traditional picture of women's sexual attitudes and behavior in the nineteenth century. It is true that we do not know at the present time who these women were or how random their selection was. But there seems little reason to believe that the women were specially chosen by Mosher, if only because the purpose of the original questionnaire as well as the use of the information gained from it were to help her in advising women students. Moreover, as an unmarried woman herself, it is very likely that the information from the questionnaires was Mosher's most valuable source of knowledge on women's sexuality. It is probably true, given the general reluctance of nineteenth-century people to discuss sex, that some women whom Mosher approached refused to answer the questionnaire. But it is worth recalling that the value even of modern sex surveys, including Kinsey's, has been questioned on the grounds that the respondents were largely self-selected. Obviously the Mosher Survey is not the final word on the sexual behavior and attitudes of women in the nineteenth century. But at the same time it ought not to be rejected because of its limited size; that would be applying a methodological standard quite inappropriate for a sensitive subject in which the evidence is always limited and fugitive.



of women's sexuality the earliest cohort of respondents was only born in the 1890s. In short, the attitudes and practices to which the great majority of the women in the Mosher Survey testify were those of women who grew up and married within the nineteenth century, regardless of when they may have completed the questionnaires.

An important consideration in evaluating the responses, of course, is the social origins of the women. From what class did they come, and from what sections of the country? The questionnaire, fortunately, provides some information here, but not with as much precision as one might like. Since the great majority of the respondents attended college or a normal school (thirty-four out of forty-five, with the education of three unknown), it is evident that the group is not representative of the population of the United States as a whole. The remainder of the group attended secondary school, either public or private, a pattern that is again not representative of a general population in which only a tiny minority of young people attended secondary school. But for purposes of evaluating the impact of the prescriptive or marital-advice literature upon American women this group is quite appropriate. For inasmuch as their educational background identifies them as middle- or upper-class women, it can be said that they were precisely those persons to whom that advisory literature was directed and upon whom its effects ought to be most evident.

In geographical origin the respondents to the Mosher Survey seem to be somewhat more representative, if the location of parents, birthplaces, and colleges attended can be taken as a measure, albeit impressionistic, of geographical distribution. Unfortunately there is no other systematic or more reliable information on this subject. The colleges attended, for example, are located in the Northeast (Cornell [6], Smith, Wellesley, and Vassar [2]), in the Middle West (Ripon, Iowa State University, and Indiana), and in the Far West (Stanford [9], the University of California, and the University of the Pacific). The South is not represented at all among the colleges attended.

Although the emphasis upon prestigious colleges might make one think that these were women of the upper or even leisure class, rather than simply middle class, a further piece of information suggests that in fact they were not. One of the questions asked concerned working experience prior to marriage. Although seven of the respondents provided no data at all on this point, and eight reported that they had married immediately after completing their education, thirty of the women reported that they had worked prior to marriage. As a side light on the opportunities available to highly educated women in the late nineteenth century, it is worth adding that twenty-seven of the thirty worked as teachers. On the basis of their working experience it seems reasonable to conclude that the respondents were principally middle- or upper-middle-class women rather than members of a leisure class.

Despite the high level of education of these women, they confessed to having a pretty poor knowledge, by modern standards, of sexual physiology be-

fore marriage. Only eleven said that they had much knowledge on that subject, obtained from female relatives, books, or courses in college, while another thirteen said that they had some knowledge. The remainder—slightly over half—reported that they had very little or no knowledge. No guidelines were given in the questionnaire for estimating the amount of knowledge. The looseness of the definition is shown by the fact that three of the respondents who said that they had no knowledge at all named books on women's physiology that they had read. From other titles mentioned in passing it is clear that a number of these women had direct acquaintance with the prescriptive and advisory literature of the time. How did it affect their behavior? Did they repress their sexual impulses or deny them, as some of the prescriptive literature advised? Were they in fact without sexual desire? Or were they motivated toward personal sexual satisfaction as the medical literature quoted in this article advised?

The Mosher Survey provides a considerable amount of evidence to answer these and other questions. To begin with, thirty-five of the forty-five women testified that they felt desire for sexual intercourse independent of their husband's interest, while nine said they never or rarely felt any such desire. What is more striking, however, is the number who testified to orgasmic experience. According to the standard view of women's sexuality in the nineteenth century, women were not expected to feel desire and certainly not to experience an orgasm. Yet it is striking that in constructing the questionnaire Dr. Mosher asked not only whether the respondents experienced an orgasm during intercourse but whether "*you always have a venereal orgasm?*" (my italics). Although that form of the question makes quite clear Mosher's own assumption that female orgasms were to be expected, it unfortunately confuses the meaning of the responses. (Incidentally, only two of the forty-five respondents failed to answer this question.) Five of the women, for instance, responded "no" without further comment. Given the wording of the question, however, that negative could have meant "not always, but almost always" as well as "never" or any response in between these extremes. The ambiguity is further heightened when it is recognized that in answer to another question, three of the five negatives said that they had felt sexual desire, while a fourth said "sometimes but not often," and the fifth said sex was "usually a nuisance." Luckily, however, most of the women who responded to the question concerning orgasm made more precise answers. The great majority of them said that they had experienced orgasms. The complete pattern of responses is set forth in table 1.

In sum, thirty-four of the women experienced orgasm, with the possibility that the figure might be as high as thirty-seven if those who reported "no" but said they had felt sexual desire are categorized as "sometimes." (Interestingly enough, of nine women out of the forty-five who said they had never felt any sexual desire, seven said that they had experienced orgasms.) Moreover, sixteen or almost half of those who experienced orgasms did so either

TABLE 1. RESPONSE TO THE QUERY:  
 "DO YOU ALWAYS HAVE A VENEREAL ORGASM?"

<i>Response</i>	<i>Number</i>	<i>Percentage</i>
No Response	2	4.4%
"No" with No Further Comment	5	11.1%
"Always"	9	20.0%
"Usually"	7	15.5%
"Sometimes," "Not Always," or "No" with Instances	18	40.0%
"Once" or "Never"	4	8.8%

"always" or "usually." As we have seen, in the whole group of forty-five, all but two responded to the question asking if an orgasm was always experienced. Of those forty-three, thirty-four were born before 1875. Five answered "no" to that question without any further comment. One other woman responded "never," and two others said "once or twice." If the "noes" and the "never" are taken together, the proportion of women born before 1875 who experienced at least one orgasm is eighty-two per cent. If the "noes" are taken to mean "sometimes" or "once or twice," as they might well be, given the wording of the question, then the proportion rises to ninety-five per cent. For comparative purposes the figures for twentieth-century women provided in Kinsey's study are given in table 2. Kinsey's proportions are arranged by age group and chronological period; hence they are not strictly comparable with those derived from Mosher's data. But the comparison is still suggestive, even when made with the women in the age group 26-30.<sup>48</sup>

TABLE 2. PERCENTAGE OF WOMEN EXPERIENCING ORGASM  
 DURING INTERCOURSE  
 (by decade of birth)

<i>Women Born</i>	<i>Ages 21-25</i>	<i>Ages 26-30</i>
Before 1900	72%	80%
1900-09	80%	86%
1910-19	87%	91%
1920-29	89%	93%

SOURCE: Alfred C. Kinsey *et al.*, *Sexual Behavior in the Human Female* (Philadelphia, 1953), 337, table 97.

<sup>48</sup> A comparison of the sexual responses of the older and younger women in the Mosher Survey did not reveal any greater interest in sex among the younger group, but the numbers involved were too small to be significant. The responses of fourteen women born before 1860 were compared with those of the eight women born after 1875. On the other hand, if the responses to the questions about desire for sex and about orgasmic experience are categorized by date at which the questionnaire was completed, regardless of the age of the respondent, there is a slight, if somewhat ambiguous, difference between the earlier and later respondents. Seventeen women completed the questionnaire before 1900; nineteen did so after 1912. Thirteen of the seventeen completed before 1900 responded to the question of whether they had experienced orgasm; four of the thirteen said they had not. Eighteen of the nineteen who completed the questionnaire after 1912 answered that question; only one out of eighteen failed to experience

MUCH MORE INTERESTING and valuable than the bare statistics are the comments or rationales furnished by the women, which provide an insight into the sexual attitudes of middle-class women. As one might expect in a population by its own admission poorly informed on sexual physiology, the sexual adjustment of some of these women left something to be desired. Mosher, for example, in one of her few efforts at drawing conclusions from the Survey, pointed out that sexual maladjustment within marriage sometimes began with the first intercourse. "The woman comes to this new experience of life often with no knowledge. The woman while she may give mental consent often shrinks physically." From her studies Mosher had also come to recognize that women's "slower time reaction" in reaching full sexual excitement was a source of maladjustment between husband and wife that could kill off or reduce sexual feelings in some women. Women, she recognized, because of their slower timing were left without "the normal physical response. This leaves organs of women over congested."<sup>49</sup> At least one of her respondents reported that for years intercourse was distasteful to her because of her "slow reaction," but "orgasm [occurs] if time is taken." On the other hand, the respondent continued, "when no orgasm, [she] took days to recover."<sup>50</sup> Another woman spoke of the absence of an orgasm during intercourse as "bad, even disastrous, nerve-wracking—unbalancing, if such conditions continue for any length of time." Still a third woman, presumably referring to the differences in the sexual rhythms of men and women, said, "Men have not been properly trained." One of the women in the Mosher Survey testified in another way to her recognition of the differences in the sexuality of men and women. "Every wife submits when perhaps she is not in the mood," she wrote, "but I can see no bad effect. It is as if it had not been. But my husband was absolutely considerate. I do not think I could endure a man who forced it." And her response to a question about the effects of an orgasm upon her corroborate her remark: "a general sense of well being, contentment and regard for husband. This is true Doctor," she earnestly wrote.<sup>51</sup>

Mosher's probing of the attitudes of women toward their sexuality went beyond asking about orgasms. Several of her questions sought to elicit the

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an orgasm. In themselves these data suggest that women who answered the questionnaire in the twentieth century achieved somewhat more satisfaction in their sexual experience than those who completed the questionnaire in the nineteenth century. But when a similar division by century is made of the questionnaires in regard to another question, that conclusion is not so clear. One of the questions asked whether the respondent felt sexual desire. Fourteen women answered the question prior to 1900, of whom only two said they had failed to feel desire. But of the sixteen who responded to the same question after 1912, three said they lacked any feeling of desire. Here the proportion of sexuality was higher among the nineteenth- than the twentieth-century respondents.

<sup>49</sup> Mosher, "Hygiene and Physiology of Women," 10: 1. Twelve of the women were asked how soon after marriage they engaged in intercourse. Six said within the first three days, while six said from ten days to a year after the ceremony.

<sup>50</sup> *Ibid.*, case no. 51. The case numbers have been assigned by Mosher herself and appear on each page of each questionnaire. Hereafter the citation of cases will carry only "Hygiene and Physiology of Women" and case number.

<sup>51</sup> *Ibid.*, case nos. 47, 40, 41.

reactions of women to sexual intercourse. What is the purpose of sex, she asked? Is it a necessity for a man or for a woman? Is it for pleasure, or is it for reproduction?<sup>52</sup> Only two of the women failed to respond in some fashion to these questions. Nine thought sex was a necessity for men, while thirteen thought it was a necessity for both men and women. Fifteen of the respondents thought it was not a necessity for either sex. Twenty-four of the forty-five thought that it was a pleasure for both sexes, while only one thought it was exclusively a pleasure for men. Given the view generally held about sexual attitudes in the nineteenth century, it comes as something of a surprise to find that only thirty marked "reproduction" as the primary purpose of sex. In fact, as we shall see in a moment, some of the women thought reproduction was not as important a justification for intercourse as love.

As one might expect, this particular series of questions was usually answered with a good deal of explanation. One woman who emphasized reproduction as the principal justification took the opportunity to condemn those couples she apparently had heard of who did not want children. "I cannot recognize as true marriage that relation unaccompanied by a strong desire for children." She thought it was close to "legalized prostitution." She admitted that because of her love for her husband she "cultivated the passion to effect the 'compromise' in this direction that must come in every other [area] when people marry." She went on to say that she did not experience orgasm until the fifth or sixth year of her marriage and that even at the time of her response to the questionnaire—the early 1890s—she still did not reach a sexual climax half of the time. A second woman was also apparently out of phase with her husband's sexual interests, for she thought a woman's needs for sex occurred "half as often as a man's." It is revealing of her own feelings that though she said "half as often," the figures she used to illustrate her point—twice a week for a man and twice a month for a woman—are actually in the ratio of one to four rather than of one to two as she said. Her true attitude was also summed up in the remark that since she was always in good health and intercourse "did not hurt me, . . . I always meant to be obliging."<sup>53</sup>

But, as the earlier statistical breakdown makes evident, the women who only tolerated intercourse were in a decided minority. A frank and sometimes enthusiastic acceptance of sexual relations was the response from most of the women. Sexual intercourse "makes more normal people," said a woman born in 1857. She was not even sure that children were necessary to justify sexual relations within marriage. "Even if there are no children, men love their wives more if they continue this relation, and the highest devotion is based upon it, a very beautiful thing, and I am glad nature gave it to us."

<sup>52</sup> Since each respondent could legitimately answer "yes" to all three suggested justifications for sexual relations, the totals here can go beyond forty-five, though not all questions were always answered.

<sup>53</sup> *Ibid.*, case nos. 24, 19.

Since marriage should bring two people close together, said one woman born in 1855, sexual intercourse is the means that achieves that end. "Living relations have a right to exist between married people and these cannot exist in perfection without sexual intercourse to a moderate degree. This is the result of my experience," she added. A woman born in 1864 described sexual relations as "the gratification of a normal healthy appetite." The only respondent who was divorced and remarried testified in 1913 that at age fifty-three "my passionate feeling has declined somewhat and the orgasm does not always occur," but intercourse, she went on, was still "agreeable" to her.<sup>54</sup>

Several of the women even went so far as to reject reproduction as sufficient justification for sex. Said one woman, "I consider this appetite as ranking with other natural appetites and like them to be indulged legitimately and temperately; I consider it illegitimate to risk bringing children into the world under any but most favorable circumstances." This woman was born before the Compromise of 1850 and made her comment after she had been married ten years. Another woman, also born a decade before the Civil War, denied that reproduction "alone warrants it at all; I think it is only warranted as an expression of true and passionate love. This is the prime condition for a happy conception, I fancy." To her, too, the pleasure derived from sexual intercourse was "not sensual pleasure, but the pleasure of love."<sup>55</sup>

A third woman born before 1861 doubted that sex was a necessity in the same sense as food or drink, but she had no doubt that "the desire of both husband and wife for this expression of their union seems to me the first and highest reason for intercourse. The desire for offspring is a secondary, incidental, although entirely worthy motive but could never to me make intercourse right unless the mutual desire were also present." She saw a clear conflict between the pleasure of intercourse and reproduction. "My husband and I," she said in 1893,

believe in intercourse for its own sake—we wish it for ourselves and spiritually miss it, rather than physically, when it does not occur, because it is the highest, most sacred expression of our oneness. On the other hand, even a slight risk of pregnancy, and then we deny ourselves the intercourse, feeling all the time that we are losing that which keeps us closest to each other.<sup>56</sup>

Another woman, in describing the ideal of sexual relations, said that she did not want intercourse to occur at any time when conception was likely, for conception should not occur by accident. Instead it ought to be the result of

deliberate design on both sides in time and circumstances most favorable physically and spiritually for the accomplishment of an immensely important act. It

<sup>54</sup> *Ibid.*, case nos. 41, 18, 2. It is worth noting that here, as elsewhere in the survey, no mention was made of religious reasons for or against intercourse. These women had almost entirely secularized their sexual ideology.

<sup>55</sup> *Ibid.*, case nos. 14, 12.

<sup>56</sup> *Ibid.*, case no. 15.

amounts to separating times and objects of intercourse into (a) that of expression of love between man and woman (that act is frequently simply the extreme caress of love's passion, which it would be a pity to limit . . . to once in two or three years) and (b) that of carrying on a share in the perpetuation of the race, which should be done carefully and prayerfully.<sup>57</sup>

It seems evident that among these women sexual relations were neither rejected nor engaged in with distaste or reluctance. In fact for them sexual expression was a part of healthy living and frequently a joy. Certainly the prescriptive literature that denigrated sexual feelings or expression among women cannot be read as descriptive of the behavior or attitude of these women. Nevertheless this is not quite the same as saying that the marriage handbooks had no effect at all. To be sure, there is no evidence that the great majority of women in the Mosher Survey felt guilty about indulging in sex because of what they were told in the prescriptive literature. But in two cases that literature seems to have left feelings of guilt. One woman said that sexual relations were "apparently a necessity for the *average* person" and that it was "only [the] superior individuals" who could be "independent of sex relations with no evident ill-results." To her, as to St. Paul and some of the marriage-advice books, it was better to indulge than to burn, but it was evidently even better to be free from burning from the beginning. A more blatant sign of guilt over sex came from the testimony of a woman who quite frankly thought the pleasure of sex was a justification for intercourse, but, she added "not necessarily a legitimate one."<sup>58</sup>

Dr. Mosher herself obliquely testified to the effects of the prescriptive literature. She attributed the difficulties some women experienced in reaching orgasm to the fact that "training has instilled the idea that any physical response is coarse, common and immodest which inhibits [women's] proper part in this relation."<sup>59</sup> That was the same point that some of the medical writers in the nineteenth century had made in explaining the coldness of some women toward their husbands.

The advice literature, for men as well as for women, generally warned

<sup>57</sup> *Ibid.*, case no. 22.

<sup>58</sup> *Ibid.*, case nos. 47, 30. Marcus found a comparable example of guilt arising out of the prescriptive literature against masturbation. In discussing the Victorian sexual autobiography *My Secret Life*, Marcus observes that the anonymous author gave full credence to the dangers described in the literature, yet he masturbated nonetheless. After doing so, however, the anonymous author reported he suffered from depression, guilt, fatigue, and general feelings of debilitation though he felt none of these symptoms after sexual intercourse. Marcus ascribes these feelings to an internalizing of social attitudes, presumably derived from the prescriptive literature against masturbation. *Other Victorians*, 112. It is significant, however, that the prescriptions did not stop the practice. Why it did not stop is suggested by a more recent study of sexual behavior. Masters and Johnson report that most of their male subjects still believed the old tales of physical and psychical harm from masturbation, especially from "excessive" activity, but none of them desisted from the practice. The authors point out that no matter how active a subject was in this respect, he always defined "excessive" as more active than his own practice. William H. Masters and Virginia E. Johnson, *Human Sexual Response* (Boston, 1966), 201-02.

<sup>59</sup> Mosher, "Hygiene and Physiology of Women," 10: 1.

against excessive sexual activity.<sup>60</sup> This emphasis upon limits is reflected in the remarks of some of the women in the Mosher Survey. One woman said, for example, that "the pleasure is sufficient warrant" for sexual relations, but only if "people are extremely moderate and do not allow it to injure their health or degrade their best feelings toward each other." Another woman had concluded that "to the man and woman married from love," sexual intercourse "may be used temperately as one of the highest manifestations of love granted us by our Creator." A third woman who had no doubt that sexual relations were "necessary to marital happiness," nonetheless said she believed "in temperance in it."<sup>61</sup> But temperance, another one of the women in the Mosher Survey reminds us, should not be confused with repugnance or distaste. Although this respondent did not think the ideal sexual relation should occur more often than once a month, she did think it ought to take place "during the menstrual period . . . and in the daylight." The fact is that this woman, in answer to other questions, indicated that she experienced sexual desire about once a week, but with greatest intensity "before and during menses." She was, in short, restricting her own ideal to what she considered an acceptable frequency of indulgence. Her description of her feelings after orgasm suggests where she learned that limits on frequency might be desirable or expected: "Very sleepy and comfortable. No disgust, as I have heard it described."<sup>62</sup>

THIS EXAMINATION OF THE LITERATURE, the popular advice books, and particularly the Mosher Survey makes clear that historians are ill-advised to rely upon the marital-advice books as descriptions either of the sexual behavior of women or of general attitudes toward women's sexuality. It is true that a literature as admittedly popular as much of the prescriptive or normative literature was could be expected to have some effect upon behavior as well as attitudes. But those effects were severely limited. Most people apparently did not follow the prescriptions laid down by the marriage and advice manuals. Indeed some undoubtedly found that advice wrong or misleading when measured against experience. Through some error or accident the same woman was apparently interviewed twice in the Mosher Survey, twenty-three years apart. As a result we can compare her attitudes at the beginning of her marriage in 1896 and her attitude in 1920. After one year of marriage she thought that sexual relations ought to be confined to reproduction only, but

<sup>60</sup> Hale cites sources ranging in origin from 1830 to 1910 on the concern for conserving sexual energy. *Freud and the Americans*, 35. Oscar Handlin sums up the advice in this fashion: "Abstinence, repression, and self-restraint thus were the law; and violations were punished by the most hideous natural consequences, described in considerable graphic detail." Handlin's conclusion, however, that the readers of that literature "were overwhelmed by the guilt and shame the necessities of self-control imposed," seems unwarranted on the basis of present evidence. *Race and Nationality in American Life* (Garden City, 1957), 122-23.

<sup>61</sup> Mosher, "Hygiene and Physiology of Women," case nos. 33, 10, 13.

<sup>62</sup> *Ibid.*, case no. 11.



when asked the same question in 1920, she said that intercourse ought not to be confined to reproduction, though she thought it should be indulged in only when not pressed with work and when there was time for pleasure.<sup>63</sup> Another woman in the Mosher Survey changed her mind about sexual relations even earlier in her sexual life. She said,

My ideas as to the reason for [intercourse] have changed materially from what they were before marriage. I then thought reproduction was the only object and that once brought about, intercourse should cease. But in my experience the habitual bodily expression of love has a deep psychological effect in making possible complete mental sympathy, and perfecting the spiritual union that must be the lasting "marriage" after the passion of love has passed away with years.

These remarks were made in 1897 by a woman of thirty after one year of marriage.<sup>64</sup>

Her comments make clear once again that historians need to recognize that the attitudes of ordinary people are quite capable of resisting efforts to reshape or alter them. That there was an effort to deny women's sexual feelings and to deny them legitimate expression cannot be doubted in the light of the books written then and later about the Victorian conception of sexuality. But the many writings by medical men who spoke in a contrary vein and the Mosher Survey should make us doubt that the ideology was actually put into practice by most men or women of the nineteenth century, even among the middle class, though it was to this class in particular that the admonitions and ideology were directed. The women who responded to Dr. Mosher's questions were certainly middle- and upper-middle-class women, but they were, as a group, neither sexless nor hostile to sexual feelings. The great majority of them, after all, experienced orgasm as well as sexual desire. Their behavior in the face of the antisexual ideology pressed upon them at the time offers testimony to the truth of Alex Comfort's comment that "the astounding resilience of human commonsense against the anxiety makers is one of the really cheering aspects of history."<sup>65</sup>

<sup>63</sup> *Ibid.*, case nos. 30, 33. Mosher gives no indication that she knew the two questionnaires were from the same person.

<sup>64</sup> *Ibid.*, case no. 22.

<sup>65</sup> Alex Comfort, *The Anxiety Makers: Some Curious Preoccupations of the Medical Profession* (London, 1967), 113.

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## The Poetry, Prose, and Politics of Léon Blum, 1872-1950

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A Review Article by JOEL COLTON

*L'Oeuvre de Léon Blum*. Volume 1, 1891-1905 (1954, pp. xxxi, 588); volume 2, 1905-1914 (1962, pp. xvi, 652); volume 3, part 1, 1914-1928 (1972, pp. xi, 586); volume 3, part 2, 1928-1934 (1972, pp. 709); volume 4, part 1, 1934-1937 (1964, pp. viii, 510); volume 4, part 2, 1937-1940 (1965, pp. 630); volume 5, 1940-1945 (1955, pp. xv, 552); volume 6, part 1, 1945-1947 (1958, pp. xiii, 472); volume 6, part 2, 1947-1950 (1963, pp. 476). Paris: Éditions Albin Michel. 30 fr. each, 225 fr. the set.

WHEN THE FINAL VOLUME of this publishing project appeared in 1972, those associated with the enterprise could take pride in their achievement. The project had been completed in time to commemorate the one-hundredth anniversary of the French statesman's birth. In honor of the centennial the Archives de France sponsored an exhibition and the Centre National de la Recherche Scientifique a colloquium. But these nine volumes, launched shortly after Léon Blum died March 30, 1950, under the general editorship of his son Robert Blum, were designed to be the lasting tribute and testimonial to his memory. The first volume, covering the years 1891-1905, was published in 1954.<sup>1</sup> Thereafter, the individual volumes were published as they became ready, although the project as a whole was organized chronologically in accordance with the major periods of Blum's life and career. The schedule envisaged the publication of two volumes a year and completion in an estimated five years, but it required twenty-two. The burden of editing the thousands upon thousands of pages that the Socialist man of letters and man of politics had turned out in his lifetime proved more formidable than anticipated; death took its toll, moreover, of at least three persons who had accepted responsibility for preparing various volumes. Financial pressures

<sup>1</sup> See my review, *AHR*, 60 (1954-55): 937. For Blum's life and career, and a detailed bibliography, see Joel Colton, *Léon Blum: Humanist in Politics* (New York, 1966; paperback ed., Cambridge, Mass., 1974). The centennial exhibit arranged by the Archives de France is described in a useful catalog with excerpts of some unpublished material, Archives de France, *Léon Blum* (Paris, 1972). The colloquium held in June 1973 under the auspices of the Centre National de la Recherche Scientifique has been published as *Table ronde sur Léon Blum et l'état* (Paris, 1973), as were the proceedings of an earlier colloquium organized in March 1965 by the Fondation Nationale des Sciences Politiques, *Léon Blum, chef de gouvernement, 1936-1937* (Paris, 1967).

were always a problem; a last-minute crisis was happily resolved when the Ford Foundation provided a small grant to make possible publication of the final volume in time for the centennial.

The designation of the project from the beginning as *L'Oeuvre de Léon Blum* calls for explanation. The French word *oeuvre* is a collective noun meaning "work" in the sense of the "work" of an artist or writer; it differs from the plural feminine noun *oeuvres* ("works"), which the French use in referring to collected works—*les oeuvres* or *les oeuvres complètes*. In this case the decision was reached at the outset that it would not be feasible, financially or otherwise, to contemplate publication within a reasonable period of time and with limited resources the "complete works," a venture that would have involved, by rough estimate, at least twice the number of volumes. *L'Oeuvre de Léon Blum*, therefore, was not launched with the object of reproducing the complete works of Blum but rather of presenting his major writings and a substantial selection of all his other writings and speeches—the "best of Léon Blum," so to speak.<sup>2</sup>

Despite the decision of the editors in Blum's case to bow to necessity from the beginning and to select from the "immense corpus" of his work, they announced their determination to capture the essence of Blum's contributions and accomplishments. No small goal, it has been admirably achieved even if captious reviewers like myself retain some reservations. The editors have indeed caught the thought and essence of this cultivated, gifted, sensitive French intellectual who before entering politics cut his literary eyeteeth on poetry and then became a leading literary and drama critic and political essayist, the author of half a dozen books even apart from the volumes of his collected literary and theater criticism;<sup>3</sup> enjoyed a distinguished legal career as a jurist on the Conseil d'État, France's highest tribunal of administrative law; served for almost twenty years as editor of the official French Socialist newspaper, *Le Populaire*, preparing a signed editorial or column almost daily; served as parliamentary leader of the Socialist party in the chamber of deputies almost uninterruptedly from 1919 to 1940; held office as premier twice before the Second World War and once after, most notably as head of the memorable and controversial Popular Front government from June 1936 to June 1937; heroically defied the Riom court and the Vichy regime in the unsavory effort to pin responsibility for the collapse of France in 1940 on the civilian leaders of the Third Republic, and indeed on the parliamen-

<sup>2</sup> The decision was made by Robert Blum and the Société des Amis de Léon Blum. In conception and execution the project has not repeated the publishing history of an earlier venture dedicated to Jean Jaurès: *Oeuvres de Jean Jaurès*, ed. Max Bonnefous (Paris, 1931-39). The "complete works" were promised, but when the first volume appeared in 1931 the editor announced that for practical reasons the original decision had been altered in favor of publishing only selected writings; even then only nine of the projected thirty volumes had appeared when further publication was interrupted by the war in 1939. See the comments by Harvey Goldberg, *The Life of Jean Jaurès* (Madison, 1962), 570-71.

<sup>3</sup> The most recent book to appear on Blum is an excellent treatment of his pre-1914 literary career. William Logue, *Léon Blum: The Formative Years, 1872-1914* (DeKalb, 1973).

tary regime itself; and finally, returning from Vichy prisons and German concentration camps once again played a role in public life after 1945, defending the "Republic," as he understood it, against Communists and Gaulists alike. The completion of the project is an outstanding tribute to one of the finest minds and most accomplished human beings of the twentieth century; and it is indeed, as Blum once said of the project conceived for Jaurès, a means by which "the history of a man shines through and illuminates the history of an age."<sup>4</sup> Enough of Blum has been brought together to show the richness, diversity, and versatility of his personality as a man of thought and man of action, and one can follow with fascination his evolution through the years from literary dilettante to Socialist party leader and theoretician to republican statesman. One can also follow the heartbreak and tribulations of France itself from the Dreyfus affair through two world wars down to the uneasy beginnings of the Fourth Republic, as well as the indecision, divisions, and frustrations of twentieth-century European socialism.

As a partial remedy for the inability to reproduce all of the assembled texts, the editors have included four separate bibliographical lists, appended to the appropriate volumes, which chronologically set forth Blum's writings and speeches for each of four major periods of his life and career. The first, the most difficult to compile as it covers the vast literary work of Blum before 1914 (the political writings are excluded), establishes Blum's authorship of some eight hundred items; it lists the book reviews reproduced in *En lisant* (1906), the drama criticism he had selected for publication in *Au théâtre* (1906-11), and the hundreds of other reviews of books and plays scattered in dozens of periodicals and newspapers, some unsigned or written under a nom de plume. This and the three companion indexes will be invaluable to scholars because they present comprehensively Blum's literary writings, daily editorials, speeches in parliament, statements as premier, speeches and interventions at party congresses, and a miscellany of other activities. The indexes not only list all the texts that can be identified as Blum's but also establish the date and place of delivery of his speeches as well as the date and place of publication of his writings, including the title and date of each of his daily editorials. They indicate further whether the item was ever reproduced in pamphlet form or included in some earlier collection of his speeches and writings and finally whether it has been reproduced in the present project, with appropriate volume and page reference. No such chronological index has appeared before, and in part it helps compensate for the limitations of selection imposed upon the project as a whole.

As to the decisions on selection I retain the wistful feeling first expressed in 1955 that in addition to reproducing Blum's major works already available on the bookshelves of most major libraries—for example, *Les Nouvelles Conversations de Goethe et Eckermann* (1901), *Les Congrès ouvriers et socialistes*

<sup>4</sup> "Idée d'une biographie de Jaurès," speech given July 31, 1917, in *L'Oeuvre de Léon Blum*, vol. 3, pt. 1: 1914-1928, 3.

*français* (1901), *Du mariage* (1907), *Stendhal et le beylisme* (1914), *La Réforme gouvernementale* (1918), *Souvenirs sur l'affaire* (1935), and *A l'échelle humaine* (1945)—an effort might have been made to reproduce a larger number of the daily editorials and speeches as well as more of the fragmentary correspondence and ephemeral, fugitive pieces, the recovery of which means so much to the professional historian. As it is, this has been done only for the volume covering the war years, and then, as we shall see, only incompletely. Even more significantly, scholars will inevitably raise the question of why certain exclusions. A selection, as the editor himself writes, cannot fail to be "risky and debatable" (*précaire et contestable*).<sup>5</sup> Robert Blum and his associates, many of them professionally trained and distinguished scholars like Julien Cain, Jean Texcier, Georges Dupeux, François Furet, Robert Verdier, Pierre Vidal-Naquet, Louis Faucon, and others, had to make some difficult choices. A decision had to be made for each volume, and the editor in chief, in consultation with those associated in the preparation of the volume, had to take ultimate responsibility.

Yet some of the omissions are troublesome. Without cataloging a long list, a few may be pointed out. Why the omission of Blum's early article, "Les progrès de l'a-politique en France," in *La Revue Blanche* of July 1892, protesting against various forms of socialist "regimentation"—and thereby showing his own uneven advance toward socialism?<sup>6</sup> Why the omission of his early political articles written for *L'Humanité* in the spring of 1917 hailing America's entry into the war, or in the spring of 1918 calling for a Jacobin defense of Paris and total mobilization of resources; did they too strongly affirm the "sacred union" policy that reformist socialists later had to live down?<sup>7</sup> But then why omit his important and often-cited article of November 1918, in which he stated that when faced with the choice "between Wilson and Lenin, between democracy and Bolshevik fanaticism," he replied: "I choose neither Wilson nor Lenin. I choose Jaurès"?<sup>8</sup>

On Blum's own intellectual and political development one looks in vain for the chapter he contributed to Louis Lévy, *Comment ils sont devenus socialistes* (1932), which is the closest approximation to an autobiographical essay Blum ever wrote. Although for the twenties considerable space is devoted to his theories about the party's role in the French parliamentary regime and his repeated warnings against prematurely accepting a cabinet role, the reader does not find his early important statement of 1926 made in

<sup>5</sup> See Robert Blum, introd., *L'Oeuvre de Léon Blum*, 1: 1891-1905, xvii; and see also his introductory remarks in the final volume published, vol. 3, pt. 2: 1928-1934, 7.

<sup>6</sup> "Les progrès de l'a-politique en France," *La Revue Blanche*, 3 (1892): 10-21; the article was subsequently edited by Robert Blum and reprinted in *Preuves*, 5 (1956): 38-44.

<sup>7</sup> The editorials referred to are "Vers la république," *L'Humanité*, Apr. 8, 1917; "Et Paris," *ibid.*, June 8, 1918; and "Rapprochements," *ibid.*, July 4, 1918.

<sup>8</sup> "Il faut s'entendre," *L'Humanité*, Nov. 15, 1918. The editorial assumes added significance in view of Arno J. Mayer's reassessments of diplomacy and public opinion in the First World War and postwar period. Mayer cites the editorial in his *Political Origins of the New Diplomacy, 1917-1918* (New Haven, 1959), 387, and again refers to it in his *Politics and Diplomacy of Peacemaking: Containment and Counterrevolution at Versailles, 1918-1919* (New York, 1967), 878.

the Salle Bellevilloise when he drew his famous distinction between the "exercise of power"—that is, the assumption of office by Socialists within the framework of existing institutions—and the "conquest of power."<sup>9</sup>

For the years 1921–33 fewer than one-fifth of Blum's daily editorials are reproduced—for 1929, sixteen of over two hundred speeches and articles; for 1930, forty-three of over 225 items; for the critical year 1933, fifty-four of a total of 318. Although his articles on the neo-Socialist schism are reproduced, his intervention at the important congress of July 1933 is missing. The close reader will note the omission of his editorials predicting that Hitler would never come to power—as well as the later editorial admitting his miscalculation.<sup>10</sup>

For the critical period from February 6, 1934 on, again only a small sampling of his speeches and writings is reproduced; for 1934 and 1935, somewhere between one-fifth and one-sixth. Omitted is his important speech of March 15, 1935, to the chamber, where, in debate with Paul Reynaud on foreign policy and rearmament, he served as spokesman for the Socialists in opposing the extension of military service from one to two years: "One plays into the hands of Hitler by stressing the rearmament of France and not the disarmament of Germany." Another striking omission is the attack in the three articles he wrote in 1934 on Charles de Gaulle's project for the creation of mechanized armored divisions, Blum fearing in the aftermath of the February 6 *émeute* that the professional corps of soldiers called for (*l'armée de métier*) could be used as a "praetorian guard" against the Republic.<sup>11</sup>

For the period of the Popular Front itself the coverage is good, and Blum's speeches as premier to the nation, the chamber, the senate, and his party are amply reproduced, including all relevant material on the Spanish Civil War as well as some retrospective judgments on that heart-rending crisis in which Blum's failure to exercise decisive leadership, no matter what the obstacles, cannot readily be forgotten. Of course he was then no longer writing his daily editorials, and his public speeches were widely circulated and available in various forms.<sup>12</sup> After his second government in March–April 1938 the coverage dwindles again, and only about one-sixth of his editorials and speeches are reproduced. As one approaches Munich, some articles that would have

<sup>9</sup> The speech to the party congress is reproduced in the pamphlet, Léon Blum et Paul Faure, *Le Parti Socialiste et la participation ministérielle: discours prononcés au congrès national extraordinaire du 10 janvier 1926* (Paris, 1926). It is almost as if the editors did not wish to couple Blum's name with that of his archrival of the 1930s (and later Vichy collaborationist); yet in the 1920s Faure was secretary of the party and second only to Blum in importance.

<sup>10</sup> See "L'Allemagne ne veut pas d'un régime fasciste!" *Le Populaire*, Mar. 14, 1932; "La défaite de Hitler," *ibid.*, Mar. 15, 1932; "La fin de Hitler," *ibid.*, Nov. 9, 1932; and for his admission of error, "Hitler au pouvoir," *ibid.*, Feb. 9, 1933. Missing also in the later volumes are Blum's retrospective reflections on the events of 1933; see, e.g., his editorials in *Le Populaire*, Jan. 15, 16, 18, 1940.

<sup>11</sup> "Soldats de métier et armée de métier," *Le Populaire*, Nov. 28, 1934; "Vers l'armée de métier," *ibid.*, Nov. 30, 1934; "A bas l'armée de métier," *ibid.*, Dec. 1, 1934.

<sup>12</sup> See especially the collection edited by Robert Blum, *L'Exercice du pouvoir: discours prononcés de mai 1936 à janvier 1937* (Paris, 1937).

conveyed the anguish of the crisis are not reproduced, including some glowing tributes to Neville Chamberlain before and after the conference itself.<sup>13</sup>

On other subjects, one looks in vain for the editorial praising the nomination in March 1939 of Marshal Philippe Pétain—"the most noble, most human of our military chiefs"—as the new ambassador to Franco's Spain. One does not find Blum's editorial of May 1939 calling for an end to the "rumors" of a rapprochement between Hitler and Stalin. The dignified way in which Blum, commenting on the vote of parliament, accepted the war when it came is not reproduced, nor for some strange reason is his article criticizing Edouard Daladier's failure to create a true war cabinet along British lines, nor his articles hailing Paul Reynaud's belated appointment of Colonel de Gaulle as undersecretary of war—"a man who understands the meaning of the new warfare and will find the proper remedies to resist it."<sup>14</sup> Blum's speech at Bournemouth to the Labour party conference in May 1940 is missing even though it was deemed important enough to appear in pamphlet form in an English translation.<sup>15</sup>

Momentarily skipping over the volume for the years of defeat and occupation, I have less serious reservations about the final two volumes covering the period 1945–50, where of about nine hundred articles and speeches almost half are reproduced. They adequately convey Blum's concern with the unrest and division in his party when Guy Mollet wrested control; the poignant disillusionment of the cold-war years; his early entry into the lists against de Gaulle's conception of "grandeur" and of the presidential system tailored to execute it; and his continuing struggle with communism. They show him valiantly trying to create a "Third Force" to protect the Republic against the double threat of "communism and caesarism"—the generation that had guarded the Republic in the Dreyfus affair and in the 1930s was still guarding the Republic after 1945.

The volume for the wartime years 1940–45, which appeared in 1955, has proved to be the only volume containing any significant unpublished material. The new material consists mainly of Blum's memoirs—"Mémoires (Fragments)"—for the critical three-month period May 15–September 15, 1940, completed in December 1940 and spirited from his prison cell. After his return to Paris in May 1945 Blum could not bring himself to publish the manuscript without revising it. Quickly caught up in active political life and—it is a fair guess—lacking the heart to revive the bitter memories of the spring and summer of 1940, he never succeeded in accomplishing the revision. The decision to publish the memoirs deserves commendation because

<sup>13</sup> For example, "Une noble audace dans la volonté de paix," *Le Populaire*, Sept. 15, 1938; "Tchécoslovaquie," *ibid.*, Sept. 29, 1938; and "L'accord de Munich," *ibid.*, Oct. 1, 1938.

<sup>14</sup> For the editorials and speeches referred to see "Le maréchal Pétain, ambassadeur," *Le Populaire*, Mar. 3, 1939; "Il faut en finir," *ibid.*, May 17, 1939; "Le vote du parlement," *ibid.*, Sept. 3, 1939; "Comité de guerre? Cabinet de guerre?" *ibid.*, Mar. 25, 1940; "Remaniement," *ibid.*, June 7, 1940; and "La guerre des chars," *ibid.*, June 8, 1940.

<sup>15</sup> Speech to Labour party conference, May 15, 1940, *Le Populaire*, May 16, 1940, published in English translation as *France at War* (London, 1940).

no other account has excelled Blum's masterful description of civilian reactions to the battle of France, the exodus from Paris, the events leading to the armistice, and the abdication of parliament at Vichy on July 9–10, 1940. On the other hand, the publication of the memoirs in a form that "without being truncated is nonetheless not absolutely complete" is less than reassuring. Certain passages have been omitted that, in his son's opinion, Blum would not have permitted to appear as written in 1940; the passages suppressed are categorized as episodes in which Blum did not personally participate but about which he learned from "third parties."<sup>16</sup> Although the relatively few excisions (about seven) do not interfere with the continuity of the narrative, less harm would have resulted from their inclusion with an editorial footnote. It can only be hoped that these passages, along with all other unpublished materials in the possession of family and friends, will be deposited for the use of scholars in the newly established Blum collection—the Fonds Léon Blum at the Fondation Nationale des Sciences Politiques; but there is no promise that they will be.

The second major part of the volume brings together the key documents in the Riom trial grouped in chronological order along with correspondence secreted out of Blum's cell by two remarkable women, Blum's daughter-in-law Renée Blum and Jeanne Levilliers Humbert, who became Jeanne Léon-Blum during the war years. The letters published here are among the very few available for his entire lifetime and reveal a remarkable courage and faith in the midst of the darkest adversity. In the appendix are portions of unpublished notes and sketches written in Germany as well as the dramatic recital of his evacuation from Buchenwald along with other special prisoners whose hostage value saved their lives until their rescue by American troops. Finally in this volume is to be found Blum's most important political essay, *A l'échelle humaine*, completed in prison in December 1941 and published in 1945. It is the very distillation of his political thought, a moving reaffirmation of his faith in democracy, humanistic socialism, and internationalism, and a reflection of his inveterate optimism. "When a man grows troubled and discouraged," are the last words of the essay, "he has only to think of humanity."

For all their shortcomings these nine volumes will make readily available to scholars and readers the major contributions of an outstanding man of letters and political leader who despite a generous share of weaknesses and imperfections displayed throughout his lifetime a remarkable integrity. The handsome photographs, one serving as frontispiece for each volume, show Blum at different stages of his life—from the young man about town in Paris of the 1890s to the serene elder statesman in semiretirement at Jouy-en-Josas in the late 1940s. The photographic reproductions of various manuscript pages show, by the very paucity of corrections and revisions, the ease and felicity with which he wrote. The prefaces to each volume and the brief edi-

<sup>16</sup> See Robert Blum's editorial note, *L'Oeuvre de Léon Blum*, 5: 1940–1945, 3–4.



torial annotations are extremely helpful. Given the practical constraints within which the editors found it necessary to operate, the declining fortunes of the French Socialist party in the last twenty years, and the less than enthusiastic support that the men of the Fifth Republic could muster for the men who championed the parliamentary democracies of the Third and Fourth, it is nothing short of miraculous that the venture was carried through to a successful conclusion. Yet for those who have studied in detail the career of Blum and the stormy times in which he lived, and will continue to do so, the volumes, though welcome, will be no substitute for examining every editorial, every parliamentary appearance, and every speech; nor will they interrupt the continuing search for unpublished materials. The "best of Léon Blum" with which we have been presented by those who have faithfully edited and produced *L'Oeuvre de Léon Blum* is good indeed, but it can be no substitute for the entire corpus of writings and speeches. But perhaps criticism should be better directed against a France and a world that can find vast sums for nuclear armaments but not for a publishing venture that in unabridged form might well have captured the history of an entire era; it is not the France or the world that Léon Blum had devoted his life to fashioning.

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## Society and Economy in the British Caribbean during the Seventeenth and Eighteenth Centuries

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A Review Article by JACK P. GREENE

CARL and ROBERTA BRIDENBAUGH. *No Peace beyond the Line: The English in the Caribbean, 1624-1690*. (The Beginnings of the American People, volume 2.) New York: Oxford University Press. 1972. Pp. xxii, 440. \$12.50.

MICHAEL CRATON and JAMES WALVIN. *A Jamaican Plantation: The History of Worthy Park, 1670-1970*. [Toronto:] University of Toronto Press. 1970. Pp. xi, 344. \$10.00.

RICHARD S. DUNN. *Sugar and Slaves: The Rise of the Planter Class in the English West Indies, 1624-1713*. Chapel Hill: University of North Carolina Press, for the Institute of Early American History and Culture. 1972. Pp. xx, 359. \$11.95.

ORLANDO PATTERSON. *The Sociology of Slavery: An Analysis of the Origins, Development and Structure of Negro Slave Society in Jamaica*. Cranbury, N.J.: Associated Universities Press. 1969. Pp. 310. \$8.50.

RICHARD SHERIDAN. *The Development of the Plantations to 1750 and An Era of West Indian Prosperity 1750-1775*. (Chapters in Caribbean History, number 1.) [Barbados:] Caribbean Universities Press. 1970. Pp. 120.

RICHARD B. SHERIDAN. *Sugar and Slavery: An Economic History of the British West Indies, 1623-1775*. Baltimore: Johns Hopkins University Press. 1974. Pp. xiv, 529. \$22.50.

"THE SUGAR COLONIES," the agricultural writer Arthur Young estimated in 1770, "add three millions a year to the wealth of Britain; the rice colonies near a million, and the tobacco ones almost as much." Young's estimation doubtless was imprecise, but his remarks vividly underline a conviction widely shared by his contemporaries: the Caribbean sugar islands were both the most valuable of the British colonies in America and a major source of wealth for the mother country. Whatever the view of eighteenth-century Britons, modern historians have not previously given these colonies an amount of attention anywhere nearly commensurate with their early importance. In this century there has been a trickle of monographs on the internal constitu-

tional development of the islands and their external relations with the mother country.<sup>1</sup> Except for the valuable studies of Eric Williams, J. Harry Bennett, and, especially, Richard Pares there has been remarkably little serious study of the economic development of the Caribbean colonies, while in the area of social history the impressive early works of Frank Wesley Pitman and Lowell J. Ragatz have been followed by almost forty years of nearly total neglect, perhaps in part because of the judgment of both of these scholars that, to the extent the West Indian colonists had succeeded in establishing a society at all, it was a "wilderness of materialism," a "degraded" and monstrous creation that bore little resemblance to the "healthy and progressive" social organisms found on the mainland, and, more particularly, in New England.<sup>2</sup> As a result of the sudden efflorescence of scholarship represented by the works considered here and a few other recently published studies, however, we are now on the verge of achieving a more systematic and thorough understanding of the economic and social development of the Caribbean colonies than we have for any other segment of the early modern British overseas empire—including perhaps New England. By making it clearer than ever before exactly what the new settlers—Africans as well as Europeans—created in the West Indies during the seventeenth and eighteenth centuries, as well as how and why they created it, this understanding provides for the first time a reasonably adequate basis for evaluating the judgments of Pitman and Ragatz.

INTENDED AS COMPREHENSIVE SOCIAL histories of the six major British West Indian islands—Barbados, Jamaica, and the four Leeward Islands of Antigua, Montserrat, Nevis, and St. Kitts—during the seventeenth century, the volumes by the Bridenbaughs and Dunn cover much of the same ground. Each volume recounts the familiar story of the original English settlement in the Lesser Antilles in the 1620s, the heavy English migration and search for a staple during the 1630s, the sugar revolution and the massive importa-

<sup>1</sup> Especially C. S. S. Higman, *The Development of the Leeward Islands Under the Restoration, 1660-1668* (Cambridge, 1921); Lillian M. Penson, *The Colonial Agents of the British West Indies: A Study in Colonial Administration, Mainly in the Eighteenth Century* (London, 1924); Vincent T. Harlow, *A History of Barbados* (Oxford, 1926), and Christopher Codrington, 1668-1710 (Oxford, 1926); Agnes M. Whitson, *The Constitutional Development of Jamaica—1660-1729* (Manchester, 1929); Richard Pares, *War and Trade in the West Indies, 1739-1763* (Oxford, 1936); A. P. Thornton, *West-India Policy under the Restoration* (Oxford, 1956); F. G. Spurdle, *Early West Indian Government: Showing the Progress of Government in Barbados, Jamaica and the Leeward Islands, 1660-1783* (Palmerston North, N.Z., 1964); and George Metcalf, *Royal Government and Political Conflict in Jamaica, 1729-1783* (London, 1965).

<sup>2</sup> Eric Williams, *Capitalism and Slavery* (Chapel Hill, 1944); J. Harry Bennett, *Bondsmen and Bishops, Slavery and Apprenticeship on the Codrington Plantations of Barbados, 1710-1838* (Berkeley, 1958); Richard Pares, *A West India Fortune* (London, 1950), *Yankees and Creoles: The Trade between North America and the West Indies before the American Revolution* (Cambridge, Mass., 1956), and *Merchants and Planters* (Cambridge, 1960); Frank Wesley Pitman, *The Development of the British West Indies, 1700-1763* (New Haven, 1917); Lowell J. Ragatz, *The Fall of the Planter Class in the British Caribbean, 1763-1833* (New York, 1928). The quotations are from Pitman, *The Development of the British West Indies*, 2, 39, 41.

tion of African slaves (beginning in Barbados during the 1640s and extending to the Leeward Islands and Jamaica after the Restoration), the subsequent drift toward monoculture and a declining ratio of Europeans to Africans, and the vigorous competition for possession of the islands by rival European powers over much of the seventeenth century. But they are very different books and represent two distinct, if also complementary, approaches to the study of past societies. One approach, of which Professor Bridenbaugh is one of the most skilled practitioners among American historians, may be referred to as "social history by example." For its effectiveness it relies chiefly upon an intelligent blending of illustrations from the surviving literary, pictorial, and material record. A second and newer approach, of which the Dunn book is a superb example, depends much more heavily upon the systematic presentation and analysis of existing quantitative data. Its success derives largely from the firmness of the data and the author's sensitivity in interpreting them. These divergent paths do, however, lead the authors to the same general conclusion. Echoing Pitman and Ragatz, as well as many contemporary commentators, they agree that, at least for the seventeenth century, the West Indian colonies were social failures.

Indeed, this is the central theme of the Bridenbaughs' study. Written from "the insular rather than the imperial point of view," this vigorously argued volume provides a comprehensive narrative of the economic and social development of the British Caribbean colonies from 1624 to 1692, with special focus upon the "lives of the people—white and black—[and] their outlook." At least in part the book is a success story. The authors admire the "astounding English vitality" displayed by the settlers and their extraordinary economic achievement. With the help of the Dutch, who provided expertise and capital, Barbadian planters in just twenty years, from 1640 to 1660, effected an agricultural and social revolution that was little short of spectacular, and, in doing so, they not only generated enormous wealth for many of themselves and their backers but established an economic model that could be—and was—transferred to the rest of the English islands. But the price of this "impressive material accomplishment," the Bridenbaughs argue, was nothing less than "social failure and human tragedy." The human tragedy can be measured in part by the enormous toll in human life, especially among the unwilling immigrant African slaves, who died in droves on slave ships and were worked so hard and mercilessly on the plantations that the mortality rate had reached at least as high as six per cent per annum by 1690.

For the Bridenbaughs, however, the even greater tragedy was the failure of the English settlers to establish "a sound white society." They went out to the islands determined "to improve their fortunes . . . and . . . to transplant as much of Old England as possible." But two related factors combined to prevent them from forming a "society resembling any in the Old World." First, the Bridenbaughs argue, although the "seventeenth century was not fundamentally a materialistic age" for the English, for the West Indian settlers

"it was little else"; and the "overweening greed for profit and a persisting overemphasis on things material" simply "prevented any successful rooting and growth of English civilization." Living in "a continual state of transition" and never committed to permanent residence, the white settlers, mostly young, male, and drawn from "a low grade set of people," with only a smattering of gentlemen leaders and no "substantial number of those middling Englishmen who figured at the same time in the building of the New England and Chesapeake societies," constructed only "inadequate institutions" and a "way of life" that was religiously and morally deficient and culturally and intellectually barren. Second, insular life was "blighted" by the massive importation of alien and unwilling African slaves. The "all-embracing difficulty" arising from this excessive materialism and the overwhelming numbers of alien Africans, according to the Bridenbaughs, was the "incomplete adjustment" of Englishmen "to the New World." The "family, the church, and the community"—"the prime institutions that had made English civilization what it was" and "provided the safeguards against barbarism"—"never grew to form a healthy, rounded, friendly society of white people." Coming from a similar climate in West Africa (and with no hope for escape) the blacks were better "able to live as families and to develop a genuine sense of community life which they expressed so fascinatingly in music and the dance." But the arrested social development of the whites, the Bridenbaughs insist, left the Caribbean islands in a condition that closely resembled Thomas Hobbes's state of nature with "no commodious Building . . . no Arts, no Letters, no Society, and, which is worst of all, continued feare and danger of violent death; And the life of man was solitary, poore, nasty, brutish, and short."

That the British West Indies were "disastrous social failures" during the seventeenth century is also the judgment of Dunn. Like the Bridenbaughs, Dunn has sought to produce "a composite portrait of English life in the Caribbean." As his subtitle implies, however, his primary concern is not with the failure to reproduce English institutions but with the "rapid rise of a cohesive and potent master class"—in Dunn's view, the "chief distinguishing feature of island society in the seventeenth century." Making ingenious use of maps, census materials, and a wide variety of other data Dunn describes the emergence, "practically overnight" in Barbados, of "the most perfectly articulated colonial aristocracy in English America" and of the formative stages in the slower development of similar groups in the Leeward Islands and Jamaica prior to 1713. The result is a study of major importance: the first systematic and extended account of the emergence and character of an elite group for any of the English colonies during the seventeenth and eighteenth centuries.

These scrabbling West Indian elites quickly created a "very durable social pattern" that, with its highly stratified social structure and great disparities in wealth and styles of life, bore a superficial resemblance to English

rural society, but it was one that "had no counterpart . . . elsewhere in the English experience." "The *one* outstanding attraction to life" in the islands, according to Dunn, "was the opportunity for making a quick fortune," and in the race for profits the rising elite rejected "most of the social values associated with the gentry in seventeenth-century England" at the same time it tried to cultivate many gentry habits that were entirely inappropriate to a tropical environment. By surrounding themselves with "hordes of restive black captives," whom they simultaneously "hated and feared," they created a situation for themselves that was so insecure as to make the islands "almost uninhabitable." Moreover, an alien climate and an unfamiliar and virulent disease environment turned the islands into "a demographic disaster area," where life expectancy was low, families were characteristically broken, deaths were usually in excess of births, and the number of young adults was extremely high. In these circumstances the plantation became a ruthlessly capitalistic institution. With little of the paternalism that helped to soften relations between landlords and tenants on English estates the sugar plantations mercilessly exploited the slaves on the conviction that "it was more efficient to import new slaves of prime working age from Africa than to breed up a creole generation of Negroes in the Caribbean." For those whites who survived, Dunn argues, there could be no permanent attachment to such a society. The only viable goal was escape, as the failures drifted off to other colonies and the successes either sold out and migrated or retired to England, leaving their estates in the hands of managers. The result was a society that was "radically different" from that of England: family structure never followed a normal pattern, traditional supportive social institutions were virtually nonexistent, there was a rapid circulation among the elite, and the entire white population was highly transient and unstable. With "a small cadre of white masters driving an army of black slaves," the British West Indies in 1713 more closely resembled a nineteenth-century industrial factory than a traditional European society.

In constructing this portrait of these "fast-living, fast-dying tropical" communities Dunn not only provides the most solid and precise account ever written of the social development of the British West Indies down to 1713, he also challenges some traditional historical clichés. Specifically, he argues plausibly that the extent of British migration to the Caribbean has been seriously exaggerated and "that the stream of migration to the mainland colonies was always larger, even before the English Civil War." More conclusively, he also shows that historians have similarly exaggerated the white depopulation of Barbados during the seventeenth century and the degree of concentration of landholding. With 20,000 whites in 1680, Barbados was exceeded in numbers only by Massachusetts and Virginia among all the English colonies in America, and the great majority of Barbados property holders at that date were still small farmers. Nor do Dunn's conclusions agree with those of the Bridenbaughs' in all respects. In particular he denies that "slaves

adjusted better than their masters to life in the tropics," that Negro family life was any less stunted than that of whites, or that slave acclimation to the tropics was a critical consideration in the shift from indentured to slave labor. Rather, Dunn attributes this shift largely to the slaves' availability, cheapness, and dependability as a labor force. More important perhaps is a subtle distinction in emphasis in explaining the social failure of the West Indian colonies. Whereas the Bridenbaughs attribute that failure to the "incomplete adjustment" of the English colonists to the New World, Dunn seems to trace it to the totality of that adjustment, to their almost complete capitulation to conditions of life they found in the islands and their successful manipulation of those conditions for a single purpose: material gain.

This harsh judgment of the Bridenbaughs and Dunn is reiterated even more forcefully by Orlando Patterson in *The Sociology of Slavery*, an inventive and perceptive book that, concentrating on Jamaica and covering the whole period from the beginning of British occupation to the emancipation of the slaves in 1838, goes well beyond those of either the Bridenbaughs or Dunn in the depth and scope of its treatment of slave culture. According to Patterson Jamaica not only failed to replicate its parent culture but was "a monstrous distortion of human society" chiefly characterized by "the astonishing neglect" and perversion "of almost every one of the basic prerequisites for normal living." The early attempts to establish "a *colonie de peuplement*" failed as Jamaica settlers abandoned all other considerations in the race for sugar profits. The result was cultural disintegration—the "almost complete disorganization" of the "values of both masters and slaves" and a society that "existed for the pursuit of one goal—that of making vast fortunes as quickly as possible from growing sugar." After 1730 "both a slave and a white creole society emerged," but by that time the society was so deeply materialistic and so malintegrated that whites were unable to perpetuate what little sense of "local patriotism" they had developed during the first half-century of settlement and fled the colony for Britain as fast as they could afford it, thus depriving the colony of "the wealthiest and most talented sector of the white creole group." The consequences of this deprivation of leadership for white society were profound: an utter lack of the educational and other social and cultural institutions of British society, "a complete breakdown of religion and morality," "the almost complete breakdown . . . of marriage and the family," "the gross mismanagement of the economic affairs of the island," and a repressive slave system that placed "total power" in the hands of the masters, with almost none of the mitigating agencies provided for dependents in other societies by state, community, and bureaucracy.

But Patterson's most important contribution is in describing the lives and culture of the slaves under this repressive regime. Making imaginative and resourceful use of a wide variety of sources Patterson provides much hard information and a series of intriguing hypotheses about such topics as the structure and disposition of the work force, conditions of life and work, the

tribal origins and social institutions of the slaves, the "slave personality," and patterns and meanings of slave resistance. He challenges the conventional belief that "Gold Coast Negroes dominated the rest of the Africans," arguing instead that the survival of so many Gold Coast cultural elements in modern Jamaica is attributable to their insistence upon keeping to themselves and that, in any case, "the Akan and Ga-Adangme peoples of the coastal strip of Ghana" were able, as the largest component of the slave population for the first fifty years, "to impose their own patterns of behavior and speech on the creole slave society which was then in its nuclear stage." Virtually every ethnic group from West Africa was represented among Jamaican slaves, and this complex ethnic diversity continued to be a divisive social force among the slaves throughout the slave period. Eventually, ethnic diversity became less important than the larger distinction between Africans and creoles (American born). Although the slaves were not yet able to reproduce themselves and large numbers of Africans had to be imported to maintain a stationary level among the slave population, by 1760 the mortality rate had decreased and the birth rate had increased enough for creoles to outnumber—and dominate—the African-born. On plantations slaves were further divided according to place and character of work, with domestics, skilled workers, and drivers having a higher status than field hands.

An important result of the creolization of this fragmented population, between 1730 and 1780, was its successful adjustment to the harsh conditions of slave life. The creoles had learned English, mastered their roles in the system, and developed patterns of behavior through which they could "best adjust" to their thralldom. The predominance of the Quashee personality syndrome, which in its manifestations of a persona of childlike inefficiency, frivolity, and ignorance bears a marked similarity to the American Sambo as described by Stanley Elkins,<sup>3</sup> is a case in point. But Patterson, in contrast to Elkins, stresses the extent to which the slaves in assuming the Quashee mask were simply catering to white stereotypes for their own ends—according to the Jamaican proverb, playing the "fool to catch wise"—rather than actually internalizing them. In any case adjustment did not mean acceptance. Servile revolt was both "continuous and intense": among American slave societies, only Brazil may have experienced more frequent or larger scale revolts. Moreover, as Patterson skillfully shows, slave songs and folktales reveal a sharply developed and persistent "sense of injustice and persecution." Despite the repressiveness of the system all slaves had some space of their own. Most slaves had a half acre of ground assigned to them on which they grew their provisions, and the custom of the country permitted them to sell any surplus at regional Sunday markets, which brought the slaves from neighboring plantations together. Because they were farthest removed from their white masters, field slaves, Patterson seems to be implying, may have had

<sup>3</sup> Stanley Elkins, *Slavery: A Problem in American Institutional and Intellectual Life* (Chicago, 1959).



more privacy, more latitude, and less pressure to socialize to white norms. This space and scope permitted the slaves, as individuals, to preserve some sense of individuality and integrity and, collectively, to maintain some elements of their African religions and to develop seasonal recreations to a high level.

Yet, Patterson stresses, the most vivid effect of the slave society of Jamaica upon both slaves and masters was its overall destructiveness. Legally the slaves had "no civil character, no personality," and the whole system constituted an overwhelming and "constant onslaught on the self-dignity and pride of slaves" and led, Patterson argues not entirely persuasively, to the "complete breakdown of all major institutions—the family, marriage, religion, organized morality"—the slaves had brought with them from Africa. Only after emancipation was there any significant reversal in these destructive tendencies—the result of two autonomous developments: the consolidation and amplification of the Afro-Jamaican cultural system that had begun to develop under slavery and "the revival of British civilization in the island after its disintegration under slavery."

BY PLACING THE BRITISH CARIBBEAN colonies in their general economic context and a longer time perspective Richard B. Sheridan in his two volumes has both amplified and modified the findings of Patterson, Dunn, and the Bridenbaughs. The product of over twenty years of research and an unparalleled mastery of existing sources, *Sugar and Slavery* is the most comprehensive and authoritative study yet published on the socioeconomic development of the early British Caribbean. Like the Bridenbaughs and Dunn, Sheridan is concerned with Barbados, Jamaica, and the four Leeward Islands. Unlike them he carries his story down to 1775, devoting more space to the eighteenth century. Admirably succinct, Sheridan's two essays for the new series *Chapters in Caribbean History*, which eventually will constitute a cooperative history of the Caribbean in fifty chapters,<sup>4</sup> summarize most of the main findings of the larger work and place them in a comparative framework that includes consideration of the French islands of Martinique and St. Domingue as well as the Spanish island and mainland colonies.

Sheridan focuses upon the organization and operation of the sugar plantation and the role of the sugar colonies in the emerging Atlantic economy, and his primary thesis is "that, however inhumane, the sugar industry made a notable contribution to the wealth and maritime supremacy of Great Britain." The "economic growth of Great Britain," he argues, "was chiefly from without inwards," "the Atlantic was the most dynamic trading area," and, next to the metropolis, "the most important element" in the growth of the Atlantic prior to 1776 "was the slave-plantation, chiefly of the cane-sugar

<sup>4</sup> Previously published in the same series are E. V. Goveia, *The West Indian Slave Laws of the 18th Century*, and C. J. Bartlett, *A New Balance of Power: The 19th Century* (Barbados, 1970).

variety in the islands of the Caribbean." By generating new trades and shipping, shifting "millions of hoe cultivators from one side of the Atlantic to the other," redirecting the movement of capital, stimulating the production of intermediate products in the temperate-zone colonies, and creating a wealthy class of planters and merchants, the plantation, Sheridan contends, "was truly an innovation in the Schumpeterian sense." Thus the sugar industry not only became the "chief source of new wealth" (much of it channeled into Britain) during the seventeenth and eighteenth centuries, it also helped, as the most important sector of the new colonial export economies, to give rise to a variety of economic linkages that in turn induced changes in productive techniques and organization within the home islands. More than "elements, indigenous to the domestic economy," Sheridan believes, these changes were critical to the emergence of the Industrial Revolution.

Sheridan is especially effective in tracing long-term fluctuations in the economic and political context of the sugar boom. Prices fluctuated, showing a consistently higher trend only during the periods 1689-1713 and 1734-58, but both the volume and market value of sugar products moved inexorably upward over the whole period 1643-1775. This movement reflected a continuous expansion of the sugar industry, the result of (1) a steady increase in per capita sugar and tea consumption throughout the Anglo-American world so that supply never quite caught up with demand, (2) the almost total exclusion of foreign sugar from the British market, and (3) the rising political influence of the West Indian "interest" in British politics. Early opposition to the Navigation Acts gave way to demands for strict enforcement as the West Indians won one concession after another from the imperial government, concessions, one contemporary estimated, that brought them £8 million in profits during the thirty years from 1730 to 1760! Fixed capital costs in labor, buildings, and machinery, as well as depreciation costs (mainly the result of high replacement rates for slaves), were much higher than for any other colonial agricultural industry, and there were no important changes in the technology and methods of cultivation, processing, or transportation. Nevertheless, expansionary tendencies were so strong that profits were extremely high—during the mid-eighteenth century up to 8.5 per cent in newer areas in good years and no lower than 4 per cent in older colonies like Barbados.

The longer time perspective permits Sheridan to chart important temporal and spatial variations in the developmental sequence described by the Bridenbaughs and Dunn. During the eighteenth century Barbados continued its inexorable movement toward "a capital-intensive, power-intensive system of agriculture conducted on a sustained-yield basis," as declining soil fertility and higher processing costs required more and more capital and labor to yield ever diminishing rates of return. But the drive toward intensive monoculture and many of the tendencies associated with that drive either lost vigor or changed in character between 1700 and 1775. An actual turning

away from sugar to livestock was manifest as early as the 1730s, and the movement toward property consolidation had leveled off by 1750, with roughly a third of the proprietors owning somewhat more than half of the estates and windmills. Although white migration continued through the 1740s, there was a reversal in the formerly steady decline in white settlers after 1710. Over the next sixty-five years the number of whites grew by about 50 per cent, to 18,500 in 1773. The slave population continued to rise, doubling over the same period, but imports, which remained fairly high, accounted for a declining proportion of the slave population. Annual mortality rates among slaves declined from 6 per cent in 1700–25 to 3.8 per cent in 1750–75, the result, Sheridan surmises, of a growing ratio of creoles to the total number of slaves and better diet and health care, as, with declining profits, it became more profitable to breed slaves locally than to import new ones.

For reasons also described by Dunn the heavy influx of African slaves, large-scale property consolidation, loss of white settlers, and intensive concentration of sugar experienced by Barbados during the half century after 1640 took place in the Leeward Islands mainly after 1713 with Nevis, which had already experienced substantial development in that direction, leading the way, followed by Montserrat, St. Kitts, and Antigua. But these islands differed from the Barbados model at least prior to 1775 in that there was no turning away from monoculture and no reversal in the decline of white settlers. In Nevis and Montserrat there was a steady loss from the 1670s to a low point in 1745, followed by a slight rise over the next decade and a continuation of the downward trend thereafter. In St. Kitts and Antigua, which developed later, white population continued to climb in the 1720s and then dropped slowly but steadily thereafter. Because the black population tripled in all four islands between the second and seventh decades of the eighteenth century, the ratio of blacks to whites was much higher than in Barbados—15 to 1 in Antigua, 12 to 1 in St. Kitts, 11 to 1 in Nevis, and 7.5 to 1 in Montserrat—with the result that all of the Leeward Islands were little more than sugar factories with a few white managers and a large gang of black workers. Far more than Barbados they had been transformed by 1770 from colonies of settlement to colonies of exploitation with an impoverished cultural and political life of the kind attributed by the Bridenbaughs and Dunn to all of the islands by the end of the seventeenth century.

Despite many similarities Jamaica diverged considerably from the patterns exhibited by the smaller islands. Although much larger it did not export as much sugar as Barbados until early in the eighteenth century, and it continued to grow slowly from 1713 to 1740 because of the secular decline of the sugar market, the engrossment of lands by large holders, an inadequate slave supply, and the fierce opposition of the Maroons, bands of runaway slaves who terrorized outlying areas of the colony, especially between 1725 and 1739. After the cessation of hostilities with the Maroons and in response to a

rising sugar market, Jamaica experienced a spectacular growth from 1740 to 1775 as the number of slaves and sugar plantations doubled. By 1775 Jamaica was exporting ten times as many sugar products as Barbados and had three times as many slaves. Over the same period the aggregate value of the colony's economy increased from just over £3.5 to over £15.1 million. But this rapid expansion produced significantly different results from those arising from the similar development of Barbados a century earlier or of the Leeward Islands a half century before. Jamaica never became a sugar monoculture. Four out of ten slaves were in nonsugar production, and more than a half of the plantations were devoted to livestock, provisions, and minor staples. Similarly, there was still much uncultivated land and considerable land wastage in Jamaica, where the plantation economy was more land-intensive and less labor- and capital-intensive. Moreover, Jamaica experienced no loss of white population, which increased slowly but steadily from 7,000 in 1703 to over 18,000 in 1774. Also slave mortality was somewhat lower, ranging from 4 per cent down to 2 per cent annually, the probable result, Sheridan thinks, of better dietary standards deriving from the allowance to each slave of a small plot of provision ground and one and one-half days per week for his own activities. Finally, Jamaica slaves developed a vigorous internal marketing system, and the free colored population of Jamaica exceeded that of Barbados by 10 to 1.

In several important respects Sheridan's findings strongly suggest that the picture of emerging Caribbean society, as drawn by the Bridenbaughs and Dunn for the end of the seventeenth century, requires some modification. On the question of absenteeism, Sheridan argues that, although it was present from the beginning of sugar culture, it did not become "a movement of consequence until the eighteenth century." In the Leeward Islands a substantial number of proprietors may have been absentees—in St. Kitts perhaps as many as half by the early 1730s. Barbados and Jamaica never had such large proportions, although during the silver age of sugar after 1740 up to 30 per cent of sugar plantations in Jamaica belonged to absentees. But prior to 1775, Sheridan emphasizes, absentees "were only a fraction of the Britishers who remained in the tropics," albeit a highly visible fraction because of their disproportionate wealth and influence in the British government. Absenteeism, Sheridan agrees, "drained away wealth and income that might otherwise have gone into public and private improvements" and "contributed to the impoverishment of political and social life" in the islands, but along with continuing, if probably somewhat declining, high mortality rates, it also functioned to keep avenues of social mobility open. Indeed, throughout the eighteenth century, white society was more open and more attractive to white immigrants than might have been suspected from the extent of property consolidation and tendencies toward monoculture and white population loss that were so strongly manifest by 1713. Even in the older and smaller islands, Sheridan shows, "each generation witnessed the rise of

new men and at times the establishment of new family dynasties alongside the stagnation and decline of planters whose indebtedness and absenteeism [or death] made their estates ripe for the plucking." Some enterprising immigrants acquired instant wealth through marriage, while others first accumulated the capital necessary to purchase a plantation through trade, office, or the law. In Jamaica, where there was uncultivated land as late as the 1760s, it was still possible for those with sufficient capital to establish a sugar plantation from scratch or for those with fewer resources to begin with minor staples and build up a sugar estate gradually from "reinvested profits"—apparently the most common pattern of estate building in the Caribbean throughout the period from 1640 to 1775. In any case opportunities were sufficient so that many planters rose up from the lower and middle ranks of society with "one stream of recruits" coming from "the professional, administrative, and especially the mercantile groups in the colonies" and a second "from subordinate managerial personnel on plantations." Many of these recruits seem to have come not from the creoles but from the newcomers and especially from the Scots, who came in large numbers after 1710. Finally, Sheridan adduces considerable evidence that, even in terms of the Eurocentric standards imposed by the Bridenbaughs, Dunn, and Patterson, Britain's Caribbean colonies were not the total social failures those writers suggest. Sheridan does not deny that the social costs of the "sugar lottery" were burdensome or that "by European standards of the time" the sugar colonies "were notoriously deficient in education, social services, and public improvements," but he does suggest that the stereotyped images of the West Indies as a social wasteland and the planter "as an improvident, indolent, and sensuous gentleman" are both one-dimensional and "in need of revision." He shows that the quality of plantation management was improving after 1750; many planters were obviously hardworking and thrifty; religious and cultural factors influenced white behavior; the professions, especially the law, were well developed and "middle-class mercantile and professional men exerted an influence that was disproportionate to their numbers"; "family life coexisted with bachelorhood"; and "it was not unusual for families to remain in possession of plantations for many generations."

One family of large planters who did not flee the Caribbean prior to the 1780s was the Price family of Jamaica, owners of Worthy Park, one of three Jamaica sugar estates with a continuous history of three hundred years and the subject of Michael Craton's and James Walvin's useful microstudy *A Jamaican Plantation*. Commissioned by the present owners to commemorate the tricentennial of the plantation the volume follows the history of the estate beyond emancipation in 1834 through a long period of decline and three changes in family ownership to its revival and expansion under the Clarkes between 1913 and 1970. But more than two-thirds of the volume is devoted to the history of the estate during the first half of its existence. In the experience of Worthy Park and the Price family one can see how the

general developments described by Dunn, Patterson, and Sheridan were refracted through the experience of one estate and one family. Most specifically the history of the Prices illustrates the life cycle of a great Jamaica sugar family.

Establishment of the family fortune, as Sheridan suggests was generally the case, was a slow process. Francis Price, founder of the fortune and a veteran of Cromwell's army, had had a small estate on which he raised indigo, cocoa, and a little sugar for seven years before he acquired by patent in 1670 the original 840 acres of Worthy Park in a lush but remote inland valley. Over the next nineteen years Price prospered. He rose from lieutenant to major of militia, twice served as member of the assembly for St. John's Parish, established a fruitful business connection with Peter Beckford, "co-founder of the largest of all Jamaica fortunes," and made an important dynastic connection through the marriage of his daughter to Francis Rose, scion of another emerging planter family. He also cleared fields, built roads and a great house, and acquired another 900 acres at Worthy Park. But he did not accumulate enough capital or labor to turn Worthy Park into a sugar plantation. At his death in 1689, it was still devoted to provisions and livestock and was "an extremely modest pioneer farm, such as might have been found in the backwoods of Virginia or the Carolinas at much the same time." The spectacular growth in the fortunes of the Price family came over the next two generations. Charles Price, Sr., who died in 1730, turned Worthy Park into a sugar estate and became one of Jamaica's "more substantial planters." Aided by the favorable economic climate after 1713 and his own great energy and enterprise, he accumulated an estate worth over £100,000, including a house in Spanish Town, the seat of fashion and power, and, though he lived all his life in Jamaica, he sent his children to England for their education. Whereas Charles Price, Sr., "was chiefly notable as an estate builder" and only dabbled in public life, his son Charles Price, Jr., became the most prominent political figure in the colony at the same time he was expanding family holdings in land and wealth. Indeed, he combined "territorial megalomania with an exaggerated sense of duty." Known to his contemporaries as "The Patriot," he was a member of the Assembly for thirty-one years and speaker for eighteen years (beginning in 1745), the leading spokesman for the Spanish Town or sugar planter interest in the island, and for four years before his death in 1772 a member of the Council. Political influence brought access to land and public works projects that raised the value of his holdings, and his activities as a speculator and developer were on a scale unequaled in the Anglophone Caribbean. At his death he owned 26,000 acres, "perhaps the largest portion of Jamaica ever owned by a single individual," and 1,800 slaves, one per cent of the entire slave population of the island. Returns were so large during the silver age of Jamaica sugar after 1745 that he was relatively free from the scarcities of capital and labor that had limited his father and grandfather. He built a costly aqueduct at

Worthy Park, an expensive town house in Spanish Town, and an elaborate country house. Yet he did not avoid the Jamaica malady of expanding his holdings far beyond his capacity to finance them. At his death his real estate empire was "staggering under the weight of mortgages." Charles Price, III was unable to save much of his father's estate in the unhappy economic climate after 1775 and finally deserted Jamaica for England in 1787, the first Price in the main line of the family to become an absentee. Although the Price family fortunes revived briefly in the 1790s, under the vigorous stewardship of Charles's heir and cousin, Rose Price, and the family managed to retain control of Worthy Park down to 1863, the fortunes of the family in Jamaica subsequently ebbed and flowed according to general economic conditions and the quality of management.

To the extent that the experience of the Price family is revelatory of broader trends, it may be used to evaluate the conclusions of the more general works considered here. In at least two senses the harsher assessments of the Bridenbaughs, Dunn, and Patterson would seem to be confirmed. First, Jamaica, if not quite a demographic disaster area, was clearly unhealthy for whites as well as blacks. Among the first three generations of Prices, life expectancy was only twenty-four (though for those who reached adulthood it was dramatically higher), while the slaves at Worthy Park continued to suffer a natural decrease through the 1780s and 1790s long after the balance among them had shifted from Africans to creoles. Second, beginning with the third and fourth generations the Prices were to some degree guilty of the extravagance usually attributed to Caribbean planters, devoting some share of the family's resources to conspicuous consumption, including the assignment of an excessive number of slaves to domestic statuses. But in many other respects the experience of the Prices deviates sharply from the projections of Dunn and the characterizations of Patterson. The first four generations revealed none of the sloth of the planter stereotype; no serious improvidence appeared until the third generation—and even then in an attenuated form. Indeed, the energy and industry of the first three generations were as impressive as those of any Boston or Philadelphia merchant family. Similarly, the Prices did not become absentees until the late 1780s, and even then they were not the stereotyped absentees who fled Jamaica as soon as they had acquired "sufficient wealth to live in ostentatious luxury abroad," but rather, in the authors' words, the "battered" victims "of an implacable system, seeking relief from the daily mounting weight and tension of plantation debt, in abdication." The behavior of the Prices in this respect raises questions not only about the authors' judgment that sugar wealth for the first Charles Price was primarily "a means of escape" from the island, but also about the conventional wisdom concerning the extent, timing, and causes of the major flow of absentees from Jamaica. Clearly, the deliberate choice of Charles Price, Jr. to remain in Jamaica was not dictated by the lack of means to leave. On the contrary, along with other aspects of his behavior, including his

building of elaborate houses in Spanish Town and at his country estate, his massive reinvestment of profits in capital improvements and labor for his estates, and his devotion to public life, it shows a degree both of commitment to the island and of local patriotism that do not easily fit the clichés about planter behavior. Far more closely than these clichés the history of the Price family would seem prior to its eventual—and perhaps reluctant—abandonment of Jamaica in the 1780s to have resembled the experiences of the great planting families on the continent—the Carters, Robinsons, Randolphs, and Lees in Virginia and the Pinckneys, Bulls, and Smiths in South Carolina—with the early generations laboring to build a large estate, later ones playing an increasingly prominent role in politics, and still later ones failing—in either the economic or political realms—to match the achievements of their progenitors.

An even more direct challenge to the view of the British West Indian colonies as social failures has been issued by Edward Brathwaite in *The Development of Creole Society in Jamaica, 1770–1820*.<sup>5</sup> Far from being merely “a loose ‘collection of autonomous plantations,’” he argues, Jamaica “had developed, from the beginning of its history, an establishment of governmental and social institutions capable not only of organizing and controlling life within its territory, but comparable, in many ways (at least up to the American Revolution), to similar institutions on the mainland of British North America.” Although he does not explore the composition of white society for late eighteenth-century Jamaica so thoroughly as Dunn does for late seventeenth-century Barbados, Brathwaite shows that it was not limited to a handful of resident managers of large sugar estates. As many as a fifth of island whites were from large landholding or wealthy and substantial mercantile or professional families. In addition there were many small planters and urban artisans, clerks, or shopkeepers, as well as estate managers. Unlike the Leeward Islands and perhaps Barbados, Jamaica managed to sustain a “self-conscious, articulate, cohesive social class of proprietor-administrators” well into the later eighteenth century. Like most colonial ruling groups its orientation was much more practical than esthetic, and its primary capital—and social—investment in the island was in the form of material improvements such as roads, bridges, public buildings, and forts. But the members of this class were not yet “passengers only.” They were “creoles” in the fullest sense of that term: that is, they were “committed settlers” who supported an active press; built churches, schools, and hospitals; and exerted political and social control through dynamic and self-conscious political institutions, especially the Assembly, the “most perfect expression of (white) creole society.” The grand houses they built in growing numbers after 1750 mark the emergence “of a creole style, a Jamaican ‘vernacular,’” that makes it apparent, Brathwaite argues, “that considerable

<sup>5</sup> (Oxford, 1971).



effort was [still] being made . . . to 'civilize the wilderness'" much like that of wealthy North Americans in their own rural settings of the same time. The political attitudes of Jamaicans and continentals were strikingly similar in the years prior to the American Revolution, and what primarily distinguished Jamaica from the mainland plantation colonies was not, as Brathwaite supposes, less "significant cultural development" or the absence of a desire to reorder society, but its relatively greater vulnerability to imperial military might and economic sanctions. Only after 1776 was the vigor of white society weakened, Brathwaite suggests, as the American Revolution in many ways isolated Jamaica "from the wider English-speaking New World area of which it was a part" and the humanitarian revolution challenged the very foundations of Jamaican society and sapped its self-confidence. Together these revolutions thus pushed the island into ever greater dependence upon "the essentially 'absentee' cultural and material influence of the Mother Country" and a revulsion against creole forms and institutions.

But the most important contribution of Brathwaite's book is not in showing that even within the Eurocentric perspective assumed by the Bridenbaughs, Dunn, and Patterson, Jamaica's "social failure" came a full century later than those writers have suggested, but in proposing a new frame of reference for approaching the analysis of colonial societies. Like Patterson and Elsa Goveia, whose earlier study of the slave society of the Leeward Islands<sup>6</sup> served as an admirable model for Brathwaite's study, Brathwaite analyzes in detail and with sensitivity the culture of Jamaica's African population. He argues, in part, in a brilliant chapter on the folk culture of the slaves, that Jamaica blacks developed and maintained a powerful "'little' tradition" in a rich variety of contexts. Afro-creole life, he maintains in a significant elaboration of Patterson, was not confined to the regimes of the sugar plantation and the routines of domestic service. The number of freedmen was not inconsequential and, although many of them lived in isolation near the borderline of poverty, others were small planters, fishermen, pilots, overseers, clerks, artisans, shopkeepers, schoolmasters, and builders. Among the slaves there were mechanics, tradesmen, preachers, seamen, woodsmen, and higglers who worked independently and whose activities provided them with considerable scope for privacy and individual autonomy. But Brathwaite's main point is not simply that, despite the internalization of a belief in Negro inferiority among slaves and freedmen, an Afro-creole tradition was able to survive and even flourish in a complex structural context. Rather, it is that that tradition constantly interacted with the dominant, if weak, Euro-creole tradition to produce a culture or way of life that was distinctively Jamaican, albeit it was also "part of a wider New World or American Culture complex," and "essentially different from the metro-

<sup>6</sup> Elsa V. Goveia, *Slave Society in the British Leeward Islands at the End of the Eighteenth Century* (New Haven, 1965).

politan model." To understand this process, Brathwaite emphasizes, we must view "white and black, master and slave," not as "separate nuclear units, but as contributory parts of a whole," as "two cultures of people, having to adapt themselves to a new environment and to each other." We have to think in terms not of acculturation—a one-way process—of black people, but of transculturation—a process of exchange—between blacks and whites. Enormously intricate, this process was most intense at points of most direct and continuous contact—at markets and army camps and in the great houses—and among groups on the boundaries between black and white society—among mulattos and domestics in the Afro-creole group and among whites "at the book-keeper or 'walking buckra' level." But it was pervasive. Among blacks it was most clearly manifest in their learning of the master's language and work routines, their identification with local symbols of authority and place of work, and, ultimately, among elite blacks and free coloreds, their imitation of the whites and rejection of many aspects of their African heritage. For whites it was apparent in language, food, dress, amusements, and sexual relationships, although the pull of the metropolis and the need to justify slavery were so powerful as to prevent them from explicitly embracing or coming to terms with Jamaica's Afro-creole tradition and to force them to cling desperately to a "bastard metropolitanism." Nonetheless, Brathwaite stresses, despite white and black resistance, the debasements caused by slavery, and the excessive imitativeness of Jamaican life an entirely "'new' construct" emerged in colonial Jamaica that was viable and creative even while no group within it managed to appreciate its creativity.

FAILURE TO APPRECIATE THIS creativity has persisted in modern scholarly judgments that the Anglo-Caribbean colonies were "social failures" or "monstrous distortions of human society." Such assertions, first advanced among modern scholars by Pitman and Ragatz and now echoed by the Bridenbaughs, Dunn, and Patterson, are patently ethnocentric, specifically Eurocentric. The question is not whether European immigrants to those colonies succeeded in establishing societies, but what kind of societies they—along with the much larger stream of immigrants from Africa—fashioned. Some colonial societies approximate to the metropolitan model of their dominant members more closely than others, and various groups within colonial societies actively cultivate many features of that model. By definition, however, colonial societies are not metropolitan ones: at most, they are no more than moderately strong reflections of the metropolis. As Dunn's work in particular so skillfully illustrates it is illuminating to contrast colonial societies to the society of their metropolis. But we may never fully understand the nature and range of colonial societies in the early modern or any other period until we stop evaluating them in terms of the standards of the metropolis and recognize that they constitute a related but sig-

nificantly different category of societies. To one degree or another each colonial society is a new society that exists within a symbiotic relationship with one or more metropolitan societies. But it also exists within a distinctive and confined ecosystem and is profoundly influenced by a number of factors, including especially the organization of its economy, the virulence of its disease environment, and the ethnic composition of its population; and the necessity of adapting to a new environment and, in many cases, to a polyethnic milieu requires a process of cultural reformulation and adaptation—to use Brathwaite's phrase, "of creolization"—that produces perceptions, institutions, social forms, and modes of behavior that invariably deviate from those of its metropolis at the same time, of course, that many of its members are striving to keep such deviations to a minimum.<sup>7</sup> For this reason it is misleading—and pointless—to condemn a colonial society for not reproducing the society of its metropolis. A more promising approach would seem to be to look at the often subtle and, as Brathwaite so strongly underlines, inevitably creative process of reformulation and adaptation against the comparative background supplied by not only the metropolis but also other colonial societies across space and time.

From such a perspective, it will become clear that, as the Bridenbaughs, Dunn, and Patterson have emphasized, the Anglo-Caribbean colonies did have social configurations that differed in many important respects from traditional England. But it will also become apparent that, as Sheridan reminds us, those configurations were always changing in response to a variety of exogenous and endogenous factors and were by no means unique. Rather, they were simply an Anglophone variation of a more general south Atlantic pattern that stretched from southeast Brazil north to the Narragansett Bay, a pattern chiefly characterized by the systematic exploitation of some people—mostly Africans—for the economic benefit of others—almost entirely Europeans. What articles were produced at what profit, how readily immigrant populations could become self-sustaining, and how fully the social features and processes of the metropolis could be replicated varied from one ecological zone to another. But every society within this system was, to a considerable degree, exploitative and materialistic, while most were also markedly polyethnic. Within the early modern colonial Anglophone world the Caribbean colonies were doubtless the most fully exploitative and the most thoroughly materialistic at every stage of their development. But from the beginning the dominant impulse was material in all of the colonies from New York south; even in New England the quest for profit was never weak and became increasingly vigorous during the eighteenth century. Except perhaps in the Leeward Islands, where by the mid-eighteenth century the white society was little more than a handful of loosely or-

<sup>7</sup> See Jack P. Greene, "Search for Identity: An Interpretation of the Meaning of Selected Patterns of Social Response in Eighteenth-Century America," *Journal of Social History*, 4 (1970): 189-220.

ganized plantation managers, the material impulse, as Brathwaite shows for Jamaica, was never so strong as to crush the complementary desire, to use the Bridenbaughs' phrase, "to transplant as much of Old England as possible." For a number of reasons that desire was doubtless more difficult to realize in the Caribbean and the coastal areas of the Carolinas and the Chesapeake: a more virulent disease environment meant that immigrant populations—European as well as African—took longer to become self-sustaining; higher returns per unit of labor meant that the proportion of Africans, the most available source of durable labor for tropical and semi-tropical zones, to Europeans was greater than in more northerly areas; and larger profits meant that more European settlers could re-emigrate to Britain. But neither in their materialistic orientation, their disease environments, their number of African inhabitants, their concern to cultivate British values and institutions, nor perhaps even their commitment to the colony was there a sharp break between island and mainland societies. Rather, there was a social continuum that ran from the Caribbean through Georgia and South Carolina to the Chesapeake through Pennsylvania and New York to urban and then rural New England. The social contrast between a sugar plantation in Barbados and a small homogeneous farming community in New England was considerable. But it would no doubt have been less apparent to a contemporary traveler had he proceeded not directly from one to the other, but through a series of intermediate stops along the coast.

Thanks to the works here considered, and especially to the careful quantitative analyses of economic and social data by Sheridan and Dunn, the imaginative reconstructions of Afro-creole life by Patterson and Brathwaite, and the detailed analysis of the experience of the Price family by Craton and Walvin, we now have a fuller picture than ever before of the Caribbean end of this social continuum and a solid basis for the construction of a clear typology of the societies of the Anglophone American world of the seventeenth and eighteenth centuries, a typology built on the recognition that colonial societies must be described and assessed in terms of the constrictions and possibilities inherent in them.

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## Reviews of Books

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### GENERAL

NORMAN J. G. POUNDS. *An Historical Geography of Europe, 450 B.C.-A.D. 1330*. New York: Cambridge University Press. 1973. Pp. xiv, 475. \$24.50.

Norman J. G. Pounds, professor of geography and history at Indiana University, has completed the first of a two-volume work on the historical geography of Continental Europe—excluding the British Isles and Russia. The subject matter is a portrayal of the complex of natural and man-made features that forms the stage setting of historical events, a stage setting that influences the course of history in many subtle ways and that is itself modified by human action. The results of this interaction are synthesized for five critical periods between 450 B.C. and A.D. 1330. The five periods are: the middle of the fifth century before Christ, when Athens was at the climax of its power and prestige; the middle of the second century after Christ, when, during the reigns of Hadrian and the two Antonines, Rome achieved for its citizens the greatest measure of individual well-being; the early ninth century, during the reign of Charlemagne; the years around A.D. 1100, at the beginning of a major period of population increase; and about A.D. 1330, just before the increase of population was stopped by the epidemics known as the Black Death. The second volume will start with the early sixteenth century and carry the story to the middle of the nineteenth century.

For each historical period six major topics are examined: the pattern of politically organized areas and the interrelations among states; the population, including density and demographic condition; the patterns of settlement, rural and urban; the agriculture; industry and mining; and, finally, trade. It is possible to follow the

developments in any one of these fields by reading the sections that deal with them in each historical period in sequence.

Climatic fluctuations are an important part of the story. Since about 25,000 B.C., when the last Ice Age was at its maximum, the climate of Europe has been ameliorating, but with periodic returns to cooler and wetter conditions. These climatic fluctuations, and also the changes of the shorelines, have affected the limits of human settlement, the productivity of agriculture, the density of population, and other conditions. But Pounds is cautious about jumping ahead to conclusions concerning the role of climatic change until more objectively tested evidence becomes available. His work, therefore, has the touch of a highly competent scholar.

I found the description of Rome in the age of the Antonines of special interest. In this city, the greatest in the world at that time, the lack of any effective central planning authority left each citizen free to place his house where he could find open space, without reference to any pattern of roads or avenues. And when open space was not available, new floors were added on top of already shaky structures. The result was incredible congestion in the central area, together with repeated crises in the supply of food and water. The solution in A.D. 64 was to burn the place down and dump the rubble into the marshes along the lower Tiber.

PRESTON E. JAMES  
*Atlantis, Florida*

MARIAN CARD DONNELLY. *A Short History of Observatories*. Eugene: University of Oregon Books. 1973. Pp. xv, 164. \$7.50.

In this brief essay of ninety-one pages, accompanied by seventy-three plates, Marian Card Donnelly seeks "to outline the development of

astronomical observatories in Europe and America as it has been shaped by the development of optical telescopes." She succeeds well in this modest aim, cataloging and illustrating the pathway from the humble observatory tower of the University of Leiden (1633) to the twin complexes now under construction for the National Science Foundation in Tucson, Arizona, and La Serena, Chile. The author shows how the observatory was for a long while a building to which the instruments (principally the quadrant and the sextant) were accommodated, but how in the later eighteenth century the Radcliffe Observatory at Oxford established new patterns of complex, differentiated design more nearly suited to the needs of the actual astronomers. Only in the nineteenth century did telescopes become the dominating instruments in observatories. Their rise to prominence was neatly symbolized by the steady swelling of the characteristic domes designed to house these machines and their attendants.

The text focuses throughout on illustrative examples, with plans, photographs, and sketches. It offers a careful, exact analysis, along with a useful bibliography and a comprehensive index. The tone is necessarily somewhat curt, as the investigation of so many different buildings inevitably leans heavily on secondary sources. Nonetheless this is a pioneering, innovative work.

A number of caveats are in order. Professor Donnelly focuses exclusively on institutional modes, thus missing such private creators of observatories as William and J. F. W. Herschel. More seriously, her study only scratches at the surface of the subject promised in her title and but rarely provides that subtle interplay of analysis and substance that is history. This is true even if her account is measured by classic "internal" internalist standards, concerned with how building styles changed over time. It is truer still, judged by her own avowed intention of providing the "external" internal history of how buildings and instruments mutually shaped each other's ends. The "internal" external history of patterns of operation and usage of observatories is nowhere touched on: no more so is the "external" external history that relates these scientific institutions to questions of patronage, financing, display, cultural competition, national purpose or social amusement. To

give but one example, the author reports without comment the recurrent abandonment of observatories. Since astronomical technique changed slowly other reasons must clearly be sought for both the origins and deaths of these institutions.

Marian Donnelly has illuminated one facet of the history of observatories. By so doing she has brought to light whole ranges of further questions for others to explore. And that is, after all, what a pioneering work should do.

ARNOLD THACKRAY  
University of Pennsylvania

HAROLD E. PAGLIARO, editor, *Racism in the Eighteenth Century*. (Studies in Eighteenth-Century Culture, volume 3.) Cleveland: Press of Case Western Reserve University. 1973. Pp. xviii, 468. \$12.95.

These are the papers presented at the third annual meeting of the American Society for Eighteenth Century Studies in March 1972, together with some from-the-floor discussion and an editorial introduction. In addition Robert Darnton's article, "The High Enlightenment and the Low-Life of Literature in Pre-Revolutionary France," first published in *Past and Present* (May 1971), is here reprinted because it won the annual prize of the Society for a scholarly article in the area of eighteenth-century studies. The title of the book derives from the symposium on racism, which accounts for only about one-third of the essays. The rest represent a variety of disciplines, but the history of literature and that of esthetics are weighted rather heavily. Darnton's essay, which is of a different genre, treating literature, or more specifically men of letters, from the angle of social history (a literary "underworld's" jealousy of the literary establishment), remains in my view the most intriguing and original of all the essays. The one paper concerning political theory should be mentioned: Beatrice C. Fink's "Benjamin Constant and the Enlightenment," stressing in Constant a strain of "teleological historicism" and attraction to the concept of perfectibility.

As for the subject of racism there was an attempt at the meeting to arrive at an explicit definition of the term. It was not altogether successful, but one infers from the papers and

the discussion that racism is generally held to involve both a factor of real or alleged biological difference and one of sociopolitical dominance. The objects of racism considered in these papers are mostly nonwhites, usually blacks. Hilda Neatby's essay, "Racism in the Old Province of Quebec," seems to be an exception in the use of the term, concerning itself as it does with what most of the authors would probably call ethnocentricity.

Richard H. Popkin's article, "The Philosophical Basis of Eighteenth Century Racism," is a useful overview of the racial theories of the century. The papers taken together demonstrate that there was a fairly broad spectrum of nuances in white racial attitudes, from the relatively neutral inquiry concerning the physiology of skin color (G. S. Rousseau, "The Cat and the Physiology of Negroes") and the less neutral science of Buffon (Philip R. Sloan, "The Idea of Racial Degeneracy in Buffon's *Histoire naturelle*") to the aggressive wish to maintain a caste system based on gradations of difference in skin color (David Lowenthal, "Free Colored West Indians: A Racial Dilemma"). But the point is made again and again that the standard for the eighteenth century is always white and European, even in the case of the Abbé Grégoire, champion of black as well as Jewish emancipation (Ruth F. Necheles, "Grégoire and the Egalitarian Movement"). This seems hardly surprising, of course, given not only the survival of Christian-medieval exclusiveness but the Enlightenment view of the history of civilization. In general the drift of the symposium papers is to emphasize what was parochial and self-serving in the thinking of the age of the Enlightenment, instead of what was universalistic, generous, or scientifically disinterested.

FRANCES ACOMB  
Duke University

DAVID F. TRASK. *Captains & Cabinets: Anglo-American Naval Relations, 1917-1918*. [Columbia:] University of Missouri Press. 1972. Pp. 396. \$12.00.

This study of Anglo-American naval relations in World War I is a fine companion volume to David Trask's earlier monograph, *The United States in the Supreme War Council* (1961), dealing with the shaping of Allied military

strategy during 1917-18. In the present work, Trask, who teaches at the State University of New York at Stony Brook, reminds us that naval power was also crucial to the Allies' victory and that its successful use involved every bit as intricate and sometimes acrimonious a balancing of conflicting views on wartime strategies and postwar political goals as did the military effort. Trask devotes only a chapter to the Mediterranean, where Franco-Italian rivalry frustrated any very effective Allied naval action, and deliberately omits any consideration of the unimportant and largely inactive Pacific theater. His major concern is the Anglo-American naval partnership and particularly the campaign in the vital North Atlantic. British and American leaders were deeply suspicious of each other's postwar naval and maritime intentions, but for the most part they worked together harmoniously during the war, Trask concludes. If they sometimes differed on such details as convoying and the North Sea mine barrage, and especially on the nature and extent of American naval shipbuilding during and after the war, they agreed on the basic wartime strategy of concentrating superior military force on the western front in France while maintaining an effective naval blockade of the Central Powers and neutralizing Germany's deadly submarine counterblockade. By mid-1918 there was no question about the success of the Anglo-American containment of the Imperial German Navy, whether it be its High Seas Fleet bottled up at Kiel and Heligoland, its scattered surface raiders, or its once-dangerous submarines.

In addition to the agreement on broad naval strategy another and perhaps more important reason for the extraordinarily close Anglo-American naval cooperation during the war was the commander of the U.S. naval forces in Europe, Admiral William S. Sims, who like his colleague in London, Ambassador Walter Hines Page, was an unabashed Anglophile. The able but supremely self-confident and egotistical Sims is probably the central figure of this study. As has been true of most field commanders throughout history Sims was soon convinced that Washington did not adequately understand his problems nor sufficiently appreciate his wisdom and achievements. Trask tends to take Sims's side as he recounts in great

detail the development of Sims's close working relationship with the British Admiralty and his touchy dealings with his superiors in Washington—President Wilson, Secretary of the Navy Josephus Daniels, and Chief of Naval Operations Admiral William S. Benson—who considered him hopelessly under British influence. Like Sims, who could not conceive of any serious Anglo-American differences, Trask is concerned primarily with the role of naval power in winning the war, not the unresolved naval rivalries that plagued the Paris Peace Conference and subsequently led to the Washington Naval Conference. Trask's book would have been more useful if he had broadened his focus to include at least the period of the peace settlement, but he has nevertheless added a new dimension to our understanding of World War I by his careful evaluation of the role of naval power in winning the war. Based on extensive use of a wide variety of American and British archival and private manuscript collections, some only recently available, and to a lesser extent on French and German sources as well, this is a valuable work of solid scholarship and insightful analysis.

E. DAVID CRONON  
*University of Wisconsin,  
 Madison*

HAMILTON FISH ARMSTRONG. *Peace and Counterpeace: From Wilson to Hitler. Memoirs.* New York: Harper and Row, 1971. Pp. xi, 585. \$12.95.

Since his undergraduate days at pre-World War I Princeton, Hamilton Fish Armstrong had been on familiar footing with many of the world's leading policy makers. His world was peopled with diplomats, scholars, military men, and ruling figures in a variety of regimes, ranging from representative governments to absolute monarchies and modern dictatorships. He was assistant editor of *Foreign Affairs* from 1922 to 1928, when he assumed full editorial responsibility. Yet despite that background the book under review provides little information about the historical forces at work between the wars. It is, rather, a series of vignettes concentrating on personality and description, a kind of in-depth

travelog, delightful to read but lacking in substance for historians or anyone seriously interested in understanding the period. The title is somewhat misleading, for this is not a study of "peace and counterpeace" so much as an informal glimpse at some of the people involved in foreign relations.

Indeed, Armstrong's view of foreign relations itself is a rather narrow one. On the plus side there is a lot of information on events and personalities involved in the Balkan countries, an area of special interest for the author, who served for a time after World War I as military attaché in Belgrade. Yet even the background material on the coming of World War I emphasizes the role of Serbian nationalists and the response of the various nations to the assassination of the Hapsburg archduke, with virtually no discussion of other critical factors. Mr. Armstrong alludes to certain selected developments, but he fails to analyze them. Some significant events are omitted. There is, for example, no mention of the secret treaties nor of the economic factors involved in the coming of the conflict.

There are few expressed values or personal opinions in this book, the one exception being the author's conviction that the United States should recognize and assume its responsibilities as a major world power. It was to encourage such a point of view that *Foreign Affairs* was founded and Mr. Armstrong, at least, finds proof of the validity of his assumptions in the attack on Pearl Harbor. He is mildly critical of some United States policies: the long wait to recognize the Soviet Union, and a repetition of that attitude in our dealings with the People's Republic of China, the refusal of the State Department to grant a visa to a Hungarian countess on grounds that she was a radical, overoptimism concerning the Kellogg-Briand Pact, and, of course, refusal to join the League of Nations and the World Court.

Throughout the book the reader is given the impression that enlightened people would almost intuitively accept the assumptions of those policy makers who promote the concept of "the American Century." When they are discussed at all, socialists and communists are the villains of the study. There is no attempt to understand or explain the attraction of social-



ism or even the struggle against colonialism, and no recognition at all of American imperialism. There is practically no mention of the revolutions in China and Mexico, and Gandhi receives only a passing comment.

A person with Hamilton Fish Armstrong's wide experience could surely have shared with us some of his deeper insights. Readers would appreciate having received his judgment regarding the underlying causes of the twentieth-century wars, what specifically United States policy should be in various parts of the world, and how future conflicts may be avoided, if, indeed, the author thought that could be accomplished.

LARRY GARA

Wilmington College

FRIEDRICH RUGE. *Bündnisse in Vergangenheit und Gegenwart unter besonderer Berücksichtigung von UNO, NATO, EWG und Warschauer Pakt*. Frankfurt am Main: Bernard & Graefe Verlag für Wehrwesen. 1971. Pp. 173. DM 16.80.

Friedrich Ruge believed that contemporary problems can be understood by analyzing the case examples of the past. In this work, a survey of typical structures and forms of alliances throughout the ages, he aimed to reveal the issues at stake in the development of stability in international and, in particular, European relations since 1945. Admiral in the West German navy with staff experience at NATO before his appointment as professor of military history at Tübingen University, Ruge accomplished his task in an attractive combination of scholarly detachment and an insider's appreciation for practical detail. Nevertheless his explanations and judgments often raised doubts about the soundness of his approach.

Ruge contended that alliances were essentially instruments of state policy to be understood only by reference to their political context. He based his alliance typology on a determination of the rights and obligations, especially over the military power, of alliance members and dovetailed his historical sections by frequent reference to the solutions alliances developed to promote respect among member states. In discussing alliances since 1945 Ruge characterized the NATO-Common Market system as egalitarian and the Warsaw Pact as in-

egalitarian. In the latter, instability accompanied unity. The Russians, as the hegemonial power, pushed for greater integration, but the alliance was under the menace of dissolution if Russian prestige ever faltered as subordinate powers sought independence. In NATO, on the other hand, democratic decision making generated the development of political unity in Western Europe. Ruge cited as evidence its decisions on a unified command, inclusion of the West German army in its ranks, strategic planning of European nuclear defense, prior consultation on diplomatic moves, new institutions for economic cooperation and policy on high-level personnel contacts.

The merit of Ruge's work was in drawing attention to the role and modifying effect of political considerations on the processes of history. But it had several demerits that were best exemplified by his optimistic estimate of NATO's ability to generate unity whereby he has left the reader completely unprepared to understand how current differences on oil, the cold war, and dollar devaluation could bring the alliance into disarray. In short, his conclusions often failed to convince because he neglected social, cultural, and economic conditions that gave rise to political problems. This emphasis on political factors raises doubts about the ability to employ power-based typologies to produce any new understanding of international problems.

JOHN F. FLYNN

University of the South

#### ANCIENT

E. S. HIGGS, editor. *Papers in Economic Prehistory: Studies by Members and Associates of the British Academy Major Research Project in the Early History of Agriculture*. New York: Cambridge University Press. 1972. Pp. x, 219. \$15.50.

The papers presented in this volume are principally concerned with the study of the domestication of plants and animals by early man. The subject is one much discussed by modern prehistorians and archeologists and often beset with jargon and preconceived notions. A group of British scientists and archeologists have now made a coordinated research effort to examine theory, methods, and techniques in an objec-

tive manner and to run some experiments in the field; the project is sponsored by the British Academy. There are sixteen papers in the present report, the theoretical ones by E. S. Higgs and M. R. Jarman ("The Origins of Animal and Plant Husbandry"), H. N. Jarman ("The Origins of Wheat and Barley Cultivation"), E. S. Higgs and C. Vita-Finzi ("Prehistoric Economies: A Territorial Approach"); methods and techniques are discussed by H. N. Jarman and others ("Retrieval of Plant Remains from Archaeological Sites by Froth Flotation") and by S. Payne ("Partial Recovery and Sample Bias: The Results of Some Sieving Experiments"); among the field and case studies are discussions by A. J. Legge ("Prehistoric Exploitation of the Gazelle in Palestine"), M. R. Jarman ("European Deer Economies"), R. W. Dennell ("Interpretation of Plant Remains from Three Bulgarian Early Bronze Age Sites"), D. Webley ("Soils and Site Location in Prehistoric Palestine"), D. H. French and others ("Excavations at Can Hasan III, a Neolithic Mound in Turkey"). The foreword is by Grahame Clark, the conclusion by Sir Joseph Hutchinson, who adds sobering words on domestication in the Indo-Pakistan subcontinent.

The volume offers much of interest to the prehistorian and archeologist, and most of it is general enough to be comprehensible to the nonspecialist. It will provide a good basis for discussion and for controversy in graduate seminars, but it stands out by its objective presentation of the subject matter and by the frank admission in many of the articles that we still stand at the very beginning of an understanding of economic prehistory. Our ignorance has been due to two factors. The first is a lack of method and clarity in the approach to the scientific analysis of domestication. The present volume is a good effort to provide constructive theoretical guidance. H. N. Jarman, for example, is helpful in clarifying terminology and sorting out concepts in her paper on earliest wheat and barley cultivation; M. R. Jarman and P. F. Wilkinson take a hard look at the criteria used in the study of animal domestication.

The second negative factor has been the lack of excavation in crucial areas and the lack of retrieval in many excavations. This shortcoming is being remedied at many sites, and ex-

amples of action by the authors of the volume are discussed in several papers. The archeologist can appreciate the reasonable demands for sample retrieval to provide sufficient and objectively collected botanical and faunal remains from his site; the articles by S. Payne explain how modern methods will function in modern context. Practical devices are discussed and illustrated, with acknowledgment of the relativity of usefulness dependent upon the characteristics of individual sites.

Some of the papers are concerned with the general natural resources that determined the movement and settlement of early man, but even with the prevailing emphasis on animal and plant husbandry the volume can fairly claim to be a contribution to the economic and ecological analysis of a crucial stage in mankind's prehistory.

MACHTELD J. MELLINK  
Bryn Mawr College

ROBERT MCC. ADAMS and HANS J. NISSEN. *The Uruk Countryside: The Natural Setting of Urban Societies*. Chicago: University of Chicago Press, 1972. Pp. x, 241. \$17.50.

The first part of the book is concerned with configurations of settlement by Adams, and the second is an analysis of archeological surface collections, mostly pottery, by Nissen. The same method of survey of irrigation and settlements in *Land Behind Baghdad* is here applied to the Sumerian south, but with more elaboration and refinement. Many maps, charts, and sketches of pottery types embellish the book, which is an ecological study throughout history. Wind erosion and irrigation patterns are indeed important factors for determining population changes in Iraq's past, and they were vitally important for this study, which opens new paths in the analysis of archeology. The study of the distribution of potsherds on the surface, plus the plotting of mounds and ridges to determine settlement, water courses, and the like, may seem primitive and uncertain, but it has revealed patterns of early settlement in Mesopotamia. The information that can be obtained from a statistical study of sizes of sites and potsherd distribution at different periods of history is impressive. There are many reservations one might have, but no one has

proposed another method to interpret large surface finds.

Rather than discuss the many charts and maps showing both geographic and anthropological data, as well as archeological, we may ask what the ensuing population density estimates show us. For one thing, historical documents find archeological support in the restriction of settlements in times of troubles, or the reverse. Another conclusion is that the hand of man, in building canals and dikes and the like, was all important in the prosperity of southern Mesopotamia, and any expansion of settlements required great labor. With the coming of Islam the area of swamps in the south increased greatly over the Sasanian period, and the land around Uruk was almost abandoned.

Although the book is in places not easy to read I believe it is an admirable approach to the land and people, from earliest times to the present. Several conclusions arrived at in studying the north are reinforced in the study of the Uruk countryside; one being, for example, that the Sasanian period saw an intensification and extension of irrigation to its furthest limits.

The book is well printed and it will undoubtedly serve as one model for later cross-cultural or interdisciplinary studies of certain areas throughout time.

RICHARD N. FRYE  
*Harvard University*

ALISON BURFORD. *Craftsmen in Greek and Roman Society*. (Aspects of Greek and Roman Life.) Ithaca: Cornell University Press. 1972. Pp. 256. \$11.75.

Alison Burford has previously demonstrated her expertise in the field of ancient Greek labor and business with *The Greek Temple Builders at Epidauros* (1969). Her present book is obviously a broad survey of skilled labor (properly, according to ancient notions, including plastic arts, as well as artisans' work in durable materials) in the classical world; in accordance with the purpose of its series the book is nontechnical and addressed to a wider audience of students and the general educated public. The long chapter on the workings of Greek and Roman society is primarily for the

latter. Ancient technology is discussed only to illustrate a point; yet inevitably a good deal of information about techniques slips in, but the book is none the worse for that. Specific crafts are not discussed as such, but rather the author describes such things as working conditions, pay (almost impossible to be meaningful about; there are too many unknown variables), the craftsmens' relations with their patrons or employers, and the nonworking activities of craftsmen.

Some points are worth noting briefly here: The proportion of highly skilled persons in ancient society was never large; such persons, contrary to common assumption, were not usually concentrated in a quarter of a given city according to their trade, but scattered about. The increase of specialization of labor during Greek and Roman times was probably much less than usually thought. The lack of progress in ancient technology is dutifully noted; more important is the convincing rejection of any attempt to attribute the fact to the existence of slavery. Moreover, the ancient world knew no such thing as revolutionary workers' groups. Craftsmen were thought to owe their achievements to their art (*techné*), and not to the inspiration of a god. One wishes that Ms. Burford had seen fit to stress the fundamental importance of the failure of ancient technology to progress as a major reason for the ultimate decline of Greco-Roman culture. And a summary chapter at the end would have been useful to clear the air after all the details. Yet this is a good and useful book, well worth reading even by people more specialized than its presumptive audience.

STEWART IRVIN OOST  
*University of Chicago*

DONNA C. KURTZ and JOHN BOARDMAN. *Greek Burial Customs*. (Aspects of Greek and Roman Life.) Ithaca: Cornell University Press. 1971. Pp. 384. \$9.75.

J. M. C. TOYNBEE. *Death and Burial in the Roman World*. (Aspects of Greek and Roman Life.) Ithaca: Cornell University Press. 1971. Pp. 336. \$8.50.

It has often been said that death is as natural as life; and archeologists, it should be noted, not only uncover and illuminate for us the

ways of ancient life but also the ancient ways of death. The two books briefly reviewed here in the same Cornell University series provide the student of ancient history with comprehensive surveys of what we know about the burial practices of the ancient Greeks and Romans. Though neither book claims to deal in any substantial way with the religious aspects of death as such, inevitably some such discussion is included. However, both books are absolutely merciless in their reporting of the factual information that has been archeologically gathered and is generously illustrated verbally and pictorially. Neither book is one for the casual reader but rather for the serious student who is interested in details. Both books tell us more about what the Greeks and Romans *did* rather than what they *thought* about death. Why the Greeks and Romans did what they did must depend upon the ancient literary evidence and the imaginative interpretations of contemporary scholars.

Both *Greek Burial Customs* and *Death and Burial in the Roman World* are generally arranged chronologically. *Greek Burial Customs* largely treats burial customs in Athens and Attica because Attica cemeteries are the best known and the best published in all periods and the literary evidence is predominantly concerned with Athens and all pertinent vase depictions are Athenian. The book is divided in two parts: "Athens and Attica" and "The Greek World." Dr. Kurtz was chiefly responsible for the first part and Mr. Boardman for the second. The cemeteries and their contents from the end of the Bronze Age, the Early Iron Age, and the geometric, archaic, the classical, and Hellenistic periods are discussed and the funeral rites that went with them. In the Greek world, topographically confined to the Aegean and selectively to the Greek kingdoms of the Hellenistic world, common graves, rites and offerings, communal graves and cenotaphs, epitaphs, decorated sarcophagi, monumental tombs and heroa are touched upon with authority if sometimes with brevity and always with readability.

Professor Toynbee's book provides a contrast in scope and style. It is much more strictly chronological and wide-ranging, constituting essentially the first comprehensive survey in English of burial customs and attitudes toward

death in the ancient world for all peoples living under Roman sovereignty or influence in both Republican and Imperial times—pagan, Jewish, and Christian in a period that was intensely interested in life after death and one that expended lavishly on the veneration of the departed—a significant contrast to Greek views and practices. Because her book ranges over so much more both geographically and archeologically the reader cannot help but be impressed by the absolute command Professor Toynbee has over her subject as she discusses the Etruscan antecedents; Roman beliefs about the afterlife, cremation and inhumation; funerary rites and cults of the dead; the layout of cemeteries and ownership of tombs, walled cemeteries, and funerary gardens; selected types of tombs; and gravestones and tomb furniture—all profusely illustrated and described in detail.

Neither *Greek Burial Customs* nor *Death and Burial in the Roman World* concern themselves with art history or the history of religions or the history of literature or the literature of history, but each of these areas will be illuminated by these books that provide an enormous wealth of data that is now readily available for exploitation, development, and interpretation.

JOHN E. REXINE  
Colgate University

TARIZL CHUBINISHVILI. *K drevnei istorii Iuzhnogo Kavkaza* [To the Early History of the Southern Caucasus]. Volume 1, *Drevniaia kul'tura Iuzhnoi Gruzii (V–III tys. do n. e.) i problema stanovleniia "Kuro-Araksnoi" kul'tury na Iuzhnom Kavkaze* [The Early Culture of Southern Georgia (5th–3rd Centuries B.C.) and the Problem of the Establishment of "Kuro-Arakskaia" Culture in the Southern Caucasus]. (Akademiia Nauk Gruzinskoi SSR, Institut: Istorii, Arkheologii i Etnografii imeni N. A. Dzhavakhishvili.) Tbilisi: Izdatel'stvo "Metsniereba." 1971. Pp. 168, 17 plates.

The Kuro-Araxes culture of Transcaucasia was first identified by B. A. Kuftin in the late thirties of the present century and rapidly attracted the interest of archeologists through the similarity of its characteristic black polished pottery with geometric grooved decorations to the Khirbet-Kerak ware found in Palestine. Despite the III millennium B.C. date assigned

to it, early excavators associated this Transcaucasian culture with a Chalcolithic level and consequently placed it at a lower stage of development than the surrounding Near Eastern civilizations that had already entered the Bronze Age.

The present book by a leading Soviet archaeologist is the first volume of a general study subsuming the work of the past fifteen years. It parallels a second study by the same author (K. Kh. Kushnareva and T. N. Chubinishvili, *Drevnie Kul'tury Iuzhnogo Kavkaza* [*The Ancient Cultures of the Southern Caucasus*], 1970) with which the present publication should probably be read, if only because of the earlier work's clearer drawings and map, as well as its useful survey of previous literature.

Basing himself on the extensive material of recent excavations and particularly on the identification of truly Chalcolithic (V-IV mil.) sites in Transcaucasia during the past few years, T. Chubinishvili presents a series of working conclusions as to the origin and character of the Kuro-Araxes culture. In agreement with most of his colleagues he transfers this culture from the Chalcolithic stage to the early Bronze Age and traces its appearance to the very beginning of the III millennium. In so doing he achieves a cultural synchronism between the southern Caucasus and the adjacent regions of the northern Caucasus, northwestern Iran and eastern Anatolia; he observes the similar elements within these areas and even suggests the possibility of common origin (pp. 11, 163). At the same time, he also notes the simultaneous appearance of Kuro-Araxeian sites in both high and lowland zones, thus rejecting the hypothesis of a gradual move into the mountain region from the southeast. On the basis of the continuity of sites, the preservation of the traditional architectural plan of the round house on a stone foundation (e.g. at Shengavit), and the survival of previous shapes in early Kuro-Araxes ware, he argues repeatedly for continuity from the local Chalcolithic to the Bronze Age society. Here again, however, he cites the observation of anthropological links between the southern Caucasus and the finds at Tepe Hissar, Sialk, El-Ubaid and Kish, and he does not reject altogether the possibility of a migration, or rather an infiltration, of new peoples at the beginning of the new era.

Finally, despite his admission that the economic and especially the social changes heralding the Kuro-Araxes culture are still unclear (p. 158), he sees it as a developing patriarchal society marked by increasingly closer contacts between mountain and valley tribes and based on a mixed agricultural and pastoral economy accompanied by a rapidly expanding metallurgy.

T. Chubinishvili is evidently thoroughly familiar with the earlier literature on the Kuro-Araxes period and with the archaeological material, much of which he knows at first hand, but some of his conclusions must necessarily remain tentative. As he himself stresses, the possibility of synthesis has not yet been reached. Any definitive position on the causes that led to the development of the Kuro-Araxes culture at the beginning of the III millennium, and on its links with either earlier local stages of development or with contemporary adjacent civilizations, must await a study of all the already extant material and especially a great deal of additional work on the Chalcolithic and even Neolithic sites now appearing in Transcaucasia. Nevertheless, the author has collected together a vast amount of scattered and valuable, if at times confusing, data on a still insufficiently familiar area of the ancient Near East.

NINA G. GARSOËAN  
Columbia University

## MEDIEVAL

ROBERT S. LOPEZ. *The Commercial Revolution of the Middle Ages, 950-1350*. (The Economic Civilization of Europe.) Englewood Cliffs, N.J.: Prentice-Hall. 1971. Pp. xi, 177. \$5.95.

DOUGLASS C. NORTH and ROBERT PAUL THOMAS. *The Rise of the Western World: A New Economic History*. New York: Cambridge University Press. 1973. Pp. viii, 170. \$7.95.

These two volumes are quite different in approach and content, but both of them are useful and interesting. Professor Lopez has put in capsule form—a bare 167 pages of text—his view of the economic history of the Middle Ages from the second to the fifteenth centuries. Since the volume is largely descriptive and does not treat the various problems in depth, it does not contain an analysis of those

issues on which other economic historians have differed with Lopez. In addition the points of emphasis are those that accord with his interpretation of what happened. Thus, it is interesting to note that Lopez sees the elements of the decline of the economy of the classical world in the late second century A.D. and places much more emphasis on the activities of the Moslims in bringing about a concentration of population in towns causing a more active economy than upon the raids and later settlement of the Northmen. Similarly, Lopez continues to hold that the tenth century was the beginning of the period of uninterrupted growth and that the development of the capital resources for the growth of the economy took place on the basis of the use of credit instruments that are difficult at best to document in any numbers before the twelfth century. The problem of the source of the so-called "primal capitalist hoard," which made possible the expansion of commerce during the twelfth century, is a most vexing one, and it is certainly inviting to believe that there was a long period of development in the use of credit instruments rather than a relatively swift adoption of them by people who grasped their utility as soon as they became properly known in the West. Credit was clearly the vital instrument that made possible the vast expansion of the commerce of the twelfth century.

What is perhaps most interesting in Lopez's treatment of the economic history of the Middle Ages, however, is the importance that he places upon secular climatic change during the period in question. Ellsworth Huntington was, of course, the great protagonist of such cyclical climatic changes as a force in history, and Arnold Toynbee a leading exponent of their importance in shaping events. This small volume is perhaps the best concise introduction to the thinking of a leading economic historian of the Middle Ages, and Professor Lopez is to be congratulated for the clarity with which he explains his views.

Professors North and Thomas have treated the economic history of the period 950-1700 from a somewhat different point of view. Arguing largely from the standpoint of economic theory they have attempted to establish clear connections between changes in property rights and the development of national states with

differing rates of economic growth in Western Europe. They have tried, insofar as that was possible, to utilize quantitative data presented in the form of graphs indicating prices, wages, real wages, rents, etc. As can readily be seen the reliability of such data increases markedly as we move into later periods, and none of those graphs published in this volume deal with the period before the thirteenth century.

After a rather brief introduction to the feudal period in which the authors contend that in the supposed "non-market" economy of the early Middle Ages it was more economical to utilize feudalism and manorialism as the primary institutions for organizing society, they contend that population expansion, commercial revival, agricultural expansion, and the creation of a market system with a money economy resulted in institutional changes in feudalism. The connection between fiscal policy and property rights was the paramount feature that influenced the development of European states, but the results were different in various states. In England and the Netherlands the established arrangements led to great economic growth, whereas in France the development was much slower, and in Spain it led to eventual stagnation and decline.

This volume could easily lead to a series of debates among historians.

HOWARD L. ADELSON

City University of New York

PAUL EGON HÜBINGER. *Die letzten Worte Papst Gregors VII.* (Rheinisch-Westfälische Akademie der Wissenschaften. Geisteswissenschaften, Vorträge G 185.) Opladen: Westdeutscher Verlag. 1973. Pp. 112. DM 16.80.

Probably the most famous of medieval "famous last words" are Gregory VII's "I have loved justice and hated iniquity, therefore I die in exile." A check of textbooks accumulated on my shelf as a result of publishers' random munificence shows that Gregory's alleged last words must count along with "When Adam delved and Eve span" as being among the most beloved hackneyed quotations of modern medievalists. Hübinger, long-time *Ordinarius* at Bonn, has carried off a *coup de maître* in transforming this seeming dross into material for a dazzling little study of Gregorian thought and modern historiography. His major conclusion,

which I feel slightly sheepish about divulging since he himself builds up the suspense as he goes along, is that Gregory's last words were not meant to be understood as an expression of earthly defeat but rather as a proof of his death as pope of the true Church and as a martyr who knew that "Blessed are they who endure persecution for the sake of justice, for theirs is the kingdom of heaven." Hübinger demonstrates this convincingly by analyzing the contents and contexts of the earliest sources. He shows that the pertinent quotation, attested for within a year of Gregory's death, was probably composed by a Gregorian partisan for service in the propaganda wars rather than having been uttered by the pope himself. None of the medieval writers who cited it thought that the substitution of "death in exile" for the reward of "anointment with the oil of gladness" originally promised to lovers of justice by the Psalms (44, 8) was an expression of bitterness; indeed, at least one thought that the saying was appropriate to put into the mouth of a dying candidate for canonization. Not so scores of prominent modern historians whom Hübinger shows to have misunderstood the text. Although he credits Giesebrecht with introducing the "bitterness" cliché, I can trump him by pointing to it in Milman, whose work antedates Giesebrecht's by a decade. No doubt others could make similar minor emendations, but few will be able to write such models of *Geistesgeschichte*. I only cannot decide whether the footnotes, which prefer remarks on Thomas Mann to H. K. Mann, are not even more fascinating than the text.

ROBERT E. LERNER  
Northwestern University

IAN KERSHAW. *Bolton Priory: The Economy of a Northern Monastery, 1286-1325*. (Oxford Historical Monographs.) New York: Oxford University Press. 1973. Pp. xii, 213. \$14.50.

Basing his study primarily on an account book covering the years 1286 to 1325, Kershaw presents a detailed and careful analysis of the economy of Bolton Priory, a small Augustinian house in Yorkshire. Accompanying the text are twenty-five tables, covering such matters as corn production, costs of seasonal and full-time labor, sale and purchase of corn and wool,

numbers of livestock, grain expenditure for bread and ale, and meat consumption. Not surprisingly, in this damp northern area, pasture farming was more profitable than arable cultivation, and the sale of wool was of vital importance, accounting for nearly half of Bolton's revenues between 1287 and 1305. Following a period of expansion, disaster struck, first in 1315 with two years of bad harvests, owing to excessive rainfall, and again with the Scottish raids of 1318-19 and the great cattle murrain of 1319-20, necessitating the temporary dispersal of the canons and the placing of the priory's economy under the control of lay guardians. With the departure of eight of the thirteen canons and the consequent reduction of the household, consumption was cut drastically, and the lay guardians were able slowly to restore the priory's economy.

Of particular interest is the way in which a study such as this supports or calls into question generalizations about economic conditions and practices of the period. The poor harvests are followed by reduced harvests, in spite of favorable weather, because insufficient seed-corn could be set aside, supporting W. G. Hoskins's conclusions on harvest fluctuations. On the other hand, contrary to R. H. Hilton's point of view, the household was reduced and servants dismissed during famine years, though the canons themselves ate and drank about as much as usual. Again, as Hilton and Postan have concluded about thirteenth-century landlords, the canons of Bolton were taking much from their estates and putting back very little in productive investment. As in many another monastic house the canons of Bolton lived well, lavishing hospitality and gifts on men of wealth and power and providing very little for the relief of the poor and destitute. Astonishingly, in addition to the heroic amounts of bread and ale consumed, the per capita meat consumption equaled that of advanced countries of modern times. But there was little thought for the morrow, and the fat years saw no preparation for the lean ones to follow.

The usefulness of this excellent monograph is enhanced by a full bibliography and a good index.

ISABEL R. ABBOTT  
Providence, Rhode Island

K. B. MCFARLANE. *Lancastrian Kings and Lollard Knights*. New York: Oxford University Press. 1972. Pp. 261. \$9.50.

Part 1 of this volume consists of lectures given to undergraduates in the Honours School of Modern History at Oxford, written between 1936 and 1947 and last delivered in 1953. Part 2 consists of those given between 1956 and 1966. Together they constitute a fine tribute to the memory of a distinguished and influential scholar, the standard of whose lectures was extraordinarily high.

Concentrating on Henry IV and Henry V, and laying stress on personalities, McFarlane is strongly anti-Stubbs and anti-Wylie in his approach. He eschews constitutional issues and conflicts of ideas and places great emphasis on personal animosities and ambitions. Written with scholarship, lucidity and authority, his lectures must have made a great impact on undergraduates, but perhaps the impact will be somewhat less overwhelming on the wider audience by which they are now received. They react a little too strongly against the Stubbsian tradition and, indeed, against a whole complex of Victorian notions of causation in history that, whatever their limitations, helped to create a very distinguished period in English historiography. They represent a brilliant tour de force, not a new and compelling approach to the problems of the past.

The essays have been edited with sympathy and meticulous care. References have been added by both editors. Those by Dr. Harris are specifically intended to help undergraduates, in particular, to be aware of the points where subsequent research has elaborated on, or modified, McFarlane's conclusions. Unfortunately, he does not make the student aware of some important differences of opinion that are a feature of the current approach to this period and that McFarlane himself tended to ignore. Should Dr. Harris, for example, have left without comment the statement that Parliament was only a particularly ceremonious way of taking counsel? Should he have left without a footnote McFarlane's view of the "curious comedy" involving the king's councillors in 1390? Has the student received adequate guidance about differing views of the "parliamentary" committee of 1398? Or the statement that when McFarlane wrote his lectures the wording

of the famous "Record and Process" of 1399 had never been critically examined? Could there not have been some helpful reference to conflicting views on the subject of fifteenth-century lords of the council, particularly as these might apply to McFarlane's treatment of the aims of the Commons in 1410? No one could reasonably expect Dr. Harris to make reference to the many larger problems, but he might profitably have extended himself a little further. The undergraduates whom he had particularly in mind may well be somewhat overwhelmed by McFarlane's authority and virtuosity, even though more experienced scholars may safely be left to take care of themselves.

The problem of the Lollard knights, which is dealt with in part 2, is much more tailored to McFarlane's genius. McFarlane's approach through the individual, even though somewhat tedious at times, produces in the end important and definitive results. He supersedes Waugh (though with a generous tribute to his predecessor) in his identification of the members of the group and his analysis of their outlook and their influence at the royal court. It is very doubtful if anything of great import has escaped his penetrating examination of the available evidence. Few scholars would have dared to make such researches the basis of a series of lectures, and fewer still would have been able to carry their audience without recourse to popularization. Both parts of the volume reveal how much Bruce McFarlane gave to his listeners and how much he expected from them. No wonder his imprint on them was deep and lasting. Like T. F. Tout at Manchester, he may perhaps be said to have founded a school of history in his pattern, a pattern more clearly revealed in his lectures than in anything else that he wrote.

B. WILKINSON  
Toronto University

JOSEPH SHATZMILLER. *Recherches sur la communauté juive de Manosque au Moyen Age, 1241-1329*. (École Pratique des Hautes Études—Sorbonne. VI<sup>e</sup> Section: Sciences Économiques et Sociales. Études juives, 15.) Paris: Mouton & Co. 1973. Pp. 183. 44 fr.

This small volume presents the results of the meticulous analysis of the court and notarial



registers of Manosque, in Provence, from 1241 to 1329. These records refer to almost 3,000 criminal and civil cases involving Jews. The results of the analysis, compared to Emery's book on the Jews of Perpignan, are disappointing. The author has reserved all economic matters for a future study and the sources are apparently skimpy for most other subjects.

Shatzmiller addresses himself to two matters: the internal life of the Jewish community (estimated at 100 to 200 individuals) and the place of Jews in the Christian society of Manosque. Repeatedly acknowledging the limitations of his sources he concludes that the Jewish community had some self-government, since they occasionally levied taxes on themselves and since the rabbi's power to excommunicate enabled him to maintain some discipline. Divisions within the community are apparent, however, since some Jews successfully challenged these very powers in the Christian court. Jewish informers for the Inquisition represent the most acute and tragic form of these divisions, accusing their Jewish enemies of sheltering Jews who were relapsed Christians.

Manosque belonged to the Hospitallers, who provided its court. The most valuable part of this book is an examination of how the court treated Jews. Shatzmiller analyzes about 400 cases in various statistical tables, and the results are consistently the same: the court treated Jews no worse than Christians and perhaps even better. The percentage of convictions and acquittals and the size of the fines levied are the same for Jews and for Christians. Jews took Christians to court as often as Christians took Jews to court. When the court looked into matters on its own initiative, it was as likely to find in favor of the Jew as the Christian, and it supported the claims of Jewish creditors against Christian debtors.

Shatzmiller finds limited but persuasive evidence that the popular attitudes of Manosque Christians were similar to those found elsewhere. Jews were occasionally suspected of maligning Christianity, of poisoning ovens or wells, and of murder. What remains surprising is that these prejudices apparently did not sway the court.

Shatzmiller's scrupulous scholarship makes for clarity and reliability. This book establishes

his qualifications to write a more complete history of the Jews of Manosque in this period.

JOHN C. MOORE

*Hofstra University*

V. P. GRACHEV. *Serbskaia gosudarstvennost' v X-XIV vv. (Kritika teorii "Zhupnoi organizatsii" [Serbian State Organization in the 10th-14th Centuries (A Critique of the Theory of "Zhupa Organization")]*. (Akademiia Nauk SSSR, Institut Slavianovedeniia i Balkanistiki.) Moscow: Izdatel'stvo "Nauka." 1972. Pp. 331.

A prominent feature of Slavic life is a seemingly overwhelming sense of inferiority. Among the East Slavs there is the saying that "the Russians never invented anything, not even the samovar." Stalin tried to reverse this by "proving" that the Russians had invented everything. In the historiographic realm Soviet scholars have gone to great lengths in the Normanist controversy over who created the first East Slavic state, the indigenes or the Vikings, to prove that it developed organically, without significant foreign influence.

West and South Slavs have similar problems about the origins of their states. Historians have tended to conclude that they were the results of Germanic, Turkic, and Byzantine intervention, rather than indigenous creations. Now Grachev has proposed developing the methodology worked out for Kievan Rus' to prove that other Slavic states also arose organically.

Grachev places his study in its scholarly context with a most welcome, lengthy historiographical essay reviewing theories about the organization of the Serbian state. The traditional view is that the Slavs, moving into the Balkans from the sixth to the seventh centuries, settled in distinct territorial-geographical regions (*zhupa*) with the *zhupan* as their tribal leader, but that, while the regions remained constant, there was a sharp discontinuity caused by external forces between the like-named officials of the Serbian state of the Nemanichi (1190s-1370s), a peripetetic monarchy with no urban base.

In the core of the work, a fine example of historical geography, Grachev analyzes every extant mention of both terms (the book, however, has not one map) and concludes that the evolution was indeed organic, caused by un-

specified economic changes in production that forced alterations in the forms of land tenure, stimulating the nonsynchronous evolution of the *zhupa* from a territorial-geographical region to a structurally unconnected administrative unit (1220s) and the *zhupan* from a tribal chief to a state administrator (ninth century) to a magnate seigneur with autonomous political authority (late thirteenth through the fourteenth centuries). Some of Grachev's interesting conclusions are solidly based, but many are tentative, speculative, and founded only on other speculations.

RICHARD HELLIE  
University of Chicago

PAUL LEMERLE. *Le premier humanisme byzantin: Notes et remarques sur enseignement et culture à Byzance des origines au X<sup>e</sup> siècle*. (Bibliothèque byzantine, Études, 6.) Paris: Presses Universitaires de France. 1971. Pp. 326. 68 fr.

Professor Lemerle is France's most eminent Byzantine historian and his book is the most complete and the best treatment to date of the first Byzantine humanism, its antecedents, and its institutional and historical setting. It also ranks with the best works ever written on any aspect of Byzantine cultural history. To be sure, the book is technical, but it is too important and too lucidly and artfully written for knowledge of it to be restricted to Byzantinists alone.

The term "first Byzantine humanism" denotes the flourishing of culture in Byzantium—practically speaking, in its capital—beginning sometime in the ninth century, culminating in the activity of Photius (810?–891?)—scholar, statesman, and patriarch—and ending with the vast encyclopedic enterprises presided over by Constantine Porphyrogenitus (emperor, 908–59). In this definition "flourishing" means two things: intensified literary production in high style and increased familiarity with the works of the past, not only those of the classics, but also those of late antique and early Byzantine authors; while "culture" means elite culture, predominantly secular, literary, and rhetorical. Scientific interests were present in our period, but they diminished as time went on. The period of the first Byzantine humanism coincided with events and controversies crucial to Byzan-

tium's history: the continued struggle with Islam (a struggle that began to turn to Byzantium's advantage), the Second Iconoclasm and the victory of Orthodoxy, the success of Byzantine religious missions, and Byzantium's first big clash with the papacy. The principal personalities of that cultural current were involved in all those events and controversies.

The book's subtitle, *Notes and Remarks on the History of Education and Culture in Byzantium*, is a *topos* of modesty. True, Lemerle does not discuss everything—he is too accomplished a historian to do so. But he examines everything that is important: every personage, text, and cultural innovation or institution relevant to his subject. In his examination he proceeds down to dating individual works, emending passages, discussing single words, and tracing errors in modern scholarship back to their seventeenth-century origins (cf. pp. 65, 67).

Thus, to single out some highlights, Lemerle offers the most thorough treatment to date of the iconoclast intellectuals John the Grammarian (pp. 135–46) and Leo the Mathematician (pp. 148–76), exquisite portraits of Photius (a realist, turned toward action; probably proud and authoritarian; a conformist and puritan, pp. 177–204) and Arethas of Caesarea (a good philologist and manuscript collector, but a narrow mind, pp. 205–41). He gives the first thorough analysis of the recently published correspondence of an anonymous tenth-century principal and owner of a secondary school in Constantinople (the crotchety old man quarreled with other masters and was the only full-fledged professor in his school, but he used older students as teaching assistants in grammar and rhetoric; the pupils of his establishment were sons of state officials or nephews of prelates, pp. 246–57). He considers the relation of the revolution in Greek script (the introduction of the minuscule, the earliest manuscript dating from 835) to the intellectual ferment of the early ninth century (the new script was a response to that ferment, but there is no certainty that the minuscule—created, according to Lemerle, in the capital—was the product of the monastic milieu, particularly of the Studios monastery, pp. 109, 118, 128), and discusses the controversial terms connected with the new manner of writing (*syrmaiographein*, being one of them,

does mean "to write in minuscule," pp. 116-17; "bombycine" does not mean "of cotton," for all paper was made of rags, but "of the city of Membedj," from where Arab paper was exported, p. 111). I regret that the author does not reflect—as Professor Mango recently did—on the fact that the Byzantine and the Carolingian minuscules appear at about the same time, the Western script being slightly earlier.

Lemerle excerpts over thirty lives of saints of the ninth and tenth centuries (cf. pp. 97-104), mostly for information on the elementary education dispensed to their heroes and on the organization of secondary education by the state (he dates such state intervention to the tenth century, pp. 259-60, 266); he analyzes the evidence that the chroniclers and the *Book of the Prefect* present on institutions of higher learning (such as the Bardas school at the Magnaura palace, organized probably shortly before 855, pp. 160, 242) and on legal education (pp. 261-63).

The strength of Lemerle's book lies in its method. The latter consists of a return to the sources, the use of common sense (the French scholarly approximation of which is *la bonne méthode*), and an awareness of what the sources do not tell us, and, consequently, of what we do not know. This leads to the deflation, or demise, of some constructs that have been passed on from one modern book to another, such as the postulate that Byzantium had an Imperial University (cf. pp. 146, 263-65, 303) or a Patriarchal Academy (pp. 87, 95-96: there is no reliable trace of the latter in the ninth century—a result that Professor Hans-Georg Beck of Munich obtained independently some years ago), or, finally, the story of the alleged burning of the Library of Constantinople by Leo III (pp. 89-93; this, however, is no longer believed by any serious scholar). Close reading of the sources also leads Lemerle to the revision of the exaggerated views on the role of the Arab "backlash" in the revival of learning in the ninth century and to the rejection of some preposterous speculations: according to one such speculation (cf. pp. 37, 42), Photius found in Bagdad, rather than in Constantinople, the 279 works summed up in his scrapbook of readings, or *Bibliotheca*. But what Lemerle retains from the sources and presents, often with caveats, to the reader merits the latter's full confidence.

All this goes to show that a conventional method is effective when it is applied by a precise and elegant mind. Of course Lemerle is aware that social needs bring about intellectual changes and that milieus and institutions are carriers of these changes. Expressions like "structures" and "clientele" do occur in his book (pp. 48, 255, 301) as concessions to modern fashion, I assume, but they do not affect the method itself. If Lemerle had used "society" or "pupils of a school" instead, his statements would have been no less valid.

Lemerle's book, exhibiting *la bonne méthode* of all good works, is not revolutionary in its general conclusions. The revival of learning and cultural curiosity that we witness in Byzantium around the year 800 was due neither to a hypothetical stimulus coming from the barbaric West, nor to a return from the East of Hellenistic works and lore once "borrowed" and assimilated by the Arabs (pp. 21, 22-42). There was a period of latency in Byzantium itself between the sixth and late eighth centuries, a latency brought about by concerns of military survival, which put little premium on the traditional cultural equipment of those who ran the state, but there was no interruption of the indigenous cultural tradition (pp. 74-75, 106, 108). The invention of the minuscule and the concomitant transliteration of literary works from the uncial were events of paramount importance, comparable to the introduction of printing (pp. 119-21). Among the great intellectuals of the ninth century, Leo the Mathematician commands most of Lemerle's sympathy; yet, he tells us, Photius was the outstanding figure, and Arethas should have kept to his manuscripts and calligraphers.

There was no interruption in secondary education—and no change in its contents—between late antiquity and the time of the revival after the First Iconoclasm (p. 105); with slight variations, school programs included grammar, rhetoric, dialectic or philosophy (roughly corresponding, we may add, to the *trivium* known in the West since Martianus Capella, Boethius, and Cassiodore), arithmetic, geometry, astronomy, and sometimes music (corresponding to the Western *quadrivium*—the expression "the foursome of the mathematical sciences" does occur in a ninth century *Vita* of Patriarch Nicephorus, ed., C. de Boor, pp. 149 l. 26, 150

l. 12). By the tenth century the state, which needed civil servants and ecclesiastical administrators—"judges, registrars and metropolitans," to cite the relevant text—took a hand in the organization of secondary education and in the appointment of professors (p. 266). All of this is attested for the capital alone, and there are reasons to believe that institutionalized secondary education was limited to Constantinople; the graduating class of a single year in the tenth century may have amounted to a mere two to three hundred pupils for the whole Empire (pp. 256-57). This figure seems woefully small, but I have no arguments suggesting that in fact it was larger.

Emperor Constantine VII was a pedant affecting a turgid style. Yet, through his own works (which Lemerle discusses succinctly, but without omission of essential detail, pp. 270-80), the compilation of excerpts in fifty-three volumes (only a thirty-fifth of this bulk came down to us, pp. 280-88), and the compendium on agriculture (pp. 288-92), he both expressed and instigated the encyclopedic movement of the tenth century, paralleled by a similar current in Islam. This movement brought forth in addition the lexicon called the "Suda" and the hagiographical encyclopedia of Symeon Metaphrastes (pp. 293, 297-99).

Incidentally, Constantine VII may not have been quite the initiator of the encyclopedic movement. In his *Vita Basilii* (pp. 314 l. 12-16, Bonn edition), he himself dated the beginning of the practice of excerpting historical works to the time of his grandfather Basil I (867-886): Basil studied the mores, biographies, handling of affairs, and wars of generals and emperors, and he had the best and most praiseworthy among them excerpted, in order to imitate them in his own actions. However, it is difficult to decide whether this passage of the *Vita* is a reflection of fact or a projection of a desirable cultural trait of the grandson's own time into that of the grandfather's.

In much of what it accomplished the encyclopedism of the tenth century looked back to the Hellenic past, or at least reconciled that past with the tenets of Christianity—which we remember had also been the case in the cultural currents of the fourth and fifth centuries. In Lemerle's view, this second reconciliation was possible because the victory of Orthodoxy over

Iconoclasm and the waning of the Arab danger relaxed the stresses under which Byzantium had lived since the seventh century (p. 300).

Lemerle grants that the Byzantine encyclopedic movement had an air of grandeur. But he is aware of its shortcomings. He also is no mere encomiast, to judge by the following sentence, which stands on one of the last pages of his book: "We are indebted to Byzantium for no contribution to progress" (p. 305). It is not easy to refute that sentence.

Byzantinists, even the most fault-finding among them, will be hard put to suggest additions to Lemerle's bibliographies or to correct his facts; for this, he is too consummate a craftsman. During my own reading, I noted only the following: (1) in the bibliography, various contributions by V. von Falkenhausen, K. Setton, H. Cherniss, O. Volk, I. Ševčenko, V. Laurent, A. Cameron, and K. Weitzmann might have deserved mention on pp. 19 n.29, 43 n.1, 44 n.3, 111 n.9, 135 n.106, 240-41, 135 n.107, 268 n.6, and 271 n.18 respectively; (2) the earliest dated minuscule manuscript was found by Uspenskij not in the St. Sabas monastery, but on Sinai, where most of it is still preserved (cf. p. 113 n.13); the *Life* of Thucydides excerpted in Constantine VII's *De Virtutibus* has nothing to do with Ammianus Marcellinus (cf. p. 286 n.58). But Byzantinists will differ from Lemerle on a number of points—some technical, where evidence seems to be less untrustworthy or less equivocal than he assumes, and some general, where that evidence just does not speak with a clear voice. I wish now to submit a few samples of differing opinion. The first three will concern technicalities.

(1) Lemerle doubts whether Constantine, the Apostle of the Slavs, was ever a pupil of Photius (pp. 163 ff.). Few, if any, students of the Slavic *Vita Constantini* will share this skepticism. The compiler of the *Vita*, whether—as I believe—he wrote in Rome soon after Constantine's death (869), or elsewhere about 885, had no interest in associating his hero with a disgraced prelate just to make him look better. Lemerle, to be fair to him, admits that Constantine could have been acquainted with Photius; we know that for sure, since the friendship of the two is attested by an independent—and contemporary—source, Anastasius Bibliothecarius.

(2) Photius, we hear, was not a teacher (pp.

183 ff.); but he was, for he called himself just that, *didaskalos*, of Amphilochios, and he addressed his *Lexicon* to a student (*mathētēs*); to differentiate between the terms *maître* and *professeur*, *disciple* and *élève*—as Lemerle does—is to make too fine a distinction (pp. 197, 199, 201). Moreover, in Photius's letter to Pope Nicholas I, I detect allusions to the regular *trivium* and *quadrivium* instruction (Lemerle thinks of an "intellectual club" instead, pp. 198–99).

(3) The date of composition, or publication, of Photius's *Lexicon* and *Bibliotheca* are still vividly debated. Lemerle assigns both to Photius's youth (pp. 185–89). But the *Lexicon* was dedicated to a distinguished official, Photius's former *mathētēs*, whom the author addressed without undue deference; surely a reasonable time must have elapsed between the official's student days with Photius and the point when he acquired his considerable position in the hierarchy. Lemerle suggests that Photius was born about 810 (pp. 180–81; I agree) and that the *Bibliotheca* was ready in 838 (p. 190; I do not agree). If Photius had been collecting material for this work for twelve to fifteen years before publishing it, as Lemerle himself proposes (pp. 190, 192, 200), he must have been a child prodigy, who started his note-taking possibly at the age of thirteen. In its final form the *Bibliotheca* is a late work.

The following three examples are of more general nature: (1) In the intellectual life of the Empire, Lemerle assigns a central position to the capital. This is certainly true for the ninth and tenth centuries, but it is only partly so for the time before the Arab conquest, when older established centers led an intellectual life of their own. In the fourth century Emperor Julian was educated not only in Constantinople but also in Nicomedia and Athens (cf. p. 60). The pagan philosophers who shortly before 532 emigrated to the court of the Persian King Chosroes—probably from Athens, but we are not sure—were said to have come from Syria, Cilicia, Phrygia, Lydia, Phoenicia, or Gaza—not a single one was called "Constantinopolitan." And even in the seventh century, as Lemerle himself narrates (pp. 81–85), the Armenian Ananias of Shirak learned the science of the Greeks at Trebizond, and people from Constantinople were said to go to study there.

The monopoly of the capital in matters intellectual was a result of the Arab catastrophe.

(2) The origins of the first Byzantine humanism will long remain a secret to us, for they go back to the obscure eighth century. It was in full bloom before the victory of Orthodoxy in 843. This can be attested by anyone who has read a few pages by authors—Orthodox or Iconoclast—who died about that time; such as the Orthodox Patriarch Methodius or the iconoclastic Bishop of Nicaea, Ignatius. Some years before the grammarian Kometas "renewed" (re-edited?) a manuscript of Homer (p. 166), Ignatius interspersed his correspondence with Homeric quotations; he must have possessed a collection of proverbs and possibly used the *Lexicon* of Phrynichus. Ignatius was a pupil of Patriarch Tarasius (787–806), who was born about 730, at the outset of the First Iconoclasm, and Tarasius—as Lemerle himself reminds us indirectly (p. 129)—must have absorbed his knowledge of antique metrics under the allegedly obscurantist Iconoclast Constantine V (d. 775). Theodore of Studios, too, got much of his good education under the same emperor (cf. p. 123). Thus there is no necessity to connect Byzantine Christian humanism with the triumph of Orthodoxy over Iconoclasm and with Photius (p. 196) or to assign to Iconoclasm a tendency to "obscure" the classical strain in intellectual tradition, a strain whose "unconscious" bearers the iconodules were (p. 108).

(3) To Lemerle the iconoclastic struggle is a struggle between the Asiatic and the Greco-Roman traditions (pp. 34, 74, 107). This may have been so; it is close to Ostrogorski's formulation given in his *History of Byzantium*; still, the thesis that Iconoclasm is a conflict between Europe and Asia is not borne out by such texts as I know. Nor does it explain the fact that the intellectual roots—by which I mean contents of school education—of Iconoclasts and Iconodules were alike; alike was also the humanism of iconoclastic and iconodulic intellectuals of the ninth century—Leo the Mathematician was an Iconoclast (in spite of Lemerle's reservations on p. 157), Photius, an Iconodule, and Ignatius of Nicaea was both.

Professor Lemerle's book grew out of years of teaching and cooperation with the group of disciples assembled and encouraged by him in Paris. To quote the names of these disciples is

to give the roster of the presently flourishing French school of Byzantinology: Ahrweiler, Astruc, Dagron, Gouillard, Grosdidier de Mâtons, Guillou, Irigoin, Svoronos. In his book Lemerle repeatedly refers to their research and acknowledges their assistance. Even the whipping boy, Hemmerdinger, proved helpful, for his extravagances inspires many of Lemerle's penetrating rebuttals.

The excellence of the book, of course, is due to the author himself. For the sake of comparison, take four recent publications on various aspects of Lemerle's topic, coming from authors of four different nationalities: E. È. Lipšic, *Očerki istorii vizantijskogo obščestva i kul'tury, VIII-pervaja polovina IX veka* (1961); J. Irigoin, "Survie et renouveau de la littérature antique à Constantinople," *Cahiers de civilisation médiévale* 5 (1962): 287-302; S. Impelizzeri, "L'umanesimo bizantino del IX secolo e la genesi della 'Biblioteca' di Fozio," *Studi Storici in onore di Gabriele Pepe* (1969), pp. 211-66; and Arnold Toynbee, "Constantine Porphyrogenitus' Works," being part 5 of his *Constantine Porphyrogenitus and His World* (1973), pp. 575-605. Of the four items, none equals the brilliancy and penetration of the corresponding parts of *Premier humanisme*, even though the first three are works of substantial merit. Only the fourth, I regret to say, offers little for a historian of culture. For an understanding of Constantine VII's intellectual world, one derives incomparably more from Lemerle's short last chapter than from Professor Toynbee's weighty tome.

Lemerle's current teaching and study has turned to Byzantine humanism of the eleventh century, centering around Psellos. Byzantinists and intellectual historians at large are awaiting the sequel to his *Premier humanisme byzantin* with great anticipation.

IHOR ŠEVČENKO  
Harvard University

APOSTOLOS D. KARPOZILOS. *The Ecclesiastical Controversy between the Kingdom of Nicaea and the Principality of Epiros (1217-1233)*. (Byzantina Keimena kai Meletai, 7.) Thessaloniki: Kentron Byzantinon Ereunon; distrib. by Library Grigoris, Athens. 1973. Pp. 108.

The disaster that overwhelmed the Byzantine Empire in 1204 was, within a few years, par-

tially redeemed by the formation of Byzantine enclaves in Nicaea, Bithynia, and in Epiros. Eventually the Nicene power, under the Lascarid dynasty, would regain the old capital and refound the unitary Empire, much reduced and thoroughly traumatized, but a going concern. For a quarter-century, however, two Greek-ruled states claimed the imperial succession. This was anomalous and unnatural, especially since both the Nicene Lascarids and the Epirote Angeloi supported their imperial identities with the sanctions of the Byzantine Church.

The complications arising from the symbiotic—not, as the author claims, "dyarchic"—relationship between *basileia* and *ekklesia* give Mr. Karpozilos his central structure. The Constantinopolitan patriarchate had moved from the capital to Nicaea and there stood behind the Lascarids. Yet in 1224 Theodore Angelos was crowned emperor in Thessalonika, the second city of the Empire and his recent war prize, by one of the ecclesiastics who upheld the house of Angelos. To sharpen the conflict a number of the embroiled clergy were talented intellectuals in the Byzantine stamp, argumentative individuals like John Apocaukos, Dimitrios Chomatianos, and George Bardanes on the Epirote side, and the Patriarchs Manuel I and Germanos II in Nicaea. At the height of the polemic the Epirote bishops pushed their support of "their" emperor to the point where they denied the ecumenical suzerainty of the patriarch himself, and schism followed. This schism was only healed when the political power of Theodore Angelos was truncated (after 1230) and the Nicene dynasty remained the only power capable of continuing the Byzantine imperial tradition.

Karpozilos uses the surviving correspondence between the two ecclesiastical camps to describe well the subtle but potent ties that connected Byzantine Church and state. The crisis of 1224-33 is properly identified here as a dramatization of the peculiarly Byzantine interpenetration of sacred and secular powers. The monograph is occasionally awkward in execution; at one point Karpozilos creates an unnecessary obscurity: a brief paragraph should explain more fully *why* and *how* Theodore Angelos became a cropper, leaving the field to Nicaea. In sum, however, the author manages to insert a modest

but significant tessera in the mosaic of Byzantine religious politics in the thirteenth century.

D. A. MILLER

University of Rochester

#### MODERN EUROPE

GEORGES CHANTRAINE, S.J. "*Mystère*" et "*Philosophie du Christ*" selon Erasme: *Étude de la lettre à P. Volz et de la "Ratio verae theologiae"* (1518). Preface by H. DE LUBAC. (Bibliothèque de la Faculté de Philosophie et Lettres de Namur, number 49.) Namur: Secrétariat des Publications, Facultés Universitaires. 1971. Pp. x, 410.

Few historical characters have been subject to so many diverse interpretations as Erasmus. Some regard him as a rationalist, a precursor of the Enlightenment, neither a Catholic nor even a Christian, who unfortunately wasted time trying to prove that he was. Others say that he was indeed a Christian but by no means a Catholic, seeing that his type of religion would disintegrate the Church. Still others say that he was a good Christian and a devout Catholic. Recent interpretations have been veering in this direction, and one of the most persuasive among them is the present study.

It is a work of extensive learning. The author is well versed in Erasmus and has covered a wide range of the contemporary literature. His goal is to isolate the core of Erasmus's religion, to formulate for him what he was never able to formulate for himself. This means making the implicit explicit. The author narrows his task by directing his attention to two works in which Erasmus did make an effort to explain his version of the Gospel. The first is the letter to Volz, the second his introductory letters to the editions of his New Testament, commencing in 1516 and amplified in subsequent editions. The development of Erasmus up to this point is briefly surveyed. The author takes a middle ground between those who see a radical break in Erasmus from classical to Christian literature and those who find an unbroken continuity. The author finds a shift from an esthetic theology to a theological esthetic. As for *The Praise of Folly*, he quite agrees with Erasmus himself that its purport was no different from that of the *Enchiridion*.

When we come to the detailed examination of the Volz letter and the *Ratio Theologiae*, the

core of Erasmian piety is found epitomized in the two words "*mysterium*" and "*sacramentum*." *Mysterium* means the sense of the Holy, the numinous, the outpouring of the Holy Spirit, the warm response of the heart, illumination, rapture. Erasmus was constantly inveighing against the late Scholastics not simply because of their interminable distinctions as because of their detachment from vital religion. The author is absolutely right in finding all of this in Erasmus, and he has served his readers well in assembling so many citations. This emphasis is distinctive within Erasmus, but how distinctive at this point was Erasmus? The author recognizes that this was the piety of the Brethren of the Common Life. The *Imitatio Christi* said that the "Trinity is better pleased by adoration than by speculation."

The second word as a key to Erasmus is "*sacramentum*." The author devotes special attention to Baptism and the Eucharist. The first is an initiation into the Christian life, the second is a nourishment whereby the Christian is transformed into the likeness of Christ. All of this is very true. But the emphasis with Erasmus is always on the spiritual attitude rather than the outward act. He claimed that baptism in infancy is of little import unless re-enacted at the age of puberty, when the initiate understands its significance and voluntarily takes for himself the baptismal vows. The Sorbonne told Erasmus he was an Anabaptist (a repeater of baptism). He replied that he had in mind not repetition but re-enactment. I have the feeling that this was a semantic subterfuge. As for the Eucharist, he could believe in transubstantiation because the Church said so, but participation would be of no avail without the Spirit. Here one needs not only to read between the lines but also to scrutinize the behavior. Erasmus was a priest. He mentions carrying his prayer book and indicates that he had heard confessions, but he never once mentions having said Mass. He must have done so, but the outward act did not compare with inward prayer and the unbearing of the soul.

To assess the essence of a man's religion one must compare him with his predecessors and contemporaries. What differentiates him from others? Erasmus stressed the warming of the heart. Theology should be measured less by a ruler than by a thermometer. True, but the

same could be said of Luther. When he read the words of the angel "unto you is born," he exclaimed, "'Unto you! Unto me. When I read those words I hate myself that my heart does not leap into flame." More space might well be devoted to the comparison of Erasmus and Luther.

These reflections arise from the reading of a book that prompts reflection.

ROLAND H. BAINTON  
Yale University

ROBERT FORSTER and JACK P. GREENE, edited with an introduction by. *Preconditions of Revolution in Early Modern Europe*. (The Johns Hopkins Symposia in History.) Baltimore: Johns Hopkins Press. 1970. Pp. 214. \$8.95.

This volume must be understood for what it is not, no less than for what it attempts to be. It is the result of a colloquium on modern revolution held at Johns Hopkins in 1969; it is not another round in the "general European crisis" controversy launched by E. J. Hobsbawm and H. R. Trevor-Roper in the pages of *Past and Present* and reprinted in part in Trevor Aston, ed., *Crisis in Europe, 1560-1660* (1965). Some of the events covered and some of the five contributors do look familiar: Roland Mousnier writes on the *Fronde*, J. H. Elliott surveys the revolts against the Spanish Empire in Italy and Iberia during the 1640s, and Lawrence Stone has a long essay on the causes of the English Civil War (subsequently expanded still further to form the bulk of his *The Causes of the English Revolution*). But there is also an account by J. W. Smit of the origins of the revolt of the Netherlands, which, like Stone's essay, seeks to relate the event in question to theories of revolution now current among social scientists, and Marc Raeff provides a final chapter on Pugachev's rebellion in 1773-74, chosen in preference to a Cossack uprising more contemporaneous with the other revolutions on the grounds that the Russian Empire had only just reached the state of administrative and social development attained by France and Spain over a century earlier.

The participants in the *Past and Present* debate used a rough-and-ready definition of "revolution," arguing that whatever its particular form, the widespread incidence of political and

collective violence in mid-seventeenth-century Europe suggested a common crisis with common roots. By contrast Forster and Greene follow J. H. Elliott closely in his skepticism over an alleged general crisis and, instead, renew the old debate over the distinguishing marks of a genuine revolution as against a mere revolt. Their eighteen-page introduction is devoted to problems largely taxonomic in nature. They want first to classify the types of violence under discussion, separating out the simple uprisings and coups from the more fundamental upheavals, and then to outline the "preconditions" of these events in categories that, while recognizing the peculiarities of early modern Europe, are still applicable to modern revolutions in general. If *Preconditions of Revolution* has an ancestor, or more accurately an analogue, it is *Anatomy of Revolution* and not *Crisis in Europe*.

Forster and Greene never fall into Crane Brinton's more mechanistic modes, and they are careful to respect the historical integrity of their subjects. They distinguish between early modern and more recent revolutionary ideology, for example, emphasizing the former's concern for the recovery of a "golden age," and they couple their talk of "extensive social dysfunction" with references to "the need to guard the local *patrie*" against the encroachments of central government. Yet for all the editors' caution, their attempt to find an appropriate place for early modern European phenomena under the general heading "revolution" inherently obscures even as it stimulates. To ask for the "preconditions of revolution" in their terms is to assume that the investigation deals with the extraordinary causes of vulnerability in states ordinarily stable and well established when, in point of fact, the five contributors are dealing with ramshackle, multinational, multiconstitutional empires of relatively recent vintage and limited military and fiscal resources. Thus as the eighth of nine preconditions Forster and Greene posit an established authority that must be "sufficiently weak as to make it doubtful that it could offer sustained resistance." As it happens, one of their more successful revolutions, the war for the establishment of the United Provinces, was fought against the strongest state in Europe at the height of its power, an undertaking characterized by William the



Silent in 1577 as "a worm turning against the King of Spain."

Most of the other difficulties of the book are endemic to the genre, the published results of academic conferences. The participants share the same vocabulary, but not always the same concepts, even for the vital term "precondition." Despite the manful attempts of the editors, meaningful comparison between essays is often difficult. Some of what is said has been said before and will probably be said again, other things are unfortunately left unsaid (for example, Mousnier's essay makes no mention of his attempt to find an alternative formulation for social conflicts in answer to the Marxist analysis of Boris Porchnev). Nonetheless, Forster and Greene did manage to organize a conference where working scholars of high reputation exchanged their views on one of the most current subjects in European historiography. The record of this exchange has its exasperating moments, but if the problems of the early modern revolts are not all solved at least most of them have been clearly stated in an important set of essays.

STEPHEN FOSTER

*Northern Illinois University*

JOHN A. ARMSTRONG. *The European Administrative Elite*. [Princeton:] Princeton University Press. 1973. Pp. xii, 406. Cloth \$20.00, paper \$9.75.

It may seem odd for an American historian to review a book on Europe, but, in fact, this is primarily a study by a political scientist of government and economic growth, which to achieve comparative perspective uses data from four European nations. Armstrong's specific aim is "to suggest ways in which modern administrative elites generally may relate to economic development."

In order to deal with this problem he has to consider basic problems of social organization and action, such as why some patterns of behavior persist, and the relations of the later influence of peer groups to early family socialization. His principal theoretical tool is role theory, with a properly strong emphasis on perception. Among some of the interesting theoretical propositions that emerge from the data are the contentions that both social and

role patterns in each nation have had remarkable stability over time and that middle-range elements such as special roles cannot be compared between nations without careful regard for the total societal setting.

There have been earlier studies of European administrative elites, noted in this volume's extensive bibliography, but they have concentrated more on the social status of the men in government control centers than on changing conceptions of their roles. Armstrong traces the development of what he calls (probably in order to avoid still more words) a developmental interventionist role definition.

In all the nations used for comparison (England, France, Prussia, and Russia) such a definition was almost totally lacking in the roles of eighteenth- and early nineteenth-century administrators. While the *Ecole des Ponts et Chaussées* was started in France in 1744, its graduate engineers did not feed into the country's administrative elite, which in France and elsewhere was still recruited on aristocratic principles. In fact much of the book underlines the influence of agrarian aristocratic and haute bourgeois values, attitudes, and educational controls in preventing the rise of scientific and technological interests in this elite. Disciples of Saint-Simone in France, in the 1830s and 1840s, were among the earliest exponents of economic development, but they made only a temporary impression on the government bureaucracy, one that disappeared in the early years of the Third Republic. Without regard to local characteristics and differences *laissez-faire* doctrines continued to dominate the European governments, with few exceptions, up to World War I.

Some chapter titles suggest the scope and novelty of Armstrong's approach: "Family and Socialization," "The Structured Adolescent Peer Group," "The Classics Barrier," "Higher Education as Ideology," "Induction to Higher Administration," and "Career Patterns and Prospects." Needless to say the range of knowledge and linguistic skills required for this book were enormous. While I am not competent to pass on the accuracy of much of the data, I can affirm that this is the type of history that mature scholars ought to attempt. It provides clues to permanence and change in societies that are buried and lost in the ordinary narra-

tive history. It is also encouraging to find a political scientist immersing himself in long-range historical research.

THOMAS C. COCHRAN

*Eleutherian Mills-Hagley Foundation*

H. S. K. KENT. *War and Trade in Northern Seas: Anglo-Scandinavian Economic Relations in the Mid-Eighteenth Century*. (Cambridge Studies in Economic History.) New York: Cambridge University Press. 1973. Pp. xv, 240. \$17.00.

This book by Dr. H. S. K. Kent is the outgrowth of a doctoral dissertation presented to the University of Cambridge in 1955. Dr. Kent bases his conclusions on manuscript sources, and he has read widely in the printed materials both contemporary and current. As a result he has produced a very thorough study of a highly complicated problem. His comparison of statistics and observations found in British and Scandinavian archives helped him to reach sensible conclusions on both legitimate trade and smuggling between the two areas.

Two treaties of peace and commerce made in the second half of the seventeenth century regulated Anglo-Scandinavian trade in the mid-eighteenth century. They implied that the commercial powers of the parties engaged would remain constant and that warfare would not change nor would the rules on contraband and trading with the enemy. "Considering the importance and volume of Anglo-Scandinavian trade, it was remarkable that government in England had so little influence on it. On the surface, England appeared to regulate trade by legislation and treaties. In fact, it was the merchants themselves in their lawful or illicit enterprises who directed trade in blissful disregard of government whenever and wherever regulations bore too heavily on them" (p. x).

Dr. Kent in the text and by an appendix written in 1759 by F. L. Fabricus of Norway shows how trade was organized. Timber, iron, and smuggled tea were England's most vital imports from the northern seas. Britain's fleet was dependent upon Scandinavia for its naval stores until after the American Revolution, and the availability of cheap tea smuggled from Scandinavia (and elsewhere) was responsible for the habit of tea drinking becoming widespread in England. The Danish and Swedish money markets were controlled by the trade in tea,

which usually went hand in hand with smuggling spirits. England in turn sent many illicit manufactured items into Scandinavia.

English commerce in the northern seas was necessary for the English who did not want a break with the Scandinavian countries during the Seven Years' War, for the trade was vital to the maintenance and extension of the Empire: timber for ships; iron for ship-fittings, armaments, and industry; and low-priced tea to keep the English people contented. England prevented France from getting the assistance she expected from the Scandinavian merchant fleet, and, at the cost of much friction, England was able to impose her will upon the two Scandinavian powers. At one time an arrogant Cabinet was saved more by luck than skill from preventing a diplomatic rupture. "In the last resort, it was due to the merchants rather than their governments that trade in the Northern Seas continued uninterrupted in war as in peace" (p. 177).

Appendixes and statistical graphs provide insights into legal and illicit trade, and the index and bibliography are excellent. The style at times is halting and pedestrian, but a subject of this type does not easily lend itself to literary smoothness. This work by Dr. Kent is one that will be consulted in the future by scholars working on the problems of war and trade in the northern seas in the mid-eighteenth century.

JOHN J. MURRAY

*Folger Shakespeare Library*

RENÉ GIRAULT. *Emprunts russes et investissements français en Russie, 1887-1914: Recherches sur l'investissement international*. (Publications de la Sorbonne. N. S. Recherches 3.) Paris: Librairie Armand Colin. 1973. Pp. 618.

There is no doubt that this is a significant book on a significant subject. The French lending to Russia in the three decades preceding World War I remains an important economic and political phenomenon in the annals of the period. There is much that can be learnt from this enormous, closely printed volume of six hundred pages, which also, incidentally, appears in abridged form. It is based on Girault's original doctoral dissertation, which was actually three times its present length.

The most impressive feature of this study is

the unbelievable amount of sheer labor that was invested in it. One can only marvel at the volume of information the author was able to gather from a large number of archives in France (both private and governmental) and Russia. Supporting and enriching this information are abundant references to memoirs, diaries, and a large body of secondary literature. Of the banking archives those of the Crédit Lyonnais and of the Banque de l'Union Parisienne proved the most productive by far.

The author is able to present in two tables (pp. 85-86), year by year, from 1888 to 1914, the cumulative volume of French loans to the Russian government (including the government guaranteed loans to Russian railroads and municipalities), as well as, again annually, the values of French direct investment in Russia in the aggregate and also separately by three basic industries, textiles, and banks. In listing Russian public borrowings the author usefully specifies both the nominal and the estimated real values, and it is noteworthy that by 1914 the former exceeded the latter by nearly 60 per cent. All the figures are given in current francs, so that the term "real" refers not to constant purchasing power, but to the funds below the par value of the bonds that Russia actually received. This is very useful information, even though some further statistical processing thereof would be very desirable, either by taking into account the changes in the value of money, or by relating the data to some significant and changing aggregate magnitudes (such as capital formation or industrial output). But for reasons to be mentioned presently our author is hardly the man to engage in such calculations.

What the reader will get from this book is an enormous mass of details both to economics and politics of Franco-Russian financial relations. He will also receive a clear idea of the techniques used in floating Russian securities and in foisting them upon the willing but, alas, gullible French savers. This surely provides sufficient justification for the book, as far as it goes. But how far does the author go in utilizing his own abundant harvest? Certainly less far than he could and should.

M. Girault has some fundamental ideas on the approach to history in general and to his specific subject in particular. These ideas can be summarized very quickly. Econometrics, the

author says, has its merits, and quantitative data are useful, but they do not go to the heart of the matter. At the basis of history stands man, who by virtue of his reason and his will has the possibility of choices. Hence the effort of the historian must be oriented toward the comprehension and explanation of those choices. Quantitative research, which it would be fallacious to shun, cannot reach the truly essential, that is to say, the causality of human decisions (p. 11).

Girault's is in principle an altogether possible and not uncommon view, but it is hardly exciting enough to wax lyrical over it, as the author does. Similar thoughts have been expressed before, for instance, by the notable Swedish economist Johann Ackerman, except that Ackerman was also a master of statistical and analytical methods. The point that the author ignores is that causal problems emerge from quantitative approaches at every level and that at some levels human motivations can reasonably and effectively be taken for granted within the framework of the analysis.

The great curiosity of the book, however, is something else. Despite the eloquent *profession de foi*, despite all the snide remarks about the misleading nature of statistics, there is precious little in the book about motivations and decisions. Throughout the volume the overriding explanatory concept is necessity. *Nécessité fait loi* is the author's favorite phrase. It is an ineluctable necessity that forces the Russians to import capital and to equilibrate the balance of payments and the budget and to promote the economic development of the country. It never occurs to the author that he might ask whether it was really at all times "necessary" to import precisely as much capital as was actually imported, no less and no more; and to do so in order to maintain a rate of industrial growth in Russia that apparently, too, was "necessarily" fixed at a rigid level—not one per cent less and not one per cent more. These are indeed unanswerable questions, but they are inescapably implied in the author's concept of necessity and reveal that concept as something destitute of meaning. He uses the concept with the greatest *désinvolture*, applying it to everything that happened anywhere, including, for instance, the policies of protectionist agrarian tariffs in Germany (p.

222), a subject incidentally of whose complexity he is blissfully unaware.

At the same time, there are recurring references to "economic laws," to the necessity, that is, "to comprehend Franco-Russian economic relations as an aspect of international economic relations which themselves are dominated by laws and rules" (p. 585). "In the capitalist world at the end of the 19th century," he says, "international relations obeyed strict laws" (p. 169). Since the author never deigns to specify the nature of those strict laws and rules the references to them are altogether cryptic, and this for very good reason. The requisite competence in these matters is simply lacking. The author does indeed discuss at some length Russian balance of trade and balance of payments, but in doing so he continually assumes that capital imports are, and in fact must be, the effect of a deficit in the balance of payments on current account. That capital imports just as well can be the cause of that deficit and that the causal nexus may be tied either way appears to be totally unknown, and the transfer problem and with it the mechanism of adjustments in the balance of payments remains a mystery to the author.

The author's ignorance of economics reveals itself disturbingly time and again. It does so when he refers to "economic theorists" in connection with profit maximization and then maintains that with regard to capital exports under certain conditions it "becomes necessary to amplify [*sic*] the profit maximization, that is to say, to exaggerate it" (pp. 294 *et seq.*) while all he wishes to convey by this pseudo-scientific terminology (what is larger than the maximum, what is better than the optimum?) apparently is that at times capital is not tempted to move abroad except by rather high profit differentials. Again, it makes no sense at all to argue (p. 134) that foreign capital would continue to be necessary for Russia until national production had increased to the point from which on the mass of wages paid [*sic*], increasing in its turn would provide consumption and investment with the necessary funds.

A modicum of sophistication would have prevented the author from stating (and italicizing) the following as the golden rule of capital export: "to invest the minimum of capital to obtain the maximum of income." This is a

time-honored logical fallacy and a practical impossibility, but the author does not hesitate to describe his golden rule (which would have amused even the golden hero of Apuleius's *Metamorphoses*) as "an axiom" [*sic*], which is "definitely at the basis of all the projected [financial] operations" and "permits [us] to understand how bankers and industrialists work who invest abroad" (p. 52). The same lack of training becomes visible when the author blandly identifies (p. 131) the volume of securities issued in a country with its aggregate capital formation, probably not realizing that for Russia, since data on output are available, at least some crude estimates of capital formation are possible with the help of assumptions regarding capital-output ratios. Such calculations may well have suggested that the part played by capital imports in the Russian aggregate was a good deal lower than the author believes. He contradicts unwittingly his own belief when he argues that large capital imports in the years before World War I, which he says were the very best years ever for the Russian economy [incidentally, an error], provide a "tangible proof for the weakness" of domestic capital formation (p. 131), forgetting that without capital imports those years might not have been the best ones. At any rate to deduce, as the author does, from an allegedly high rate of capital imports the "necessity" to import capital is, of course, a *non sequitur*. All this is rather unfortunate because disabilities of the sort were so readily remediable. If the author had devoted a minute fraction of the time he spent in the archives to the study of elementary economics, his study would have been immeasurably improved.

The book cannot shed any analytical light; nor is it a piece of probing entrepreneurial history precisely because according to the author "foreign markets obey their own laws and not the will of a few financiers" (p. 200). The bankers and the statesmen are helpless against the sway of the alleged "necessities" that leave them without choices. Without alternatives nothing remains of the author's fundamental philosophy of history. In fact after having been told that it is *man* whose reason, will, and choices make history, the author lets time and again *abstracta* such as imperialism and capital-

ism (and even *capitalisms*) make decisions and act.

The author tries—not without some success—to relate the volume of French capital exports to Russia to the ups and downs of economic conditions in the two countries. Also the faster growth of French direct investment in Russia after 1895 (even though quantitatively remaining considerably below loans to government) is explained in the same fashion. This is not entirely convincing because his references to the stagnation and “atonie” (p. 169) of the French economy with its “Malthusian enterprises” (a terminological curiosity on p. 107) and entrepreneurial weakness lose much of their plausibility after the middle of the nineties when French industry started growing at a fairly high rate.

In general, the author is hampered throughout by his lack of understanding of the processes of industrial development, particularly in conditions of backwardness. He is correct in stressing the strong role of heavy industry in Russia and the attractiveness of those branches of industry for French capital. He advances some reasons for the fact such as easier control, smaller risk, and more regular profits (p. 108), to which he also adds the more dynamic nature of the regions in Russia (pp. 258–59) in which heavy industry was located. The last point surely reverses the causal nexus because the regions were “dynamic” precisely because they harbored heavy industry and did so for good locational reasons, which again are beyond the author’s purview. What he does not understand, although it bears crucially on the problem, is the differential incidence of technological progress in the period under review along with advantages of scale and the linkage effect upon the economy.

No greater perception is displayed in discussing the role of investment banks in Russian industrialization in this century before World War I. It eludes him completely that in that period, as a result of the preceding evolution, Russian industrial development had matured to a point where the country became ready to rely on banks as an important agent in development. What is a clear emanation of the general historical pattern of development is seen instead as a rather accidental phenomenon. When it comes to the role played by the French

*banques d'affaires* in the process, the author quickly develops a dichotomy between industrial and speculative entrepreneurship (which is unpleasantly redolent of Feder’s distinction between *schaffendes* and *raffendes* capital). Thus the investment banks become the real villain of the piece being exclusively interested in high dividends paid by the industrial firms of their concern and in unloading the stocks at higher prices, thus preventing the industries from engaging in profitable long-term investment and foisting a low time-horizon upon them.

Behind this view stands the neglect of the momentous policies of productive entrepreneurial guidance pursued by the German banks (to which a single brief phrase alludes on p. 513) and in fact also of the policies of the Russian banks. But behind it also stands the unwillingness to raise the question whether an aggressive “German-like” policy of the French investment banks would not only have accelerated French economic development, but would also have provided an important alternative to French capital exports and by the same token would have deprived those exports of much of their profitability, which the author is willing to measure narrowly, if not narrow-mindedly, in terms of the existing interest rate differentials. The reader is not told that the problem was not just the question of *existence* of investment opportunities in France, but the *creation* of such opportunities by appropriate vigorous banking action. Discussion of these matters absolutely belonged in the book. As it is, the reader is just surprised to find toward the very end of the book (p. 588) a reference to the lack of capital for French industry and an entirely unelaborated intimation that capital exports were a subject of controversy in French literature.

It is regrettable that the reader’s admiration for the wealth of details he is offered in the book is all too often attenuated by the general questions the author raises and to which satisfactory answers are rarely given and perhaps cannot be given. This is true, for instance, of the “fundamental question” (p. 545) whether in Franco-Russian relations there was a “correspondence between the economic and the political contexts” and whether before World War I “the alliance fundamentally obeyed to

economic considerations, it being understood that other considerations of political, military, and psychological nature influenced the process" (*ibid.*). It is not surprising that no funded answer is given to the "fundamental" question beyond the shallow suggestion of interrelationship between economic and noneconomic factors.

In this connection the author rather lightly touches on the problem of origins of World War I and to do so worries for all it is worth the decline of stock prices that occurred in May and June 1914. He asks: Was this just a stock exchange crash or was it the end of the long upswing and in this sense premonitory of things to come? No answer, we are told, can be given, but "the question remains posed" (p. 585), which cryptic phrase apparently means the justification for asking a further question: Did the capitalists want the war as a means of escaping from the threatening crisis? (p. 575). The author is uncertain: The crisis apparently [*sic*] did not play a role in the actual outbreak of hostilities, but nevertheless its importance must not be underestimated. Thus also this question "remains posed," that is to say in the author's words "risks to remain without a precise answer," because as he assured us earlier "no banker, no industrialist, even no armament maker can humanly want the war" (p. 546). All this back and forth, all these questions without answers that are gratuitously raised and serve only to remind us that a scholar should be wary of becoming a *causeur*.

The impression of the author's superficiality in dealing with general problems is reinforced when in the concluding paragraphs he suddenly comes to speak of the "blossoming forth" of French imperialism, which again he calls a "fundamental phenomenon." A quick definition is offered: Economic imperialism means successful attempts on the part of a developed power to reserve for itself the economic domination over a weaker state. Were the Franco-Russian economic relations to be understood in these terms? The answer is in the negative. The French capitalists had indeed tried to achieve that goal, particularly between 1906 and 1909, but by 1914 they had not yet succeeded, being prevented among other things by the shortness of time and the reversal of the cycle (which we were told may or may not have actually oc-

curred). But the attempts, we are assured, would have continued (p. 593), the author thus leaving us with another posed and answerless question.

It is indeed unfortunate that an enormous scholarly effort of this kind is marred by inane speculations that are bound to irritate the reader and to blur in his mind the feeling of gratitude for the richness of the factual material offered. And yet it is to be hoped that the book will make its mark and that much of the information, so sedulously collected, will be incorporated, with knowledge and imagination, in a variety of constructive explanatory frameworks.

ALEXANDER GERSCHENKRON  
*Harvard University*

HANS A. SCHMITT, editor. *Historians of Modern Europe*. Baton Rouge: Louisiana State University Press. 1971. Pp. xviii, 338. \$11.00.

This collection of essays continues what its editor calls "a Chicago tradition"—a *Festschrift* with a theme." The book is dedicated to S. William Halperin, who is also the subject of a brief introductory appreciation by Hans A. Schmitt. This "interim report" on Halperin's scholarly work points out that one of his emphases as editor of the *Journal of Modern History* was publishing historiographical articles. So this *Festschrift* happily combines an interest of the honoree with an even older Chicago tradition of historiography as the theme of a book of essays.

Within this broad theme the *Festschrift* turns out to be a *mélange*. Three essays treat only a part of the subject's work. In the briefest of all, Edward Whiting Fox argues that, after transforming history into phophecy, Arnold J. Toynbee rejected the role of prophet because he refused to choose between his belief in freedom of the will and his distrust of human self-centeredness. George T. Peck's account of Gaetano Salvemini as a champion of the South asserts but fails to demonstrate that his historical work between 1896 and 1911 was largely inspired by *meridionalismo*. William Savage deals only with Jacques Chastenet's books on recent French history, stressing the breadth of view, versatility, and elegant style of a participant-observer.

New York universities are represented by three historians. Hans Kohn is "judged on the basis of his own standards" by Louis L. Snyder, who points out the relationship between the Enlightenment and Western nationalism in Kohn's thought, but does not explain why he chose to deal with the topic. Why Carlton J. H. Hayes took up nationalism is clearly analyzed by Carter Jefferson. Hayes's vision of the medieval socioeconomic structure and his Catholicism combined to produce an attack on capitalism and secularization that had opened the gates to nationalism. Jefferson also elucidates the initial success of a conservative Catholic in a liberal Protestant academic and intellectual establishment by pointing out that Hayes's conclusions coincided with contemporary conventional wisdom. Kenneth F. Lewalski shows how Oscar Halecki's work with the League of Nations led the historian of Poland into the mainstream of international historiographical debate and to the realization of the historical significance of Catholicism, paving the way for his concern with the meaning of Europe as a civilization of diversity.

Charles F. Delzell opens his essay on Adolfo Omodeo with the observation that in Italy politics and historiography have been closely related. This is borne out in four essays, the one on Salvemini having already been mentioned. Delzell successfully relates Omodeo's transition from a liberal monarchist to a democratic republican through his study of the *Risorgimento*. Federico Chabod's leadership of the Piedmontese resistance movement at the end of World War II and his advocacy of autonomy for his native Val d'Aosta appear to have been an aberration in a brilliant career recounted by A. William Salamone. Edward R. Tannenbaum expresses surprise that a nationalist historian, Gioacchino Volpe, would stress the social and economic roots of politics, but nevertheless manages to show how this fitted into Volpe's acceptance of fascism.

Besides Toynbee, the English historians included are A. J. P. Taylor and J. L. Hammond. H. Russell Williams explicitly states that "Taylor's background does not explain his craving for paradox or his hostility to orthodoxy," but despite an excellent critical evaluation of Taylor's publications, he finally is unable to explain the "triumph of perversity." Henry R. Winkler

develops the quality of life as the central theme of the studies of the Industrial Revolution published between 1911 and the 1930s by Hammond, a journalist like Chastenet, and his wife Barbara. Surely her name should have appeared in the title of the essay.

France is represented by Ernest Labrousse as well as by Chastenet. Pierre Renouvin offers a fair and dispassionate appraisal of his significance in shaping the study and profession of history in France while expressing basic reservations about the importance Labrousse attaches to economic explanations. By focusing solely on the main features of his work, however, Renouvin conveys the image of a personality no more intrinsically interesting than the movement of prices in the eighteenth century.

Gerhard Ritter is the sole German entry. For William Harvey Maehl, Ritter's participation in the opposition to Hitler represented "responsible elitism" and expressed his emphasis on the history of individuals and ideas rather than on the masses and the material. Significant as Ritter is, Maehl's overly long essay could not have been complete, and its succinctness would have been improved had he left the attack on Fritz Fischer to Ritter rather than participating himself. Moreover, as a Bowdoin undergraduate, I learned from E. C. Helmreich, a product of the Harvard of Fay and Langer, that "guilt for the war was nicely divided," as Maehl phrases it. But my early mentor has never depicted himself, as Maehl does here, in the company of Oncken, Delbrück, and Meinecke as one of "Germany's leading historians" who held that the war aims of Ludendorff and Hindenburg were more rabid than those of Bethmann-Hollweg.

Four points remain. The book's organizational division into "Anglo-American Perspectives" and "Continental Perspectives" is meaningless. The inclusion of Hayes is a silent, salutary reminder that one does not have to have been born in Europe to be a historian of modern Europe. Tannenbaum's brief comparison of the common departure point and subsequent divergence of Volpe, Salvemini, and Benedetto Croce tantalizes and deserves to be followed up. And Salamone's sensitive explication of Chabod as a whole person deserves to

be followed, too, for it is a sophisticated, elegant model for a historiographical essay.

HERBERT D. ANDREWS  
Towson State College

F. P. KING. *The New Internationalism: Allied Policy and the European Peace, 1939-1945*. (Library of Politics and Society.) [Hamden, Conn.:] Archon Books. 1973. Pp. 230. \$12.00.

R. C. MOWAT. *Creating the European Community*. New York: Barnes and Noble. 1973. Pp. 235. \$7.00.

These two small volumes consider the political and economic reorganization of Europe during and after the Second World War: F. P. King analyzes Allied peacemaking in Europe during the war (that is, the negotiations and tribulations of the satellite countries in leaving the war), and R. C. Mowat deals with the postwar diplomacy in Western Europe that produced plans eventuating in the Common Market and its recent enlargement to include Great Britain. The King book uses the newly opened British diplomatic archives, and the Mowat volume is based on published sources.

Each of the books is a modest contribution to knowledge, the information therein mortgaged by a point of view. King covers a wide series of negotiations with a large amount of detail. He relates the military strategies of the Western Allies and the Soviet Union and properly notes how the failure to open a second front in France before 1944 created much dissension. He describes the making of peace for Romania, Bulgaria, Hungary, Yugoslavia, Albania, Greece, Finland, the Baltic States, Germany, and Austria, devoting special attention—and rightly so—to Poland. He also describes the growth of the United Nations idea, ending his account with Yalta. As a summary the book has value, but there is a strongly critical strain throughout that seems to derive from the author's dislike of Churchill and, to a lesser extent, Roosevelt, and from his approbation, perhaps even enthusiasm, for the diplomacy of the USSR. In a half paragraph King treats the Katyn Forest massacre as an episode in Polish-Russian relations.

The Mowat volume is a work of devotion to the unity of Europe, and especially the idea of Christendom. The author traces the wartime

resistance movements and federalist ideas and gives in detail the development of the Marshall Plan and the activities of Jean Monnet and Robert Schuman; culminating in the Common Market and, after the years-long obstructions of Charles de Gaulle, the recent entrance of Britain. His presumption is that at last the essential unity of Europe is asserting itself. He presumes also that this is a result of Christian qualities in most of the major figures, public and private. He often describes their religious outlooks, and if they happened to be irreligious, as in the case of Monnet, he points out that Christian qualities nonetheless were present. The book's range made the author's task of information-gathering difficult, and his wishes perhaps were father to some of the information, as on page 33 where he acclaims the late Harry S. Truman as "a respected presiding judge for eight years in the County Courts" and (shades of the "Missouri Waltz"!) "a sensitive man who had nearly essayed a career as a concert pianist."

ROBERT H. FERRELL  
Indiana University,  
Bloomington

A. D. FRANCIS. *The Wine Trade*. (The Merchant Adventurers.) New York: Barnes and Noble. 1973. Pp. vi, 353. \$12.50.

No doubt editors differ considerably in calculating their responsibility to authors; no doubt authors differ as much in their response to editorial advice; but all should agree in providing readers with a well-organized book. This mélange of incidents, statistics, and gossip by the former English consul-general at Oporto is a case in point. It recalls nothing so much as the first draft of a doctoral dissertation wherein the writer seems to have strung together every note he took. Since in the present instance the chief sources were port books, state papers, and such, the product, without rigid editing, understandably becomes a tissue of miscellaneous information with little coherence. The topic sentence of a paragraph is followed by statements that in no way fulfill its promise; many sentences must be read two or three times to extract their meaning and/or relevance, which often turns out to be relatively unimportant.



Following some casual references to evidence of wine and wine trade in England during the first Christian millenium Mr. Francis begins his detailed account with the fourteenth century. By the sixteenth century the Crown had drawn the main lines of policy and the taste for wine had reached a discriminating stage, though it must be immediately emphasized that tastes in wines fluctuated a great deal, often for reasons that had nothing to do with quality. International relations influenced the consumption of this or that wine quite as much as good seasons, the capacity of wine to travel or keep, fashions, or the discovery of new vintages. If the reader has patience he can learn a great deal about commercial policy, prices, and social pretensions, in short, about political economy. He can also with much less effort and some little satisfaction note names—Gordon, Harvey, Sandeman—and types—champagne, claret, madeira, port, sherry, and a score more—that may cross his vision daily. Mr. Francis clearly knows wines and writes of them with the affection of an antiquary. Had he pruned his vines and now and again aided less sophisticated readers with glosses he would have enabled them to share his affection and at the same time gain a clearer picture of a trade unsurpassed in the variety of its ramifications.

CHARLES F. MULLETT  
University of Missouri—  
Columbia

ARTHUR JOSEPH SLAVIN. *The Precarious Balance: English Government and Society*. (The Borzoi History of England. Volume 3: 1450–1640.) New York: Alfred A. Knopf. 1973. Pp. xiv, 397. \$7.95.

In writing this book, and in editing the series of which it forms a part, Dr. A. J. Slavin has sought to go beyond the conventional framework of political history to the economy and society of the age. In this he has, to a certain extent, succeeded. An introductory chapter entitled, like the book, "The Precarious Balance," describes the social structure of the country from the mid-fourteenth century to the mid-sixteenth. Later chapters explore the passage of England through the upheavals of the Wars of the Roses and the experiments in order culminating in the achievement of the Tudors in establishing a stable, unitary state. With the

coming of the Reformation the book reaches its central theme. Thereafter it deals with the interplay between religion and politics—though when it comes to theology the author sometimes treads water—and then moves from that to analyze the agrarian and industrial developments of the time. The book concludes with a retrospective survey of the whole age.

As one lays this book aside a central question comes to mind. What function, it is reasonable to ask, should a history textbook for the 1970s be expected to serve. Manifestly it should inform, and here Slavin's book is up to the standard of comparable studies. Though there are obscure and elliptical passages, most of the book is clearly written and up to date. Where the author is not himself committed, he synthesizes the contributions of modern scholars, though the limits imposed on the footnotes severely restrict his acknowledgements for information or conclusion. There are signs also of haste: we meet the eminent theologian Bullinger, and "the grey Duchess of Suffolk," while Bucer is spelled in two different ways on succeeding pages. But Bullinger, Bucer, and the Duchess of Suffolk, along with much else, are missing from the index.

There is, however, another important criterion. If a modern textbook is to rise above the level of competence, the young student should emerge with an enhanced awareness of what history is about. He should feel that it is more than a packaged subject (as all too many think it is) and that it owes something of its interest and value to the changing perspectives of historians within the same generation, out of which some of the most exciting and rewarding controversies have emerged.

This, I think, raises a fundamental criticism of the book. Slavin has read widely, but only occasionally is the young historian made aware that there is room for debate and diversity in historical conclusions. This is particularly striking in the chapters on the mid-Tudor period in which Slavin is himself a specialist. Here he puts forward an establishment interpretation, and he thereby misses a valuable opportunity to enliven and illumine his narrative by drawing attention to alternative interpretations as well. I will give one example. In a very odd footnote, on page 105, Slavin gives the name of the article in which a distinguished scholar

put forward his thesis (with which, incidentally, I agree). It is followed by a reference for the article in which the same scholar refuted his critic. But the article in which the critic had challenged the thesis (and without which the second article cannot be understood) is simply not given.

The bland passage on page 107 which begins "Freedom and the concrete liberties of which it consists are everywhere dependent on authority for vindication" (does authority never restrict or destroy liberty rather than vindicate it?) will leave an intelligent student wondering whether everything really was the best in the best of all possible worlds. It is, in this context, interesting to follow through Slavin's references to Thomas More. A reader of this book, unfamiliar with the work of other historians, would not dream that More gave his life because he was more skeptical than is Slavin about the official standpoint. A history of the sixteenth century that cannot deal justly with dissent—in those days by contemporaries, in ours by historians—presents a partial image of a complex and fascinating society.

All this is a pity because there is a good deal of sound reasoning and intelligent commentary in the book. Yet, in the end, a flourishing school of history, like a flourishing liberal society, is dependent upon diversity of opinion. Otherwise, as Erasmus said long ago about society as a whole, and as Aristotle had said long before him, truth will not be heard. No less important, the postulant historian will be studying a subject drained of some of its color and vitality.

JOEL HURSTFIELD

University College London

DAVID CECIL. *The Cecils of Hatfield House: An English Ruling Family*. Boston: Houghton Mifflin Company. 1973. Pp. 320. \$15.00.

Now retired after a distinguished career as professor of English literature at Oxford, Lord David Cecil has turned back to the home of his childhood and to the family whose spirit Hatfield breathes. His own spirit breathes through the pages of his book, still with the curiosity of childhood but now invested with understanding from a lifetime of informed and imaginative scholarship. He begins with an evocative description of the house where, much the

youngest child of the fourth marquis of Salisbury, he rambled alone from rooftop to cellars, a little frightened in one room by sinister-eyed portraits of the Valois rulers of France in the time of the first Cecils, surrounded in another room by walls of well-used books, maps, and pamphlets accumulated by his scholar-statesmen forebears.

The bulk of the book is a chronicle of the inhabitants of Hatfield—from the young Elizabeth in the old palace, the trusted Lord Burleigh to whom she gave the estate, and his crippled younger son, the first earl of Salisbury, who built the great house while he served the old queen and her inglorious successor; through lean generations of increasingly incompetent nonentities and the return to respectability during the reign of George III; the return under the third marquis of Salisbury, Queen Victoria's last and most valued prime minister, to a greatness equal to, though more fragile than, the greatness of the family's founders; and finally into the twentieth century when the prime minister's children lost the ability to place the family's impress upon the country. Until the story reaches the last years of Victoria's prime minister, it does not cut new ground. But throughout it is touched with a descendant's fascination, not so much with what his ancestors did, as with what they were like. Lord David is also sensitive to nuances in the combination of political craft and religious seriousness that run as a common thread through the life stories of the three greatest Cecils.

It is in the last chapter on the third marquis's children, who, as it were, refracted his light, that the book regains the intimacy of its opening description of Hatfield. It is in the author's loving portrait of his mother, a descendant of Lady Palmerston who was brought up in an oppressively evangelical household, that his appreciation of Whig society in transition, which he displayed years ago in his biography of the young Melbourne, comes most delightfully into play.

PETER MARSH

Syracuse University

LAWRENCE STONE. *Family and Fortune: Studies in Aristocratic Finance in the Sixteenth and Seventeenth Centuries*. New York: Oxford Uni-

versity Press. 1973. Pp. xviii, 315, 7 plates. \$12.50.

Eight years ago Professor Stone published his monumental study of the English aristocracy (*The Crisis of the Aristocracy* [1965]). This new book pursues some of the same themes and fills in much detail, but it is very much more than a mere addendum to the larger work and stands on its own feet as a notable study of family history. The format is quite different from that of its predecessor since it consists of five separate studies, each dealing with the fortunes of a great aristocratic house over several generations. All five are peerage families, and all stood in the first rank of their order. Three of them—the Cecils (Robert and his descendants), the Wriothesley earls of Southampton, and the Howard earls of Suffolk—were politically eminent in at least one generation. The other two—the Manners earls of Rutland and the Berkeley lords Berkeley—enjoyed great wealth and prestige but were of little political consequence at any time. The studies vary in length; the Cecils, whose story is carried down to 1733, fill up more than half the book; the Manners' fortunes are traced from the Wars of the Roses to the Restoration. The other three families are more briefly treated, but in two cases (the Wriothesleys and the Howards) we have the satisfaction of following the families from the first elevation to the peerage to their extinction in the male line. It is, of course, the very uneven survival of evidence that dictates the choice of subject and the length of each study.

There can be no doubt that Stone has exhausted all existing materials, and one cannot but regret the great lacunae that make so many important questions unanswerable and flaw the comparability of much data. There is nevertheless enough to make each family history interesting and rewarding in its own right. Collectively they evoke an appropriately Jacobean sense of the mutability of human affairs and the vanity of human ambitions. The two great Cecils were succeeded by a line of non-entities whose follies sadly diminished the family wealth and altogether destroyed its political eminence. The hard-headed founder of the Wriothesleys was followed by two soft-headed earls, but the family ended on a more dignified note with the career of the worthy,

unambitious lord treasurer of the Restoration. The Manners produced no representative of more than average ability (except perhaps the short-lived Earl Edward) in eight generations, but they were shrewd and fortunate enough to survive, even modestly to thrive. The Suffolks were founded by an extraordinarily lucky adventurer whose luck did not quite last out his lifetime. None of his descendants was able to deal with the burden of debt he left behind, and the family sank to squalid obscurity before its extinction in the male line in 1745.

Two contrasting aspects stand out in this collection of family histories. One is the role of blind chance, particularly biological accident. The failure of male heirs meant total extinction, too great fertility a dispersion of family resources. The vagaries of royal favor could carry a family to the heights or leave it becalmed on a sea of frustration. But, on the other hand, once a family had climbed to such heights as these five reached they were in many respects invulnerable to fortune's treacherous arrows. The immense prestige of their position, the fundamental solidity of the social order, and the highly successful device of the strict settlement kept them from falling out of the magic circle. A rich aldermanic marriage, court favor, or the economic flexibility provided by large capital resources generally enabled them to recover from the follies or incompetence of any particular generation. The very mischances that afflicted individual fortunes only serve to highlight the massive stability of the aristocratic order, even in a century of civil war and endemic political disorder.

Probably the most interesting section of the book is the long study of the Cecils, particularly Earl Robert. The evidence for his career is reasonably full and by good luck particularly so for the key years 1608–12 when he was lord treasurer. The divergent sources of his income—privateering, customs farming, salary, the profits of office (particularly wardship), his Spanish pension—are all laid before us, although often in baffling incompleteness. The details of expenditure and of investment, particularly in urban real estate, are a good deal fuller. Stone emphasizes in his study of Robert Cecil's career the growing corruption of the political world. There can be little doubt that contemporaries believed this to be so, and the

resulting breakdown of public confidence in the Crown is a still underestimated factor in the determinative shift of English politics in the 1620s. But one could wish we had a clearer perspective on the Elizabethan past that would enable us to determine how far there was a real growth of corruption and how far the change was one in public opinion. If only we knew much more about the fortunes of the great Elizabethan political adventurers—Hatton, Burghley, and above all, Leicester. But this is to rail against fortune and not against Stone. He has laid out his evidence fully and persuasively, and it must weigh heavily in our judgments on the early Stuart political world.

WALLACE T. MACCAFFREY  
Harvard University

RETHA M. WARNICKE. *William Lambarde: Elizabethan Antiquary, 1536-1601*. [Chichester:] Phillimore. 1973. Pp. xv, 188. \$9.95.

In *The Elizabethan Court of Chancery* (Clarendon Press, 1967) Professor W. J. Jones remarked that "Lambarde is one of those secondary figures whom a full-scale biography might well elevate to the first rank of importance." While doing the research that underlay *The Justices of the Peace in England, 1558-1640* (1969) I reached much the same impression. Clearly Wilbur Dunkel's *William Lambarde, Elizabethan Jurist* (1965) was not such a book. Thus it is a great disappointment that Mrs. Warnicke's *William Lambarde: Elizabethan Antiquary*, although very much better than Dunkel's effort, does not win for Lambarde the recognition that is probably his due. Her research in England was rewarded by the discovery of important and extensive manuscript materials, hitherto largely unknown. A diary, account books, and drafts of Lambarde's treatises are still in the possession of a lineal descendant. Correspondence with one of his closest associates and friends, Sir John Leveson, is preserved in the archives of the Staffordshire County Council. Other records further supplement the better-known manuscripts in the Folger Library. Possibly the assize rolls for the southeastern circuit in the Public Record Office might yield some further evidence, but it is unlikely that Mrs. Warnicke has missed any important documents. She has indeed written

a commendable account of Lambarde's life and shown clearly his close relations with Lords Burghley and Cobham and particularly with Sir Thomas Egerton. As her subtitle indicates, however, she was beguiled by Faith Thompson's phrase, "The Prince of Legal Antiquaries" (the title of chapter 9) in her estimate of her subject. A professional lawyer's training may well be required to make a definitive analysis of Lambarde's legal writings—not attempted by Mrs. Warnicke, nor by anyone else—and so to demonstrate his full stature.

Few doctoral dissertations attain the standard of the best historical work, and a reviewer should show compassion. However there are here rather too many technical flaws. More serious is Mrs. Warnicke's failure to keep abreast of Jones's work. Although she used his London dissertation, she seems to be unacquainted with his book in which he acknowledged the great profit he derived from the Egerton manuscripts he later found in the Huntington Library. Likewise she gives no evidence that she consulted my own study of the justices of the peace in which both Lambarde and Leveson are given considerable attention. Lambarde still awaits the mature study he deserves.

JOHN H. GLEASON  
Pomona College

JOHN MILLER. *Popery and Politics in England, 1660-1688*. New York: Cambridge University Press. 1973. Pp. xiii, 288. \$14.50.

Dr. Miller's concerns are (1) to examine the character of English Catholicism between 1660 and 1688 and (2) to explain how English Protestants' fears of popery affected politics from the Restoration to the Glorious Revolution. Drawing upon his own dissertation researches (with some results presented in tabular detail in appendixes) and upon the work of other students of recusancy, he identifies two fairly distinct Catholic communities—one of the shires, which was seigneurial and survivalist, the other of the metropolis, which comprised artisans and shopkeepers as well as court elements. This court-country distinction is central to his second theme, for he contends that it was court Catholicism and, above all, the knowledge of the conversion of the duke of York that reawakened traditional fears of

popery (usually perceived as a complex of Catholicism and absolutism) in the early 1670s. In tracing the antipopery theme through Charles II's and James II's reigns Dr. Miller also offers an explanation, framed primarily in biographical terms, of the divergent fates of the royal brothers. On the one hand Charles remained at least nominally Protestant until his death, and after the first shock of the Popish Plot began to fade he shrewdly, in order to ward off the pressure for exclusion, exploited fears that 1642 had come again. On the other hand James openly proclaimed his Catholicism at his accession and rashly proceeded to turn what had been a possible threat of popery into a full-scale Catholicizing policy. James, indeed, comes off very badly in this account as neither genuinely committed to religious toleration nor systematically working to erect an absolute monarchy. His Catholicizing measures are adjudged ill-conceived and impractical (e.g., the missionary effort that is here well delineated), and almost as soon as he unexpectedly fathered a Catholic heir he was overthrown.

Dr. Miller, then, is intent on refuting what he calls "the Whig interpretation" of James's reign, which in his view uncritically embodied the anti-Catholic biases of that king's own subjects. He also gives short shrift to those historians who have accepted James's sometimes-professed opposition to religious persecution. Furthermore, he questions—without considering the full range of the evidence offered—newer studies that have suggested that, had it not been for William of Orange, James could have maintained himself against his domestic opponents and even, given three score and ten, passed on his crown to his son. Professor J. R. Jones "take[s] James's policies seriously" (*The Revolution of 1688 in England* [1972], p. 11) and the late J. R. Western credited the king with establishing by mid-1688 "something like an equilibrium" so that "the nation had not the means without foreign aid to bring [him] to book" (*Monarchy and Revolution* [1972], p. 238). And if Dr. Miller's very useful study has any serious flaw it is his precipitate dismissal of this revisionist perspective.

HENRY HORWITZ  
University of Iowa

ALISON GILBERT OLSON. *Anglo-American Politics 1660-1775: The Relationship between Parties*

*in England and Colonial America*. New York: Oxford University Press. 1973. Pp. xiii, 192. \$7.50.

This brief book is, in the author's words, a study of "the way in which the first British Empire affected local political divisions in England and America, and, in turn, the way in which the emergent parties helped to bring the empire together at first and to tear it apart in the end." Although, as Olson admits, her work is no more than a "suggestive essay," it is filled with useful insights into the nature of the imperial political relationship.

Despite its dual title the book is firmly based in England. Olson traces the ebb and flow of English colonial policy over the years, not within the familiar context of "benign neglect" versus involvement, but rather in terms of the ways in which English politicians viewed colonial factions. She argues that the shifts in English policy were based on ministers' and opposition groups' respective assessments of the benefits they might reap from strengthening or weakening colonial opposition movements. Accordingly, for her the question of whether or not colonial politicians could find English allies is of crucial importance, and so she places great stress on such developments as the collapse of the first Tory party, which deprived dissenting Americans of an avenue for circumventing the ruling Whigs. She contends further that as a result of the excise crisis in 1733, American politicians turned increasingly to their constituencies for support instead of continuing to look for aid to their more traditional transatlantic connections. The ultimate consequence of this development, Olson declares, was independence.

In the light of the critical role Olson assigns to the excise crisis (referring to it twice as "a significant turning-point in Anglo-colonial history"), it is unfortunate that she does not deal with it in a unified or extended fashion. She discusses the affair only on three scattered pages and leaves the reader somewhat mystified as to why its impact was so marked. Further, the evidence she then presents in support of her contention that the English opposition after 1733 systematically supported harsh colonial legislation in hopes of embarrassing the ministry and arousing provincial discontent is less than convincing. Nevertheless, her treatment of the general subject of Anglo-American politics

works well until she reaches the immediate pre-Revolutionary period. Then it seems to fall apart, basically because the British-centered approach she has used throughout the book no longer applies to colonial circumstances. After 1765, and especially after 1773, American politics were wrenched out of their imperial context, and a model designed to fit that context is simply not capable of dealing adequately with the rise of extralegal associations, committees, and congresses.

One final point: this otherwise impeccably scholarly book surprisingly lacks a bibliography.

MARY BETH NORTON  
Cornell University

ANNE WHITEMAN *et al.*, editors. *Statesmen, Scholars and Merchants: Essays in Eighteenth-Century History Presented to Dame Lucy Sutherland*. New York: Oxford University Press. 1973. Pp. xviii, 375. \$19.25.

Dame Lucy Sutherland, teacher, scholar, administrator, and public servant, is a distinguished historian of wide interests, and these are reflected in the diversity of studies presented to her by friends on both sides of the Atlantic. A brief eulogy by J. S. Bromley prefaces the volume. Immediately preceding the index is a list of Dame Lucy's writings, 1931-73. A review cannot do justice to all offerings but can, perhaps, indicate the exceptional quality of this *Festschrift*.

Three excellent studies concentrate on the critical period in Anglo-American relations. Paul Langford, in "The Rockingham Whigs and America, 1767-1773," scrutinizes the role of a well-organized coterie reputed to favor the colonies. During their first term in office they had repealed the Stamp Act but were, of course, equally responsible for the contents and passage of the Declaratory Act. During the years between repeal and the Boston Tea Party the record does not support the reputation often accorded them. Far from adhering to "the principles of Repeal" the Rockingham Whigs offered no contrary motion to the enactment of the Townshend duties, nor did they endorse the earl of Chatham's distinction between internal and external taxation. William Dowdeswell and Sir George Saville, prominent in the party, agreed that strong measures were necessary when, for example, the New York Assembly revolted against the Mutiny Act.

Even the resolutions written by Edmund Burke after the Boston Massacre were, Horace Walpole thought, "strangely refined and obscure." *Thoughts on the Cause of the Present Discontents* ignored the American problem. The colonies were "wild and absurd" and "distempered and delirious": the Rockinghamites deplored their violence. The chief service of these Whigs was to oppose outright war and, doubtful of its outcome, to be prepared, even without a victory, for imperial principles and arms, for concessions to avoid and then to end it.

Ian Christie in "The Historians' Quest for the American Revolution" notices that nearly two centuries later there is no consensus about the causes or even the nature of the Revolution. There is today a diminishing interest in economic explanations and a greater concentration upon the events as "a problem of government," on internal conflicts in the colonies between ruling elites and their would-be successors, on a listing of what angered Americans, and on those ideas brought forward to support their cause. Topics that, he suggests, may be further explored are prompted by those questions raised by imperialists about commerce, currency, and frontier defense, preoccupying British ministers *before* and American statesmen *after* the wars, and by yet more consideration of that decline in the Anglo-American community pointed out by Thomas Barrow, Michael Kammen, and John Shy. Jacob M. Price adds to his studies of the Chesapeake trade in "Joshua Johnson in London, 1771-1775" and illuminates the problems of a Maryland merchant during years complicated as much by depression and inflation as by the mounting political crisis.

"Changing Attitudes towards Government," by Norman Baker, makes judicious commentary, not upon another "revolution," but upon less tangible shifts in attitudes, preparing the way for more dramatic changes of practice affecting those in public service. John B. Owen magisterially reconsiders George II and concludes that, though his grandson has been thought the more effective monarch, there is not really much to choose from between their records. Both were determined to make their voices heard about policy and to retain a firm hand on a great deal of government patronage. Both were obliged to find a commoner who could link closet and Lower House. George II enjoyed less room for maneuver than his successor, but

made royal authority felt in a number of crises. Robert Walpole declared his master required "tenderness and management"; Lord Hardwicke opined that ministers should be prepared prudently to submit to "minus malum." These, Owen concludes, are scarcely the words of politicians who dictated to their sovereigns. About George III, John Brooke explains, posterity has been deceived by Horace Walpole. The famous *Memoirs* are largely responsible for long-lived myths about the Princess Dowager, Lord Bute, and George. Walpole often relayed only malicious gossip: his work reveals, all too often, more of himself than of the monarch.

Statisticians will find Anne Whiteman's probing of population estimates illuminating. She decides, on excellent evidence, that writers have relied chiefly on reports on the Bishops' Survey of 1676 and that no census was taken in 1688, 1689, 1690, or 1693, in spite of the inexplicable inclusion of a paper derived from the survey in *The Calendar of State Papers Domestic* for 1693. J. S. Bromley examines legal and diplomatic problems in "The Jacobite Privateers in the Nine Years' War." Even before the diplomatic revolution of 1756 the old alliance between Britain and Austria was weakened, as P. G. M. Dickson shows in "English Commercial Negotiations with Austria, 1737-1752." An interesting account of a London merchant, "James Sharp, Common Councillor," is based on an unusually large collection of family papers. The Sharps, among whom Granville the reformer is best known, were a closely knit family sharing interests in reform, canals, communications, and music.

The transition from church-oriented factions to modern secular parties is illustrated, Alison Olson thinks, by the disputes among Anglicans in New York in the early eighteenth century, and she traces the history of differences between Governor Hunter and William Vesey of Trinity. An extraordinarily interesting cast of characters, Whig and Tory, orthodox and socinian, appears in "Fathers and Heretics in Eighteenth-Century Leicester," by R. W. Greaves. In the neighborhood of St. Martin's Church and Wigston Hospital lived, for example, Thomas Carte, historian, nonjuror, and Jacobite; Samuel Clark, Boyle lecturer and socinian; and his disciple John Jackson, contributor to the *Old Whig* and author of other polemical pieces.

G. D. Gurney makes use of many unfamiliar sources. "Fresh Light on the Character of the Nawab of Arcott" shows "an enigmatic and perplexed" Indian confronted by the changes wrought by the East India Company and determined to maintain as much as possible of his own tradition. Warren Hastings was, P. J. Marshall thinks, a student and patron of Oriental studies both for the assistance these might afford the English in governing India and for the advantages of understanding all parts of the world. E. P. Courtney analyzes opinions common to both Burke and the philosophers. With them he also owed much to the men of the early English Enlightenment—a subject too much neglected by students of the Irishman's intellectual heritage.

Perhaps the most charming and perceptive contribution to this volume is Thomas Copeland's "Johnson and Burke." "That fellow," the doctor remarked, "calls forth all my powers." Friendship and rivalry were maintained over twenty-six years. Yet Copeland, like Sir Joshua Reynolds, detects in Johnson a feeling of inferiority to Burke and to support this adds the evidence of a reported nightmare. Without determining the matter of pre-eminence of either as conversationalists, Copeland sees in Johnson's advice to Burke (not to commit civil suicide by retirement) a hint of regret over his own questionable withdrawal after receipt of the pension and over the thought of possible careers unessayed. Johnson went out of his way to pay excessive praise to Burke, prompted perhaps by a determination to do justice to a secretly envied ability. Copeland gives many variants, which must be read to be appreciated, of the story based on the presumption that even in a shower of rain, a stable, a rain of bullets, and so on, you would in Burke's company instantly recognize his extraordinary quality. But no student of the period can afford to neglect this admirable and deserved tribute to Dame Lucy.

CAROLINE ROEBINS  
Rosemont, Pennsylvania

W. S. LEWIS *et al.*, editors. *Horace Walpole's Correspondence with John Chute, Richard Bentley, the Earl of Strafford, Sir William Hamilton, the Earl and Countess Harcourt, George Hardinge; Horace Walpole's Corre-*

*spondence with the Walpole Family*. (The Yale Edition of Horace Walpole's Correspondence, volumes 35 and 36.) New Haven: Yale University Press. 1973. Pp. xxxii, 649; xxxix, 336. \$20.00 each.

In this day of inflated prices and ephemeral values the enduring quality of the Yale edition of *Horace Walpole's Correspondence* is of the first consequence. The work is a monument not only to the eighteenth century but to the twentieth as well, representing, as it does, a rare combination of the arts and skills of editor, collector, and printer.

These two most recently published volumes present, but in very different ways, distinguishing facets of eighteenth-century life. Whether unconsciously reflecting his own age or commenting on it with personal concern or assumed, Olympian detachment, Walpole, with the help of his correspondents, recreates a period as no secondary account has ever succeeded in doing.

Volume 35 particularizes a subject frequently mentioned in other correspondence: the inception and development of plans for Strawberry Hill. John Chute and Richard Bentley made a special contribution to the remodeling of the house in the Gothic style, not only by their encouragement and suggestions but also with designs for the actual construction and decoration of this famous showpiece. Other letters give evidence that Walpole was not alone in spending time and fortune on such an enterprise. To acquire, build, rebuild, furnish, landscape, and then to entertain and visit in return—these were among the ways in which the leisure class loved to spend its time.

This way of life was made possible by a great servant class, largely taken for granted. Yet before the Industrial Revolution, mutual responsibility tended to characterize the relation between master and servant. One of several illustrations in these pages occurs in Walpole's lament that he could not, or would not, dismiss a gardener who was "incredibly ignorant and a mule" (vol. 35, p. 479).

The other volume consists of the correspondence of Walpole with twenty-six members of his extended family to which an end paper containing the family tree furnishes a welcome guide. The chronological order of the letters, exceptional where several correspondents are concerned, serves to give continuity to the story

of Walpole's relations with other members of his family. They begin with childhood letters to his "dearest dear Mama" (vol. 36, p. 3) and conclude with one last letter to his favorite niece, who was then the duchess of Gloucester.

A unique feature of these letters is the reason for their having been written. Whereas Walpole normally chose his correspondents for the possibility of mutual entertainment, this correspondence was largely the result of a sense of the responsibility, often affectionate, that was typical of what an eighteenth-century gentleman might have felt for his family. Many of these letters represent genuine pleasure, others real distress on Walpole's part.

Supplemented by the appendixes "Lord Orford's Illness" and the "Case of the Entail," several of the letters concern Walpole's unhappy relations with his nephew, on the one hand, and his uncle, "Old Horace," on the other. During the former's periods of insanity Walpole felt obliged to assume the management of his affairs. Walpole's distaste for his uncle was the result of his belief that he had planned to deprive him and others of their intended inheritances. Both problems are related to Walpole's characteristically eighteenth-century disappointment at the failure of the direct line of descent from his father and the decline of the family estate at Houghton.

All that is lacking to give these volumes their rightful place alongside others in the series as aids to the study of the eighteenth century is the index, which presumably will follow in due course.

DORA MAE CLARK  
Wilson College

P. LANGFORD. *The First Rockingham Administration, 1765-1766*. (Oxford Historical Monographs.) New York: Oxford University Press. 1973. Pp. 318. \$14.50.

This model monograph, the first full account of Lord Rockingham's ministry of 1765-66, makes a contribution of the first magnitude to the political and constitutional history of the early reign of George III. Research has been exhaustive, and narrative and conclusions are drawn from every relevant major manuscript and transcript collection in Britain and the United States. The comprehensive bibliography



of published sources, pamphlets, and secondary works is fully exploited. Langford gives us, indeed, a new "standard" work; and no scholar, Whig or Tory, engaged with British politics from 1760 to the Age of Reform can in the future evade this masterful treatment.

The first Rockingham ministry, lasting barely a year, determined both the character and the policies of the Whig opposition during the age of the American Revolution. It also bequeathed to their nineteenth-century descendants a mythology concerning origins and ideology articulated principally by Edmund Burke. The actions of this brief ministry thus have a meaning out of all proportion to the length of time spent in office.

Viewing the myth as already essentially dismantled by Sir Lewis Namier and his successors, Langford concentrates on finer points of detail. The familiar problems of the king's relations with his ministers and the Stamp Act crisis receive much attention, to be sure; but they are set in a rare perspective: two entries in a list that also includes foreign policy issues; the debate over free ports in the West Indies; the meaning of the king's friends for the emergence of "party"; the impact of Wilkes and radicalism on the Old Whigs; regional politics, turning especially upon the cider excise controversy; the constitutional role of the Lords in the repeal of the Stamp Act; the influence of the new industrialism in the crisis; and the interplay of personalities.

Spurning Burke's apologia, offered in his *A Short Account of a Late Short Administration*, as "a flawless piece of political propaganda" hopelessly lacking in veracity (p. 266), Langford shows that Old Whig measures were negative in nature, mere concessions to outside forces. Even their successes were fragile and transient, and gross political misconceptions and blunders were more common. The ministry fell, it is asserted, because in political terms it deserved to fall, and its constituents and followers remained so long in the wilderness because they were more comfortable in opposition than in office. To rationalize their predicament, Rockinghamites developed the themes of secret corruption, double cabinets, the devil Bute, a perfidious monarch, and the "garrison"—the word is Burke's—of "king's men" controlling access to the sovereign and working to subvert a constitution divinely given by earlier Whigs.

Self-styled defenders of "old" Whig values, Rockingham and his friends are shown here to have founded, unconsciously enough, a new party ironically resembling the "Country" Tories under George II and perpetuating an ideological myth that became in due course a resource upon which the party of Grey and Russell might draw.

Langford's argument is powerful and is massively supported. It compels acceptance.

CHARLES R. RITCHESON

*University of Southern California*

RICHARD GLOVER. *Britain at Bay: Defence against Bonaparte, 1803-14*. (Historical Problems: Studies and Documents, 20.) New York: Barnes and Noble. 1973. Pp. 235. \$10.50.

Recently A. J. P. Taylor, in one of his sillier moods, proposed that military historians would soon be as obsolete as handloom weavers. While war in the twentieth century is more terrible and senseless than ever, it seems likely that we shall have new ones to study for some time to come. Whether or not war is now obsolete the study of war in the past remains a vital part of the historians business. Richard Glover, whose 1957 essay "War and Civilian Historians" provides an excellent defense of the military historians craft, has given us a first-rate example of its practice in *Britain at Bay: Defence against Bonaparte, 1803-1814*. This well-written monograph reminds us that the first responsibility of the British ministries facing Napoleon was not to defeat him but to defend the home island against the most serious and sustained threat of invasion in more than a century. With the staff officer's practiced eye Glover economically lays out Britain's strategic and logistical problems and describes the military and naval organization that had to cope with them. He reconstructs Napoleon's plans for invasion and the British plans for defense. Glover has been over some of this ground before in his *Peninsular Preparation . . . 1795-1809*. He has not been satisfied, however, merely to summarize recent work, though he does that admirably. Using War Office and Home Office papers he has found fresh things of independent value to say about the British struggle to fortify the island and to recruit men. (American historians should be interested, for example, in his defense of the scrupulous legality of Admiralty impress-

ment of "American" seamen.) Part of the manpower problem was political—to find a policy that would provide the men without wrecking the constitution. On the topic of politicians as war ministers he has much to say of interest, largely exculpatory. He defends the hapless Addington, the duke of York, Castlereagh, and the young Palmerston at the outset of his career. Like Fortescue, to whom he defers, Glover is critical of Pitt and even finds a good word for purchased commissions. Nevertheless, this account demonstrates again that the time to replace Fortescue's monumental work is now long past due. In the meantime the present volume, with its long appendix of illustrative documents, will be of considerable use to scholars and students in the field.

S. J. STEARNS  
Richmond College,  
City University of New York

F. B. SMITH. *Radical Artisan: William James Linton, 1812-97*. [Totowa, N.J.:] Rowman and Littlefield. 1973. Pp. x, 254. \$14.50.

Linton's story, writes Dr. Smith, "seems less than its parts." This is true in part because Linton did not gain real eminence at his various endeavors as wood engraver, republican, poet, editor, Chartist, biographer, and journalist. Moreover, he is diminished by Dr. Smith's presentation of him as an artisan without dignity, a journalist without an audience, and an entrepreneur dependent upon charity until he moved to the United States in 1866 and became an expatriot yearning for home.

But Linton is of interest because of his uneasy class politics, for he inhabited the under-explored borderland between working-class radicalism and middle-class liberalism. He was liberal in his analysis, radical in his measures, working class in his sympathies, middle class in his aspirations. For more than two decades his political activity and concerns revolved around the murky and sterile world of the continental exiles in London. Mazzini was his great hero, and Linton's one brief moment of political prominence came in 1844 when he took a leading part in the exposure of the opening of the great man's letters. Mazzini saw enough of Linton to weary of him: "He means good."

The end of this remarkably full and very welcome biography leaves one wishing for a

more integrated analysis of the style, class content, and political thrust of Linton's journalism, engraving, and other artistic endeavors. The details swamp any clear sense of Linton's achievement. Part of the difficulty comes from Dr. Smith's choice of a chronological organization that does not leave him the scope to integrate Linton's many pursuits.

Another problem arises because the author seems to have little respect for his subject. Linton was a social climber who married above himself three times and who was frequently rebuffed in his attempts to enter attractive cultural circles. But he was one of many Victorians of talent who suffered because the class system limited their opportunities. Linton's attempts to fulfill himself deserve sympathetic consideration. And whatever his faults, he did not compromise his politics or trim his religious doubts to gain acceptance.

THOMAS MILTON KEMNITZ  
University of New Hampshire

GEORGIANA BLAKISTON. *Lord William Russell and His Wife, 1815-1846*. [Wilmington, Del.:] Scholarly Resources. 1973. Pp. xvii, 566. \$17.50.

A great-granddaughter of Lord William and Lady Russell presents an impressive collection of private letters of the Russell family and their numerous connections. Only a few touch on the political and economic problems of the period, and the value of her work lies primarily in the area of social history.

The merits of the British public school system are occasionally discussed, and some vivid facts regarding army life during this period of "sempiternal peace," as Hastings Russell, eldest son of William, called it, can be gleaned from their letters while in service.

Students of medical history might ponder the efficacy of such remedies as Seidlitz powders, riga balsam, Carlsbad waters, and homeopathic medicine for ailments diagnosed as torpid liver, ague, and (a recent Russian import) the gripe. Among other facts, we learn that the life expectancy of an adult was then reckoned at fifty-seven years.

But this is primarily the story, often curious and sometimes pathetic, of a needy younger son and his peripatetic wife, who by necessity spent most of her youth abroad, continuing as an expatriate by choice later on.

Lord William Russell emerges as an individual who never quite found himself. His father's money and connections brought him a parliamentary seat and an army commission. Later his brother, Lord John Russell, secured several diplomatic posts for him abroad. Only in the army did he make a mark, and that not a heavy one. His parliamentary constituents finally rejected him for chronic absenteeism. The high point of his diplomatic career was a liaison in Baden Baden that further complicated his already disorganized marriage.

His wife, the virtuous Elizabeth Anne, was a stronger personality than William, and she was a devoted mother. But there was a certain callousness and hauteur about her, especially in later years, that might repel the reader. Her eldest son, Hastings, and his uncle, Lord John, are undoubtedly the most attractive characters in the story.

These letters provide intimate insights into the way of life of the nineteenth-century aristocrats, who regarded individuals like Lords Grey, Althorp, and Brougham as "gentlemen," but denied that status to others, such as Henry Goulburn and Sir Robert Peel. On the surface theirs seems a pleasurable life, even magnificent. Yet in the final analysis many of them apparently fought a continuing battle against ennui and desperately groped from some meaningful occupation amid circumstances that rendered their existence almost otiose.

WILBUR DEVEREUX JONES  
University of Georgia

DEREK FRASER. *The Evolution of the British Welfare State: A History of Social Policy since the Industrial Revolution*. New York: Barnes and Noble, 1973. Pp. xviii, 299. \$12.50.

The index of this book lists sixty-one acts of Parliament that, from 1833 to 1948, gave the government greater powers to care for its citizens. The reasons for this evolution lie, according to Fraser, in "the practical, pragmatic, unplanned, *ad hoc* response of the state" to the problems of an industrial society (p. 108). In such responses rather than in any Whiggish "grand scheme of progress" lie the origins of the welfare state (p. 1). The Whigs indeed were no more important than the Tories. Fraser argues that "social policy cut right across normal party lines" (p. 110).

An analysis of these sixty-one acts provides some evidence for Fraser's assertions. Of the sixty-one acts, thirty-three were passed by Whig, Liberal, or Labour ministries, twenty-one by Tory, Unionist, or Conservative, and seven by coalition governments. But a further analysis shows that of the Tory's twenty-one acts, three in the 1840s (two on lunacy and one on mines) were private bills of Lord Ashley and not bills generated by Peel's government. Indeed Peel's government initiated only one new department to help the working classes, an office to regulate the payment of wages to London's coal-whippers, while the Whigs from 1832 to 1841 and from 1846 to 1854 created more than ten such departments, including an education department not listed in Fraser's index because it was created by an order in council. Furthermore many of the Tory acts, like the Factory Act of 1874 and the Public Health Act of 1875, were also largely consolidating and amending acts, acts emerging from bureaucratic processes. Tory innovations in the construction of a welfare state thus rest largely on the housing measures of Disraeli, Salisbury, and Chamberlain and the Education Act of 1902. Fraser himself admits that the Conservative legislation in the decades before and after World War I did less for the poor than did the legislation of the Liberals from 1908 to 1914 and Labour from 1945 to 1950. He even sees the political reasons for it in the Liberals' fear of losing the worker's vote and in the coming to power of Labour.

The Conservatives, of course, also feared losing the workers' vote, but they feared more the alienation of that proud array of vested interests that ranged from Anglican bishops and railway magnates to steel barons and Harley Street physicians. From 1839, when the bishops nearly killed the Whig's education department, and 1848, when George Hudson and his fellow Tories killed the railway commissioner's right to audit railway companies, until the late 1940s when steel barons and Harley Street physicians fought many of Labour's reforms, these powerful constituencies have kept the Conservatives from making bold innovations. The Liberals, on the other hand, though not unchecked by similar constituencies, have felt more acutely the need of placating the working classes. Thus, in the evolution of the welfare state, the really significant breakthroughs—the Education Or-

ders and Health Acts of the 1840s, the old-age pensions and national insurance of the early 1900s, and the National Health Service and National Assistance Act of the late 1940s—have largely come from the left side of the Commons. Class politics as well as *ad hoc* responses help explain the growth of the welfare state. So do “mentalities,” though these are harder to delineate. But it is perhaps not accidental that one of the most innovative of Tory social reforms, the Education Act of 1902, owed much to the ideas of the Fabian Socialist Sidney Webb; nor is it perhaps accidental that no Tory from Pitt to Peel could construct a poor law as bureaucratically efficient as the New Poor Law of 1834, even though such a law served the self-interest of Tory landowners, most of whom supported it. Rationalist planners of the Benthamite persuasion constructed it. A century later another rationalist planner served Tory interests by helping construct the Education Act of 1902. Sidney Webb's father had raised his son on the ideas of John Stuart Mill. There are “mentalities” as well as “constituencies” in the complex forces leading to a welfare state. It is not all *ad hoc* responses.

Fraser, in fact, is fully aware of these “constituencies” and “mentalities” though in an implicit rather than an explicit way. Had he been more explicit about them he would have made his vigorous, lively, always clear and useful account of how the welfare state came about into one that also would have broken new ground in analyzing why it came about.

DAVID ROBERTS  
Dartmouth College

LEE HOLCOMBE. *Victorian Ladies at Work: Middle-Class Working Women in England and Wales, 1850-1914*. [Hamden, Conn.:] Archon Books. 1973. Pp. x, 253. \$12.00.

This is an efficient, tidy book. It is written with clarity, concreteness, and economy—all a welcome change from the learned shroud of abstract jargon in which some authors delight to bury subjects of this kind with full academic honors. After a chapter on the women's movement and working ladies and another on women's education in Victorian England, Dr. Holcombe examines five fields of employment for middle-class women: teaching, nursing, shops, offices, and the civil service. The author

has searched a good many promising corners in the vast lumber room of Victorian printed sources, particularly the *Fortnightly Review* and *Parliamentary Papers*, and she has had her reward. The final chapter summarizes the main points made in the book and offers some observations on the results of the Victorian women's movement. A statistical appendix, founded on census figures, illustrates both their richness and the maddening difficulties of using them.

There are surprising omissions from the list of occupations discussed, perhaps arising from the very rigid plan on which Dr. Holcombe has constructed her book. Why so little mention of women doctors? They were never numerous, but they represented a very important aspect of the whole movement for women's rights, particularly to equality of opportunity. Why nothing about women in the arts, especially as novelists? What about women in business for themselves, particularly toward the end of the century? And surely there should have been some discussion of prostitution. Wasn't it sometimes a profitable sideline, in a more or less genteel way, for the underpaid dressmaker or shop assistant? Arthur Munby seems to have thought so, and he studied women's occupations very thoroughly.

A discussion of the more general aspects of the subject, particularly the reaction of the men, would have been welcome. *Punch*, faithful mirror of the middle-class masculine mind, is consistently and almost hysterically catty about the new woman from the sixties onward, suggesting rising masculine panic at the menace of the advancing female hordes. Doctors, in the contorted maneuvers to keep women out, displayed a malicious deviousness that had a curiously female quality about it. And, on that subject, it would be worth examining why the women, in their assault on the professions, concentrated so heavily on Physic, rather than Law or Divinity. Finally, in considering the results of rising employment of middle-class women in Victorian England, surely there is more to be said than Dr. Holcombe has given us in her final chapter.

Within the limits she has set herself, Dr. Holcombe has given us a valuable book. Let us hope she will see fit, in future, to go further and wider.

W. J. READER  
London, England

MAXWELL PHILIP SCHOENFELD. *The War Ministry of Winston Churchill*. Ames: Iowa State University Press. 1972. Pp. xix, 283. \$9.95.

One must give Maxwell Philip Schoenfeld higher marks for his audacity than for his scholarship. Firm in his conviction that "the record of what happened is now sufficiently well established that it is not likely to undergo major revision," he has essayed an evaluation of Churchill's war ministry (1940-45) without waiting to consult official documents at the Public Record Office. Nor, for that matter, has he delayed to discover what revelations—if any—will be made in the appropriate volume of the authorized Churchill biography. Instead, he has chosen to rely exclusively on the standard published works, from which he has culled a hodgepodge of anecdotes and historical judgments. If nothing else this effort testifies to the peril of writing modern British political history without access to public and private archives, and without reference to the contemporary press. Moreover, it points to the futility of working only with printed evidence in a field where major works, based on either personal observation (like W. P. Crozier's interviews) or original archival research (like A. J. P. Taylor's monumental life of Lord Beaverbrook), continue to appear.

To all intents and purposes Schoenfeld provides an uncritical reworking of the account in Churchill's own *History of the Second World War* (1948-53), embellished with panegyrics from an array of more recent memoirs and scholarly investigations, not always clearly differentiated. There are some curious omissions: Churchill himself alludes, in one quoted passage, to a conversation with Lord Moran, his physician, but the Moran diaries (1966) are inexplicably ignored. Schoenfeld has, however, quarried the published diaries of Lord Alanbrooke (1957, 1959) and Harold Nicolson (1967), whom he absurdly celebrates as "Churchill's Parliamentary Boswell." Given the date of his preface—December 1970—one is presumably expected to excuse him for failing to cite Lord Butler's valuable autobiography, which appeared the following year, but he ought to have seen the transcripts of Butler's BBC interviews, which had run in the *Listener*. What is completely inexcusable is that Butler receives no mention in the text, although there are gratui-

tous references to Adlai Stevenson and Abraham Lincoln.

Misleadingly titled, this book is at most a study of the wartime Churchill and those particular areas of strategy and administration in which he took a keen interest. His colleagues are rarely allowed to intrude into the limelight, and his critics are summarily dismissed. Disclaiming "historical dispassion," the author professes to have given us a portrait of his subject, "warts and all." But the effect is never more unflattering than five-o'clock shadow. "Giant among men was Churchill, and gigantic was his accomplishment," "a statesman of unparalleled experience and vast historical knowledge," "the most humane of statesmen"—surely the case could be put with greater reason and less bombast. There was enough that was genuinely heroic about Churchill without exaggerating his youthful valor in India, the depth of his family commitment to Tory democracy, or his affinity with de Gaulle. Or perhaps, as Hector Hushabye cynically proclaimed to the guests at Heartbreak House, "it is the imaginary hero that supplants us all in the long run."

STEPHEN E. KOSS

Columbia University

PATRICK BUCKLAND. *Irish Unionism*. Volume 2, *Ulster Unionism and the Origins of Northern Ireland, 1886-1922*. New York: Barnes and Noble. 1973. Pp. xxxvi, 207. \$11.50.

IAN BUDGE and CORNELIUS O'LEARY. *Belfast: Approach to Crisis. A Study of Belfast Politics, 1613-1970*. [New York:] St. Martin's Press. 1973. Pp. xxi, 396. \$10.95.

While these two studies share a timely concern with the origins of the Northern Ireland crisis, they are not of equal merit. Patrick Buckland's latest work is disappointing, especially in contrast to his earlier treatment of southern Irish Unionism. Though the focus of both works is essentially political, the first is wide ranging in its use of sources, rich in textual detail, and provocative in interpretation. The sequel is much briefer, sparsely documented, and often equivocal or superficial in its judgments. To be fair, Buckland promises only a "non-specialist history of Ulster Unionism" to 1922. But given the gravity of the subject, its intricacy, and the wealth of available source material, the brevity

and lack of searching analysis of this volume are regrettable.

Insofar as Buckland advances a thesis, it is the hardly novel one that Ulster Unionism was neither monolithic nor synonymous with Orangeism. He notes that the revival of Orangeism in the 1880s in response to Parnellite nationalism could not harness Unionist forces in Ulster. The Orange Order's "essentially working-class character and its sectarian exuberance" repelled the often tolerant Protestants of the middle and upper classes. As evidence of the divisive effects of class and economic interests on northern Unionism, Buckland instances the Ulster Tenants' Defence Association of the 1890s, the proletarian and extremely sectarian Independent Orange Order, founded in 1903, and the Protestant labor challenge in Belfast after 1918. The author also points out that not the Orange lodges but rather the political clubs promoted by the Ulster Unionist Council furnished the basis for grass-roots resistance to Home Rule in 1911-14.

What Buckland fails to emphasize is the vital function increasingly performed by the Orange lodges after 1885: their role as the great emollient for class and economic antagonisms among Ulster Protestants. It is strange that Buckland finds no place in his story for the historic post-1885 transformation of the Orange Order from an overwhelmingly working-class institution into a mass body comprising the great majority of middle- and upper-class Protestants. Buckland also fails to note how the abolition of proportional representation contributed to Ulster labor's postwar weakness as an independent political force, despite the Unionist party's failure to develop either a democratic system of working-class participation or a social program with broad proletarian appeal.

The subjects of class, religion, and party competition are treated more authoritatively in the ambitious study of Belfast politics by the political scientist Ian Budge and the historian Cornelius O'Leary. The marriage of disciplines is not complete and the coverage less than the subtitle suggests. The authors mercilessly compress their account of the period 1613-1800 into fourteen painful pages and do not deal directly with the post-1967 Northern Ireland crisis, though they have much to say about its advent, partly by extrapolating many of their conclu-

sions from the municipal to the provincial level. They devote almost half their book to an exhaustive, rigorous analysis of a 1966 political survey of Belfast residents, municipal councilors, and news correspondents.

The significance of the authors' findings, often unsurprising when considered individually, derives mainly from the generally successful attempt to integrate them into a coherent explanation of the onset of crisis in 1968. Nineteenth-century Belfast, the authors show, saw sectarianism and political intransigence become salient features of municipal life. Religious riots were the most visible sign of sectarianism; its basic causes were the Orange Order's growth (partly in response to heavy Catholic immigration), the inflammatory preaching of certain Protestant divines, apartheid in education, and Protestant alarm over manifestations of Irish nationalism. Challenging a traditional view, the authors reject Catholic-Protestant competition for jobs as a major cause of either religious riots or sectarianism in general. Religious allegiance came to determine political identification, partly because the intensity of denominational loyalty severely depressed class consciousness, especially among workers, and partly because Belfast politicians found in the appeal to religion the easiest path to office. By playing "the Orange card" continually, the Conservatives (from 1886, the Unionists) maintained an unbroken one-party dominance of municipal office, a hegemony resting not on the gerrymander, as in Londonderry, but on majority support. Yet after 1870 Conservative or Unionist control was usually characterized by political immobility arising from intraparty factionalism, the social elitism of party leaders, and Nationalist weakness and ideological rigidity. But by the early 1960s Nationalist leaders, less elitist and thus more responsive to changes in attitude among Catholics, shifted their emphasis to questions of social welfare and discrimination. Rank-and-file Protestants had also changed their attitudes by 1966, a majority now favoring rapprochement, either "spontaneously" or prompted by Prime Minister O'Neill's initiatives. But the opportunity was missed. Unionist councilors underestimated popular Protestant support for rapprochement and were themselves inconsistent in action while agreeing in principle. Their vacillation or unwillingness

to act boldly convinced Catholics, now expecting change, that immobility still reigned supreme. Street demonstrations followed, reviving Unionist intransigence. The authors conclude that "difficulties in communication rather than an absence of majority support for reform seem to have been at the heart of the crisis."

Certain unfortunate gaps in analysis, however, weaken this intriguing study, notably the authors' neglect of the political implications of economic and social developments after 1920 and of the causes and timing of changes in popular political attitudes prior to 1966. Moreover, Budge and O'Leary do not offer wholly convincing evidence that the Unionist reform impulse in Belfast was sufficiently strong to avert a crisis had the glaring deficiencies in communication not existed. The issues they use to test support for conciliation in 1966 are quite narrow. Total Unionist approval of the Catholic position on these issues would not have significantly assuaged Catholic resentments. Yet Budge and O'Leary have much enriched our understanding of the tragic continuities of history in Belfast and Northern Ireland.

JAMES S. DONNELLY, JR.  
University of Wisconsin,  
Madison

MARC BOULOISEAU and BERNARD CHÉRONNET, editors. *Cahiers de doléances du Tiers État du bailliage de Gisors (secondaire de Rouen) pour les États Généraux de 1789*. (Ministère de l'Éducation Nationale, Commission d'histoire économique et sociale de la Révolution française. Collection de documents inédits sur l'histoire économique de la Révolution française.) Paris: Bibliothèque Nationale. 1971. Pp. 271.

The *cahiers* of grievances prepared throughout France before the meeting of the Estates General in 1789 have long been recognized as an important source. Many have been studied, and now we have those of Gisors, a "secondary" *bailliage* lying within the *grand bailliage* of Rouen. This volume, issued under the direction of the Commission d'histoire économique et sociale de la Révolution française, is a detailed analysis of fifty-one *cahiers*—those of forty-nine parishes plus that of the town of Gisors, all being combined into the general *cahier* submitted to Rouen. Since the *cahiers* of Rouen have already been edited by Bouloiseau (2 vols., 1957, 1960), with those of the remain-

ing secondary *baillages* in preparation, we now near the point where the printed documentation for one of the largest and most varied *baillages* of ancient France will be available.

A general introduction including maps and many tables deals with the history and administration of the *bailliage* of Gisors—the soil, people, economy, officers, taxes, electoral procedures, and a general analysis of the *cahiers*. One sees how sophisticated such historical study has become since Edmé Champion's pioneer work, *La France d'après les cahiers* (1897). The actual texts occupy the greater part of the volume. Six *cahiers* closely follow a model, the "Essai d'un Cahier de pouvoirs et instructions" of Jacques Thouret, a prolific publicist and member of an enlightened bourgeois Society of Thirty in Rouen. Eighteen further *cahiers* show how neighboring parishes produced documents with strong similarities. Twenty-five are original, some pathetically crude, and others fluent and sophisticated, these showing a dominant, if undetermined, hand. The editors argue that the parish assemblies assented, with very slight additions or modifications, to a draft put before them, confirming what Beatrice Hyslop wrote in 1967—that the local *cahiers* may not be as reliable an expression of the peasants as was once thought. Three parishes simply sent their representatives to Gisors with full powers.

In general these *cahiers* conform to patterns already familiar. While those based on a model had a wider view of France's problems, the general concern was with acutely felt local grievances. Many interesting details emerge. An average of 25.6 per cent of those on the tax rolls actually appeared in the parish assemblies, in numbers ranging from 4 to 53. Only 14 out of 873 names were signed with a cross, which may simply mean that the illiterate stayed away. A few *cashiers* spoke of the damaging effects of cotton spinning machines ("la fabrique anglaise") upon cottage handicrafts.

The volume also prints the *cahier* for the town of gisors and the *cahier* made by combining this and the parish documents into the general *cahier* taken to Rouen. In this last stage the urban Gisors *cahier* gave little regard to the long list of specific local grievances, illustrating one of the factors making the repre-

sensation in the Third Estate at Versailles overwhelmingly bourgeois. A most useful "Index des Doléances" systematically groups complaints. The largest concerns the *impôts*; one of the smallest deals with pigeons ("trop grand nombre"). Only three ask for improvements in education. Finally, a two-page "Essai de lexique sociologique" demonstrates the emergence of such terms as "bien-être," "capitaliste," "citoyen," "nation," "patrie," "peuple," and "sujet."

This and similar volumes will be invaluable for a wide range of historical purposes. The conclusion deserves quotation: "Point d'attitude politique, point encore de conscience de classe, mais un embryon de conscience sociale" (p. 105). One must regret that Beatrice Hyslop, a pioneer in the field, did not live to write this review.

ERNEST JOHN KNAPTON  
Chatham, Massachusetts

DIRK HOEGES. *François Guizot und die Französische Revolution*. (Romantische Versuche und Vorarbeiten, 44.) Bonn: Romanisches Seminar der Universität Bonn. 1973. Pp. 199.

The title of this compact dissertation is a misnomer. It deals less with Guizot's attitude to the Revolution than with the entire nexus of his ideas about history and social relations. Guizot liked to think of himself as an agent of a historical process. He was aware of the immense complication of history, and for this reason he preferred Shakespeare to Racine for the rendering of historical complexity. But his powerful systematic mind sought themes and principles in the story. The principle of European civilization was heterogeneity. The English were strong in practical action but somewhat deficient in speculative thought; the Germans were the reverse—thought-rich but deed-poor; only the French kept social circumstances and ideas going together. The central theme of French history was class struggle. Since the twelfth century the middle class has been struggling to advance its fortune, first in alliance with the kings and then against them. As the bearer of reason in history, it should triumph and establish a representative regime where good sense, tested by free debate in the market place of ideas, will prevail. It opposed

both the arbitrary fanaticisms of the left during the Terror as well as the egoistic despotism of Emperor Napoleon. By granting the Charter of 1814, Louis XVIII had adopted the Revolution as well as the principles of 1789 and of the middle class. Frenchmen should defend the charter and keep the government true to its principles, which are those of French history.

All of this has been said before about Guizot and even subtly related to his personality and life by Douglas Johnson (Guizot: *Aspects of French History, 1787–1874* [1963]), Mary O'Connor (*The Historical Thought of François Guizot* [1955]), and Charles Pouthas (*Guizot pendant la Restauration* [1923]). Dr. Hoeges's scholarship is impeccable, but the net gain from his investigation is simply to offer a convenient, compact statement of Guizot's thought.

HAROLD T. PARKER  
Duke University

LOUIS GOTTSCHALK and MARGARET MADDOX. *Lafayette in the French Revolution: From the October Days through the Federation*. Chicago: University of Chicago Press. 1973. Pp. xi, 586. \$22.00.

The present volume, the sixth in Professor Gottschalk's multivolumed biography of Lafayette, traces its subject through a period of only nine months, which were, however, the time of Lafayette's greatest influence and prominence. They were also the most peaceful period of the French Revolution, reaching from the women's march on Versailles in October 1789 to the *fête de la fédération* celebrating the first anniversary of the fall of the Bastille, that is until July 14, 1790. As in the preceding volume (*AHR*, 75 [1970]: 1128–29) the treatment is strictly narrative and chronological. It draws on an exhaustive examination of the periodical press and on contemporary letters, diaries, and pamphlets, with a judicious use of memoirs written in later years and a thorough knowledge of unpublished papers by or about Lafayette found in many depositories in France and the United States.

Lafayette during these months was commandant of the Paris National Guard, the citizen militia that was the only armed force in the capital. It was both his duty and his personal desire to transform the Revolutionary



action of 1789, as codified in the newly emerging constitution, into a new regime of stability and order. As a highly visible military officer, and in his actual exercise of what was in effect the police power, he became a conspicuous target for all who wished to use the Revolution of 1789 for other purposes or to reverse it altogether. These peaceful months of the Revolution were in fact a time of conspiracy and intrigue, on both right and left, punctuated by aristocratic plots. Orleanist machinations, radical agitation of the kind represented by Marat, and more ordinary political maneuvering at the government level, as in the relations between Lafayette and Mirabeau. It is doubtful whether anyone could have stabilized a constitutional order in such circumstances or imposed any order except by a form of dictatorship, which was precisely what Lafayette, as an admirer of Washington and the Americans, refused to accept. The book, like its predecessor, evokes the day-to-day immediacy of a city and country in turmoil without much general analysis or comment, but what it shows is the problems besetting an upper-class and high-minded liberal during an actual revolution. Within another two years, by July 1792, Lafayette was to be a defector, scorned by the zealots of all parties, and never thereafter quite the hero in France that he has remained in America.

It is regrettable that Gottschalk's long-time collaborator, Margaret Maddox, did not live to see the present publication, but the results of her work will no doubt be evident in the volumes to follow. Fortunately the principal author seems indestructible, and two more volumes are to bring Lafayette to the moment of his withdrawal from the Revolution in 1792.

R. R. PALMER  
Yale University

MICHAEL L. KENNEDY. *The Jacobin Club of Marseilles, 1790-1794*. Ithaca: Cornell University Press. 1973. Pp. xiii, 245. \$12.50.

There have been numerous general studies of the Jacobin club and its influence on politics and society in Revolutionary France. Similar works have been devoted to the various provincial clubs, but none have been concerned primarily with the club at Marseilles, the

"eldest son" of the Jacobins and certainly one of the most powerful and influential in France. Professor Michael Kennedy has undertaken the task of presenting for the first time a detailed analysis of the composition, organization, operations, and policies of the Jacobin club of Marseilles and the influence it exerted throughout France.

Although a study of the Marseilles club is useful in understanding the general operations of the thousands of provincial clubs, Kennedy correctly observes that this analogy must be limited, for the Marseilles club maintained a vast degree of independence and exercised an inordinate influence over both local and national policies. He describes the origins and early struggles of the club with the national guard and the municipal governments for ascendancy over Marseilles. Later this influence was extended to the neighboring departments through its sixty-two daughter clubs, roving commissioners, and eventually the direct intervention of the city's troops. Through its ambitious publishing program of newspapers and circulars and the efforts of its aggressive corresponding committees the views of the Marseilles club were presented and urged upon the central government and the clubs of the other eighty-three departments of France. Often arrogant and intolerant over its leadership role in the Midi the officials of the club successfully defied the Legislative Assembly, were among the vanguard of those clamoring for the overthrow of the monarchy, and attempted to circumvent the authority of two leading representatives on mission, Barras and Fréron. However, with the centralization of power by the Committee of Public Safety in 1793, the club became more docile and readily adopted the views and policies of the *Montagnards*.

Kennedy effectively traces the transformation of the principles and policies of the club after the Federalist revolt and the effects of democratization of its membership. From a club dominated by moderate bourgeoisie it became a militant class-conscious organization willing to defend the popular terrorism of Robespierre. It is unfortunate that the records of the great debates and speeches of the club members no longer exist, resulting in what seems to be rather superficial coverage in some instances. Nevertheless, Professor Kennedy is to be com-

mended for his patience and determined efforts in seeking out the remaining pertinent documents scattered throughout France. In addition to his use of untapped sources and his re-evaluation of previous studies on the Jacobin clubs, Kennedy has presented a concise, well-written, and objective study that will add a new and important dimension to our understanding of the club as a cultural and political force in Revolutionary France.

DONALD D. HORWARD  
Florida State University

LOUIS CHEVALIER. *Laboring Classes and Dangerous Classes in Paris during the First Half of the Nineteenth Century*. Translated from the French by FRANK JELLINEK. New York: Howard Fertig. 1973. Pp. viii, 505. \$15.00.

The publication of this faithfully translated edition of Chevalier's overpowering and provocative study opens a vast panorama of early nineteenth-century Paris to a wide student audience, and it will enable them to come directly to grips with a number of important questions concerning the range and methods of historical analysis.

After fifteen years Chevalier's plea for the infusion of demographic materials into social history is no longer on the historiographical frontier, but his attempt at a synthesis of census data and literary documentation will have a durable appeal to anyone who feels uncomfortable about the limitations imposed on scholarship by the escalating demands for technical specialization.

The theme of the study is clear and simple. Following a long period of demographic equilibrium, population pressure in Paris between 1815 and 1848 produced a pathological city, biologically, economically, and politically. While the stated methodological aim is to set up demography in a *menage à trois* with economics and political action, *Laboring Classes* is far more an affair of demography and literature. Chevalier begins and ends as the collector of the collective literary consciousness of Paris. He is less interested in artistic creations than in background images that novelists could not help recording. The meandering Eugène Sue is the ideal author, having written his serialized *Mystères de Paris* in continuous dialogue with his readers.

Chevalier's approach is certainly more reminiscent of Hugo and Michelet than of the sixth section. The vivid prose that permeates his book is testimony to his fear that the mere combination of fact and analysis runs the risk of literary mortality, unless novelists, who may begin as sociologists, infuse them with "a higher and more immediate form of life" (p. 31). *Laboring Classes* wants not just to convince but to cast a spell. From the outset one is plunged into a shadowy underworld where a perpetual miasma hangs over a brutal landscape. Descriptions of sewage and industrial poison are interspersed with cold tables of illegitimacy and suicide. Piling determinisms of architecture on those of number, Chevalier even evokes a somber God of evil who hovers over the worst sections of Paris.

The statistics are handled very much like the literature. Chevalier values them not for what they imply about individual or group action so much as the hints they give of attitudes and behavior beyond the act itself. His search for "the biological bases of social history" includes the expected survey of population in terms of age, sex, immigration, and so on. The excretions of society into its hospitals, orphanages, old-age homes, and graves tell a story of illegitimacy and concubinage, madness and suicide, failure, disease, and death. The ultimate determinant is a massive migration into the city, producing a population of nomads and criminals and deteriorating the working classes of Paris. Pathology envelopes normality. Chevalier's biological interpretation is unrelenting. A degenerative process produced a race of hostile primitives who were literally nasty, brutish, and short. These beings, savages in the full nineteenth-century sense, were without morals or religion, family or law. Social and biological descriptions are blended to portray an ugly, degenerate "race" of Parisian workers.

The statistics often concern not specific human groups but geographical areas, opening the door to an urban jungle of ecological fallacies. Even more serious, the reliance on contemporary images often leads to a blurring of analytical powers. For example, crime is purportedly the most significant of all indicators of pathology. But not a single table of criminal activity for the period is provided—no totals,

no time series, no breakdowns by age, sex, occupation, types of crime—in short, no numbers. As with the literature, specific accounts of criminal acts are ignored. Why? Because for Chevalier crimes are not acts involving agents and victims. Crimes are not committed but excreted. Their stink is more important than their contents. Collective awareness of criminality is what really counts. Even criminal recruitment is not described. It is simply inferred from statistics on illegitimacy, not because any data are given to show covariance, but because in 1847 the Paris chamber of commerce concluded that those born outside the law were cursed with “a flaw *ab initio*.” The breach of the family contract led logically to the breach of the social contract.

Finally the book touches on political violence, the most dangerous form of working-class violence before 1848. Chevalier merely takes us to the threshold of the phenomenon, announcing that as crime is the extreme, so politics is the supreme form of violence, both arising from the same pathological condition.

However, in the spirit of his sprawling novelists, Chevalier often presents enough data to make him a witness to alternative explanations. Sometimes his data overrun the boundaries of the period and allow one to question the whole thesis of physical deterioration. His generous sampling of eighteenth-century commentators casts more than one shadow of doubt over the uniqueness of nineteenth-century urban pathology. Regarding working-class political action, Chevalier briefly acknowledges that Parisian violence was often the work of native artisans rather than marginal nomads. Finally, without tackling the implications, Chevalier twice traces a movement of opinion that increasingly differentiated workers from criminals and converted the “proletariat” from a savage “race” to an identity-proud class.

Does the sewer, after all, tell all? In *Laboring Classes* we gaze deeply into all the inglorious days and nights of Paris. Chevalier, deeply moved by his own investigation, attributes to this misery the unusual volatility of Paris before 1848. Yet, was Paris unique in its epidemics or overcrowding, its concubinage or its crime? Even on the banks of Walden Pond one could muse that most men lead lives of quiet desperation. It was, however, the novelty

of collective violence for emancipation that drew attention to the banks of the Seine. That was the mystery of Paris. Chevalier's attempted synthesis of demography and literature and of politics and biology leaves one acutely aware of the problem of missing links.

SEYMOUR DRESCHER  
University of Pittsburgh

AUGUSTE COMTE. *Correspondance générale et confessions*. Volume 1, 1814–1840. Edited by PAULO E. DE BERRÊDO CARNEIRO and PIERRE ARNAUD. (Archives positivistes, 6.) Paris: Mouton. 1973. Pp. xxxi, 437. 88 fr.

This is another volume in the series put out by the Archives positivistes. Since 1939 six volumes have now been published. The present volume gathers together Comte's letters from 1814, when he first arrived in Paris from Montpellier, to 1840, just two years before the publication of *Le Course de Philosophie Positive*, on which he was still working. Further volumes will include the letters from 1840 to Comte's death in 1857.

In the absence of an autobiography, which he had intended to write, Comte's correspondence provides the best personal information available to us on his ideas and character. Earlier partial collections of his letters have appeared, the first as early as 1870, under the direction of the positivist disciple, Laffitte. The executives of Comte's estate, at odds with Laffitte, later put out three other volumes of *Lettres a divers*, between 1901 and 1905. Several smaller collections also appeared down through the years, so that by early in the present century most of his known correspondence had been published. Between the First World War and 1939 other letters turned up and were published. Most recently some have even been found in the shops of autograph dealers. Everything written between 1814 and 1840, that had been found as of 1972, has been included in this present volume. Still missing are some six hundred of Comte's letters, in addition to the letters that he wrote to his family in Montpellier, despite efforts by several generations of Comte specialists to locate the documents.

This present volume provides an excellent, easily usable tool for the historian. It may, in

the notes, presuppose great familiarity with some aspects of nineteenth-century French history, but it is still the volume to have for a start on the complete chronological collection of Comte's correspondence. The July 1824 letter to Thomas Jefferson, written by a twenty-six-year-old Comte, lays out the plan of Comte's life work on how to reorganize society and asks for comment from "an illustrious man who both as a thinker and as a statesman is set so high in the history of his country and of humanity as a whole." There seems to be no reply from Jefferson.

JOHN W. PADBERG, S.J.  
St. Louis University

LAWRENCE C. JENNINGS. *France and Europe in 1848: A Study of French Foreign Affairs in Time of Crisis*. New York: Oxford University Press, 1973. Pp. vii, 280. \$6.50.

This book will prove valuable to all students of international affairs in 1848, who for an understanding of French policy have hitherto had to depend upon older works and studies limited to certain topics or to a portion of the revolutionary year. The author has done exhaustive research in the French diplomatic archives and has also consulted those of Great Britain, Austria, Sardinia, and Denmark. Rich in detail on the response of European governments to the February revolution and many other issues, this study is not, however, as comprehensive or as balanced as the title implies.

Jennings devotes far too much attention to the Italian question, which was undoubtedly central, but which has been thoroughly examined before; indeed, he adds little to the analysis offered long ago by A. J. P. Taylor. Moreover, the author considers relations with Austria almost solely as an aspect of the Italian problem, ignoring the Hungarian and Czech movements and French attitudes toward the possible dissolution of the Empire. Even the German unification movement is handled in one of several chapters dealing with Italy.

This book will be of interest primarily for its factual detail, because its general conclusion, that French policy was cautious and pacific, is already standard and a number of interpretations, especially on German unification and on the Roman question, are highly debatable. Oc-

asionally also Jennings stumbles into that old pitfall awaiting the diplomatic historian, attributing too much significance to what one agent said to another. For example, on the idea of a Russian alliance, Jennings attaches more importance to a Russian feeler in Naples than to a formal proposal from the head of the French state to the tsar. Jennings is only partially successful in his attempt to account for the failure of the French to make war on behalf of other revolutionary movements, because his scarcely original explanation is that its leaders were moderate republicans rather than Jacobins. Yet he does demonstrate the continuity of French policy throughout the year. From Lamartine to Bastide and Cavaignac, he tells us, France pursued a "traditionalist oriented foreign policy," and his summary conclusion contains no reference to that supposedly great turning point, the June Days.

In sum, this book is informative but not definitive. For an adequate understanding of French policy on a number of issues one must still consult other works.

FREDERICK A. DE LUNA  
University of Alberta

A. G. SLUTSKII. *Parizhskaia Kommuna 1871 goda: Kratkii ocherk* [The Paris Commune of 1871: A Brief Sketch]. 2d ed.; Moscow: Izdatel'stvo "Nauka." 1971. Pp. 276.

B. S. ITENBERG. *Rossia i Parizhskaia Kommuna* [Russia and the Paris Commune]. (Akademiia Nauk SSSR, Institut Istorii SSSR.) Moscow: Izdatel'stvo "Nauka." 1971. Pp. 201.

The centennial of the Paris Commune inspired the USSR Academy of Sciences to publish these studies in its "scientific-popular" series. With little new documentation and inadequate bibliographies, the Slutskii and Itenberg works show that the conventional Soviet view of the Commune has not been altered.

When Slutskii wishes to interrupt his narrative to point out the "errors" of the Commune leaders, quotations from Marx are at hand. The Communards, we are told, proved they could substitute a popular alternative to a decadent class-controlled state structure but Proudhonist federalism weakened their position. Lenin, in his delineation of a Soviet-controlled society, developed Marx's insights. Nowhere is it sug-

gested that the complexity of events in 1870-71 prevent overconfidence in generalization, that Marx's analysis and recital of events are open to question; "bourgeois historians" (not named) are condemned for neglecting the social elements of the Revolution! The author adds his own criticism of the Communards: they should have acted earlier to suppress hostile newspapers, and they should have seen the need for greater discipline in the armed forces. Slutskii's style is simplistic and stilted; he assumes readers so ignorant that they must be informed in a footnote who were the Orleanists.

Russian reaction to the Commune hardly ranks in interest with the Commune itself, but Itenberg, known for his writings on the Social Revolutionary movement of the 1870s, has provided some information about the response of Russian liberal, conservative, and socialist publicists to the events in Paris. S. D. Kuniskii in *Russkoe obshchestvo i Parizhskaia kommuna* (1962) had noted the importance of the Commune to the development of Lavrov's concept of the militant party. Itenberg cites material from an unpublished Lavrov manuscript to demonstrate how early Lavrov's experiences during the Commune had affected this direction of his thought. But Lavrov like Bakunin, Itenberg claims, misunderstood the application of the lessons of the Commune to Russia. Lavrov felt the peasants would play the leading role in the Revolution, while Bakunin failed to see the need for organization. Lenin, on the contrary, understood correctly the meaning of the Commune: do not trust the bourgeoisie and establish an independent organization of the proletariat to carry on the class war. Poor Communards, your history, even under the auspices of the Academy, must serve the party.

FRANKLIN A. WALKER  
Loyola University,  
Chicago

THETA H. WOLF. *Alfred Binet*. Chicago: University of Chicago Press. 1973. Pp. xiii, 376. \$13.75.

Convinced that Alfred Binet, late nineteenth-century psychologist as well as experimenter and reformer in the field of education, has been "strikingly, singularly neglected in the history of psychology," Dr. Wolf has undertaken to fill the gap. She confesses that she found the

"diversity, minute detail, and apparent unrelatedness of his projects . . . unsettling." In addition the sheer bulk of his productivity might have deterred her from including the comparisons with the concepts of his contemporaries that might have fleshed out the study and enhanced its value. The book as Wolf conceived it, however, is a fine piece of scholarship; her research in the voluminous and mainly French sources has been thorough, and her good fortune in gaining interviews with Binet's associate, Théodore Simon, was of tremendous value. Despite her obvious empathy with her subject she evaluates his many productions objectively. Her organization, however, seems unnecessarily artificial. After a thirty-nine page "overview" of Binet's entire life and a second chapter on his first professional decade (of "errors compounded"), the author divides her material topically according to Binet's major interests. The result is a series of separate essays, and the reader must provide himself with a set of mental cross-references in order to create a sense of continuity. Moreover, Wolf's belief that her book "reflects the effects on a man's career of his personality and the personal events of his life" is erroneous so far as the latter is concerned, since almost nothing could be learned about Binet's relationship with the mother who reared him, with his wife, or with his two daughters beyond the important series of tests to which he subjected his offspring. On the other hand the author was able to glean several basic personality traits from Binet's relationships with colleagues and subordinates.

It was natural for the young psychologist to be drawn into the controversy over newly realized potentialities of hypnosis, and it was equally natural for the intellectually aggressive Binet to take up the cudgels for his famous mentor Jean Martin Charcot. But the opposing Bernheim-Liébault group demonstrated the role of suggestibility in Charcot's sensational achievements with hysterics, and Binet emerged from his humiliation with a life-long conviction regarding the importance of that factor in all human relationships. Frequently thereafter he warned "about its insidious infiltrations into the work of unsuspecting experimentalists, especially psychologists and psychiatrists." Wolf might have emphasized further the importance

of this insight; not until recent years have some psychiatrists, notably Jan Ehrenwald and Jule Eisenbud, publicized the frequent incidence of unconscious communication with patients, and only more recently have psychologists begun to recognize—as a new discovery—its impact on the outcome of experiments (cf. Robert Rosenthal, *Experimenter Effects in Behavioral Research* [1966]).

As the range of Binet's experiments broadened so also did the scope of his writing; the author comments that his genius drove him "compulsively" to "fantastic productivity." In addition Binet was director of France's first psychological laboratory, editor of its first psychological journal, and president of and guiding force behind a society of school administrators and instructors. The author is especially interested to discover why such a brilliant, indefatigable, and prolific scientist failed to receive recognition—scarcely at all in his own country, and beyond France only for the famous Binet-Simon intelligence scale (under current criticism, ironically, for the very reasons that Binet urged it should not be used: as an absolute standard without continued experimentation and without adjustments to meet environmental variations). Among the factors blocking success were Binet's failure to receive a professorship, as did his German contemporary William Wundt, to whom students flocked from abroad, the aloofness among peers that Wolf ascribes to diffidence based on this lack of institutional status; the dispersed nature of the work and its appearance in scattered publications; and the caustic pen that particularly alienated the medical profession. Yet much of Binet's writing is still revelant; Wolf finds it a mine of ideas for researchers. It is to be hoped that her book will succeed in establishing the credit that is long overdue.

MARY R. DEARING  
Montgomery College,  
Takoma Park

EDWARD L. MORSE. *Foreign Policy and Interdependence in Gaullist France*. (Written under the auspices of the Center of International Studies, Princeton University.) Princeton: Princeton University Press. 1973. Pp. xiv, 336. \$14.50.

Increasing transnational interdependence since

World War II has produced dramatic changes in international relations. Gaullist foreign policy illustrated the limits imposed upon national independence in a highly interdependent system.

The first third of this study develops a series of analytical generalizations intended to describe the major alterations produced by modernization of the nations of the Western world and their growing interdependence. Centralized, multifunctional, highly politicized modern states tend to place greater priority on domestic needs than on external needs. They have been encouraged by the demands of modernization to remove barriers between themselves through increasing coordination of national policies, though such attempts have generally fallen short of actual integration. This "systemic interdependence" tends to weaken the distinction between domestic and foreign policy, reduce the traditional priority placed on security and defense, and emphasize the achievement of wealth and welfare. A state's control of both its domestic and foreign policies has, consequently, decreased, and it has become virtually impossible to achieve national objectives in isolation. Rational control of foreign policy is reduced, and attempts to assert national autonomy often lead to counterproductive solutions or even violence.

Morse tests these theoretical hypotheses by examining the general problems of French foreign policy between 1962 and 1969. Gaullist France is selected because of "its consistency in foreign policy objectives" and its emphasis on national autonomy. A detailed examination of monetary policy (an excellent synthesis), Common Market relations, the Strategic Nuclear Force, and national planning leads Morse to conclude that the French quest for autonomy was severely curtailed by interdependence.

Occasionally the study's theoretical structure tends to encourage a fatalistic approach to Gaullist policy. Greater consideration might have been given to the question of how truly modernized Gaullist France was, at least in relation to the other states of the Western world. It is possible that the anachronistic character of French foreign policy was in part determined by the need to cope with the remnants of the "stalemate society."

Morse's account depends heavily on secon-

dary sources and contains no bibliography. Although he cites the earlier memoirs of de Gaulle, Morse does not use the more recent *Memoires d'espoire* (1970-71), nor does he refer to Couve de Murville's *Une politique étrangère* (1971).

Although the hypotheses, as the author suggests, must be more fully tested by examination of the foreign policies of other modernized states, they offer a thought-provoking reinterpretation of both the foreign and domestic policies of Gaullist France.

EDWIN H. TRAINER  
University of Tennessee,  
Knoxville

MARIO DEL TREPPO. *I mercati catalani e l'espansione della corona d'Aragona nel secolo XV*. (Università di Napoli, Seminario di Storia Medioevale e Moderna, 4.) Naples: L'Arte Tipografica. 1972. Pp. xiv, 870. L. 12,000.

This is a massive work in every sense. Professor Del Treppo has collected, analyzed, tabulated, and synthesized a mass of data that should, I hope, radically change some of the opinions of historians who view Iberia through the eyes of a Madrileño. In the past few years Anglo-American historians have demonstrated that, while Bourbon Iberia may have been a dying polity (despite the economic reforms of Carlos III), Bourbon America was vibrant and expansive, and as late as the 1790s (as Warren Cook in *Flood Tide of Empire* has shown) Bourbon power was sufficient to prevent both Russia and Great Britain from turning the Pacific Northwest into a Russian or British preserve. Now we have an Italian historian who has examined the so-called Catalan Decadence, i.e., the post-Black Death period. Given the fact that this period was one of general West European economic depression, what emerges from Del Treppo's work is a picture of a vibrant empire whose political power was such as to dominate the western and central Mediterranean and to turn the area into a Catalan economic community. Just as historians have demonstrated that the true power of Castile shifted to the Americas, so Del Treppo demonstrates that the true economic power of the Crowns of Aragon shifted to the Italies (Sicily, Naples, and Sardinia). Using Aragonese-Catalan and Italian

archival material the author examines cargo, ships, armaments, methods of business, insurance practices, times of sailing, salaries and profits of those involved, and imperialistic-economic policy. (I wish he also would have expanded on what he called the increased industrialization of the Italies, but his chief interest is commerce not industry.) With tables, charts, lists, and even a biography, the author inundates the reader with a flood of data to demonstrate the vitality of the "decadent" polity that conquered Naples and spread its mercantile power through Italy and the eastern Mediterranean.

This work is of such superior caliber that one hesitates to say anything negative, but there are questions. There is no bibliography. What the author, or perhaps the publisher, has done is to prepare an index of authors used. In a work this size and of this quality the index of authors is not only no substitute for a bibliography but might raise questions about the author's scholarship. The first chapter of the book, for example, outlines Aragonese-Catalan imperialism from 1220 to 1390, but the significant works of Charles Emmanuel Dufourq are not mentioned. I would suppose that Del Treppo knows Dufourq's extensive work on Catalan-Berber economic relations; I am also sure that he must be familiar with the extensive work of Filipe Mateu i Llopis even though the only work mentioned is *La Moneda Española*, but because there is no bibliography, I have no hard evidence. In addition to the index of authors used, the book has an index of places mentioned (including New York) and an index of people, but no index of ideas and concepts. There is no quick way to find "insurance contracts," "partnerships," or even what products were sold.

I would also question the accuracy and validity of using percentages. A historian who depends upon archival material must realize that he is using legal evidence only. What about smuggling? Did all Catalan ships leave from ports where a royal notary kept record? How must commerce left for Alexandria from a sandy beach near Palma or near Naples? Del Treppo used material in Europe, is there anything in Muslim archives? Perhaps in a lesser work this question might be less important, but in a work of this size and thoroughness the

reader might be led to believe that when the author lists x number of ships sailing for Alexandria in a given year that was all that went. Basing percentages on incomplete data really creates more problems than it is worth. Until the Muslim archives are examined I doubt whether we can fully appreciate the extent of the Catalan economic community, but until the Arabs, Berbers, and Turks comb their archives, Del Treppo's book is the best we have.

J. LEE SHNEIDMAN  
Adelphi University

GRUPO '73. *La economía del Antiguo Régimen: El señorío de Buitrago*. Madrid: Departamento de Historia Moderna y Contemporánea, Universidad Autónoma de Madrid. 1973. Pp. 221.

Professor Artola and his students have produced a study that dissects the eighteenth-century society and economy of the district of Buitrago, a mountainous area north of Madrid. In the process they have given us a valuable case study of how a compact royal and seigneurial jurisdiction functioned in Old-Regime Spain. Based on the amazingly full sources of the Catastro of 1750, the work gives us a carefully detailed account of land use, income flows, population, and distribution of wealth. The mechanisms of control that determined the emphasis on sheep raising and transferred a sizeable share of the regional product to a few people and institutions outside the district itself are clearly delineated.

As the title implies, the region presents in microcosm most of the characteristics of central Spain. Within the district the administrative center is the only town with more than the most rudimentary occupational structure or higher individual incomes and most of this additional activity depended upon governmental authority for its sustenance. Since most of the Spanish interior had a similar relationship with its capital, the analogy is close. The analogy also holds in the analysis of the agricultural self-sufficiency of the Buitrago area and the difficulty of finding commercially viable products given the lack of access to markets. The only regional exports were wool and charcoal, paralleling the wool and supply trades that characterized the interior as a whole.

The one weak aspect of the book is the final

chapter, which traces the evolution of the region to 1870. Following the careful analysis and impressive empirical foundations of the body of the book, the treatment of nineteenth-century developments is somewhat simplistic and unconvincing. Nevertheless, this is a suggestive book, not only because of its content, but because of its origins. It is a collective work by Professor Artola and a seminar of nineteen *licenciado* students. In American terms, it is best described as a collective senior thesis. That being the case, the book speaks well not only for Professor Artola as a scholar and teacher, but also for the level of scholarship being reached in Spanish universities despite the difficulties of their current situation.

DAVID R. RINGROSE  
University of California,  
San Diego

MIGUEL ARTOLA, editor. *La España del Antiguo Régimen: Estudios históricos*. Volume 6, *Castilla la Nueva y Extremadura*, by MARIA DOLORES MARCOS GONZÁLEZ. (Acta Salmanticensia, Filosofía y letras, 64.) [Salamanca:] Universidad de Salamanca. 1971. Pp. 122, 7 maps.

In the historical literature of the Spanish Old Regime, very few studies have been devoted to Castile and Extremadura. This book makes available new data, as well as graphs and maps. As the dual title suggests, Miss Marcos González undertakes to portray the character of both regions at the end of the eighteenth century (roughly 1750 to 1830), and her conclusions aim to instruct others in the proper means with which to analyze the Old Regime in Spain as a whole.

Miss Marcos treats Castile and Extremadura as geographic, social, and economic unities. She studies population, seigneurial jurisdiction (the best chapter in the book), and economy. The sources are mainly printed information (Sebastián Miñano, Canga Argüelles, Larruga), which she describes in certain detail. They appear in the form of appendixes, useful information on population, demographic changes, seigneurial jurisdiction according to provincial and administrative divisions, industry, and reproductions of Tomás López's maps of various towns. If, however, the strength of local studies lies in the capacities to expose the uniqueness of a region and to quantify the data behind the



generalizations conceived on a national basis, this study does not succeed.

The book shows that although Castile and Estremadura form a geographic unity, they have distinct characteristics. Miss Marcos establishes that population growth has been larger and more concentrated in Castile, particularly the secondary and tertiary sectors, whereas Estremadura's population originates mainly from the primary sector. But demography is not the only difference between the two regions; there are also diverse rents, tithes and taxes, as well as different types of seigniorial landlords (Church, military orders, and aristocrats).

The chapter devoted to the economic structure is far more puzzling; the reader has the feeling of threading his way through land property, agricultural prices, cattle raising, and industrialization with no clear direction. The author makes constant comparisons between such disparate dates as 1799 (the main source is the *Censo de frutos y manufacturas*) and 1967 (Banco de Bilbaos *Renta nacional de España*) and thus gives a rather unbalanced and blurred picture of the economy during the Old Regime.

Herein lie the flaws of this book. Miss Marcos describes but does not analyze, and her conclusions are too general and not always clear when confronted with the central issues. The book lacks the precise chronological frame of reference needed to deal with the Old Regime and makes no distinction as to the different stages of economic development within such a broad period.

Perhaps the fascicule has been long in press and hence does not make use of important studies in this field. Yet, it is inexcusable that it makes no reference to previous work by Pierre Vilar and Gonzalo Anes, who proved that at least since 1787—and particularly 1797—there were clear signs of capitalism in Spain, in contrast to the earlier part of the century, still immersed in Old Regime structures. Furthermore, although the title claims to deal with the Old Regime as a whole, the sources stress mainly the very late eighteenth and early nineteenth centuries. Also, it must be pointed out that the scant sources are printed with an almost absolute neglect for archival documentation, except for the Ensenada census. Problems arise as well with the figures on population

growth. The reader hardly ever knows whether they are total series (national) or partial (regional). In short, the information presented does not form a coherent pattern and the reader is left without a comprehensive view of Castile and Estremadura during the Old Regime.

Economic matters are not dealt with with care or subtlety. Yet, the book proves the obvious: the need for regional studies in Peninsular history. In this sense, one must congratulate Professor Miguel Artola for having undertaken the direction of this series, of which two other fascicules on Salamanca and Castile la Vieja have already appeared. When the remaining six studies are completed, historians will have a basis for furthering the study of the Old Regime in Spain, both in the national and the local level. The present work is a step in the right direction.

IRIS M. ZAVALA

State University of New York,  
Stony Brook

ANTONIO GARCÍA-BAQUERO GONZÁLEZ. *Comercio colonial y guerras revolucionarias: La decadencia económica de Cádiz a raíz de la emancipación americana*. (Publicaciones de la Escuela de Estudios Hispano-Americanos de Sevilla, number 206.) Seville: the Escuela. 1972. Pp. xix, 254.

With the approach of the Holy Alliance troops the liberal Spanish government forced King Ferdinand VII to take up residence in Cadiz. As the main port of trade with Spanish America this city had a mercantile bourgeoisie of a very liberal spirit. Once the absolutist regime was re-established Cadiz sent to the king a report with a dual purpose. On the one hand the city sought forgiveness for having been his prison, and on the other it asked to be recognized as a free port to compensate for the economic crisis caused by the independence of the Spanish American colonies and subsequent loss of trade.

Using this document as a starting point and complementing it with extensive research, Dr. García-Baquero, of the University of Seville, has brought to light in this book much new information about the fluctuations that characterized the city's commercial life. These ups and downs were linked not only to the decline

of trade with Spanish America, but also to the general crises of the eighteenth century. Commerce with the Spanish-American colonies was, he notes, basically of an intermediary nature, since Cadiz imported foreign products that were then exported to the colonies as Spanish. Entering Spain through Cadiz were coffee, dye-producing scale insects or cochineal, gold, and, more important, the Enlightenment and freemasonry.

Cadiz's economic decline followed Spain's. In 1829 Ferdinand VII recognized Cadiz as a free port, but, despite a notable increase in trade with Europe, the measure was largely ineffective and commerce with the former colonies continued to suffer.

This study by Dr. García-Baquero documents a troubled period in the port's history and gives us an idea of the economic effects throughout Spain of the loss of her American empire. It would, perhaps, have been opportune to tie this decline in with the evolution of the city's political stance. Cadiz grew from Spain's most liberal city, thanks to its mercantile bourgeoisie, into one of its most revolutionary. While the mercantile bourgeoisie tempered its ideals the masses sought to put them into practice to alleviate the harsh effects of Spanish-American independence on their daily life.

Cadiz is an example of something worthy of note: that it is frustration rather than desperation, and loss of prosperity rather than poverty, that lead to politically radical action.

VICTOR ALBA  
Kent State University

T. K. DERRY. *A History of Modern Norway, 1814-1972*. New York: Oxford University Press. 1973. Pp. xii, 503, 2 maps. \$16.00.

This is a well-conceived, well-balanced, and well-written book for which the British author deserves great credit. The portion on Norway in 1940 is based on original research published as part of the United Kingdom History of the Second World War under the title *The Campaign in Norway* (1952). The rest is derived from a judicious reading of printed sources and literature, mainly in Norwegian but also in other languages. There are thirty-four pages of reference notes and bibliography that provide

a useful guide to any librarian wishing to check holdings in Norvegica. Here, as in the rest of the book, there is an astonishingly high level of accuracy with only a few orthographic mistakes of little consequence. As in all other works on Scandinavian history in English, one could wish that the index had treated the special letters the way the Library of Congress does, that is, *aa* for *å*, *ae* for *æ* or *ä*, and *oe* for *ø* or *ö*.

In his preface Dr. Derry states that: "This book presents the history of Norway since its separation from Denmark as a study in the growth of a small nation with aspirations towards political democracy, egalitarian social forms, economic advances, and cultural achievements" (p. vii). He does this in a masterly way by interweaving political history, traditionally the chief concern of Norwegian historical writing about the nineteenth century, with social, economic, literary, and artistic developments. Thus Ibsen, Björnson, Grieg, and Edvard Munch all receive their proper places, and other such important figures as Amundsen, Nansen, Koht, Trygve Lie, Undset, and Thor Heyerdahl are not forgotten. His account of Quisling, unlike that of Hayes, is to be trusted. It is surely a proof of the importance of Norway's recent history that so many Norwegians can be identified by educated Westerners. The accomplishments of Norwegians who emigrated in such large numbers to the United States receive proper attention, and it is reassuring to see how well Derry has penetrated the extensive Norwegian-American historiography. Hyphenated Americans like Ole Rølvaag, who wrote *Giants in the Earth* in Norwegian, and Thorstein Veblen need to be seen, as the author does, against their Norwegian background.

Considering that Dr. Derry is not a Norwegian this is a very Norway-centered book. There is sometimes a gentle note of impatience or slight irritation when Norway's neighbors are discussed, especially Sweden. There is a tendency to underplay the importance of Scandinavianism in the lives of ordinary people (something that has recently been reinforced by the coming of television and increased travel between the Scandinavian countries) and the Nordic Council with its many accomplishments. Still there is no doubt that just as modern Norway has fulfilled most of its nineteenth-century aspirations so has Dr. Derry succeeded

in writing a truly fine history of this accomplishment.

ERNST EKMAN  
University of California,  
Riverside

HOLGER HJELHOLT. *Arvefølgesag og forfatningsforhold i det danske monarki ved midten af 19. arhundrede: Fr. v. Pechlins virksomhed for monarkiets opretholdelse ca. 1845-51* [Succession Questions and Constitutional Conditions in the Danish Monarchy at Mid-19th Century: Fr. v. Pechlin's Activity to Maintain the Monarchy, ca. 1845-51]. (Det Kongelige Danske Videnskabskabernes Selskab, Historisk-filosofiske Meddelelser 46, 3.) Copenhagen: Munksgaard. 1973. Pp. 199. 60 D. kr.

This volume is primarily concerned with the matter of the royal succession in Denmark and with the work of Friedrich v. Pechlin to maintain the Danish monarchy in the years 1845-51.

Pechlin was born in Schleswig, educated both in German and Danish, and for twenty years he represented the interests of his king in the German Bund. By 1845 he was increasingly aware of the dangers inherent in the probable extinction of the branch of the House of Oldenburg then ruling in Copenhagen. He viewed with anxiety the sudden eruption of German nationalism in the 1840s and the subsequent rise of a new Danish national feeling and a strengthening of Scandinavianism. There were several younger branches of the royal line, and Sweden, Russia, and England were intensely interested in continued Danish national independence. Any Danish succession question would afford Austria and Prussia a chance to bring Schleswig into the same relation to Germany that Holstein held, perhaps to Germanize it completely.

Pechlin held that in 1721 the Danish Law of Succession had been extended to Schleswig, but not to Holstein. He also felt that under no circumstances should Schleswig become a part of Germany, yet he also felt that its relation to Holstein should be maintained. All the possible heirs to a vacant throne, except the husband of a Danish princess (the crown prince of Sweden, the later Charles XIV), were Germans, who held very lightly any allegiance to the Law of Succession. This did not make the task any easier.

Even more dangerous was the fact that both Christian VIII, dead in 1848, and Frederik VII, his childless son, hated the possible heirs to the throne, a feeling reciprocated by all the heirs. The Oldenburgs were fundamentally Germans and their wilfulness and stubbornness made their autocratic ideas most difficult to apply. Nicholas I of Russia could readily keep the Swedish Bernadottes in check, but Palmerston in England was a free agent, consistent only in inconsistency. Metternich in Austria and Radowitz in Prussia, though in constant opposition to each other about German affairs, could not but be German in response to the wave of popular feeling that Schleswig should be assimilated to Holstein and must be made German in every way. Metternich disappeared from the scene in 1848, but to Austria anything that involved Prussia in a foreign imbroglio and diverted German liberal and radical attention from Bohemia, Hungary, and Italy could not be wholly bad.

Pechlin strove for all these years to keep the Danish monarchy intact as a triple entity made up of Denmark, under its own law and administration, Schleswig, with a German majority and a Danish minority beginning to react vigorously against the waves of German nationalism—a Schleswig that should remain both German and Danish, bound to Denmark by king, army, treasury, and law, but separate from it in identity—and Holstein, to which Schleswig was tied by historic and linguistic bonds. Holstein differed from it in that by long standing it was a member of the Germanic Diet. He strove in every way he could devise to make certain that the application of the Danish Law of Succession to Schleswig be recognized; that the question of the succession be made either a Danish family matter to be settled by family negotiations or else one in which a Danish monarchical solution be confirmed by those great powers who had by the Congress of Vienna ratified Denmark's boundaries and existence.

The volume is most informative and interesting, even if Pechlin's work was doomed to fail in the face of the impossibility of reconciling the numerous personalities, interests, and movements. Between the Danish royal house and its expectant other branches; between England and Russia; between Austrian and Prussian hopes and ambitions; between Danish national-

ism and the exuberance and witlessness of German expansionism, the sober rationalism of Pechlin had little chance. I compliment the author on his work and not in the least on his success in untangling so many twisted skeins of diplomacy and personality. I cannot but feel that German popular behavior from 1840 to 1852 does much to explain how and why 1933 became possible and must enter my caveat against Burke's dictum that a whole people cannot be indicted. I feel that Pechlin could not have been successful—he was too mentally balanced in a milieu so comprehensively opposite.

FRANCIS J. BOWMAN  
University of Southern California

PETER BLICKLE. *Landschaften im Alten Reich: Die staatliche Funktion des gemeinen Mannes in Oberdeutschland*. Munich: Verlag C. H. Beck. 1973. Pp. xxi, 609. DM 98.

This book examines the political role of the "common man" (meaning, essentially, the peasant) in early modern Germany. It is a constitutional study focused on the territorial diets or similar local assemblies in which peasants were directly represented, and its attention is confined, therefore, to the southern territories of the Empire, roughly from Alsace to the archbishopric of Salzburg. Blickle works within, yet modifies, the conceptual framework pioneered by Otto Brunner and Karl Bosl. His *Landschaft*, the corporate body of legally privileged subjects within a lordship, is more comprehensive than the nobility and clergy, which have received almost exclusive attention in previous literature of importance. His shift of emphasis to the lower social orders, the skill with which he summarizes earlier work and integrates his findings into it, and the broad range of his own archival research make Blickle's study a major contribution to the history of representative institutions in early modern Europe.

After a general survey of the territories where peasants had representation in diets or other assemblies, Blickle presents a detailed analysis of three regions: the Tirol, the Vorarlberg, and Kempten. The origins, structures, and functions of the assemblies are examined in their local historical contexts then drawn together in a systematic discussion that compares Blickle's

results with available literature on the territories not examined so intensively. The determination to be comprehensive and comparative is certainly laudable, yet the result is also an overly long and sometimes annoyingly repetitious book. The schematic organization of the monograph makes it easy for the nonspecialist to skim, however, and he will find the effort well worth his while.

Blickle demonstrates the active role of peasant assemblies in their territories even after the defeat of the rebellions that culminated in 1525. Their activities in defense, taxation, and general finance were most important but come as no surprise. More striking are the author's findings concerning their strong influence on territorial legal codes, ability to spare peasants from carrying the full burden of imperial taxation, establishment of fixed norms for dues and services, and internal composition. Although the elected representatives in the diets were generally wealthy peasants with experience as local officeholders, Blickle finds little evidence of close family ties among delegates in the Vorarlberg or Kempten. This fact, the absence of co-optation, and his conviction that peasant leaders enjoyed the real confidence of their local communities lead Blickle to doubt the existence of rural oligarchies in the *Landschaften* of southern Germany. But most readers will probably prefer to reserve judgment on this issue until more local prosopographical research has been done. Considering the enormity of that task, Blickle may surely be excused for not undertaking it in this book. Less excusable and quite disappointing, however, is his failure to document carefully his assertion that the assemblies were active in shaping important social legislation like poor relief.

Blickle's emphasis on peasant political activities does not lead him to exaggerate their importance, despite the literary flourish referring to democracy and parliamentarianism in the last paragraph of the book. Earlier he shows that the development of estates (both as social orders and formal political institutions) depended essentially on the prior existence of territorial lordship; indeed, the strength of the peasantry in the diets of the south resulted from its more direct seigneurial and juridical ties to the rulers there. Blickle also traces the gradual weakening of peasant initiative and of the diets themselves

over the early modern period. Both fell victim to a more dynamic force in German politics—the steady bureaucratization of the territorial state.

GERALD L. SOLIDAY  
Brandeis University

S. L. VERHEUS. *Zeugnis und Gericht: Kirchengeschichtliche Betrachtungen bei Sebastian Franck und Matthias Flacius*. (Bibliotheca Humanistica & Reformatorica, volume 1.) Nieuwkoop: B. de Graaf. 1971. Pp. 121. 45 gls.

Until rather recently historical writing during the Reformation was commonly judged by the standards of modern "objective" scholarship and hence often considered simply apologetic if not downright unhistorical. Attitudes have changed in the last two decades, however, as historians have come to view history writing as a product of an age worthy of investigation in its own right. In this translation of his revised 1958 Dutch dissertation Dr. Verheus has contributed to this rehabilitation by examining closely what he considers the two great histories produced by the early Reformation: the *Geschiedtbibel* (second edition of 1536) of the spiritualist Sebastian Franck (1499–1542), and the *Magdeburg Centuries*, the guiding light of which was the rigid Lutheran Matthias Flacius Illyricus (1520–75). Verheus believes that both historians and theologians have neglected these works and failed to understand that theology, and especially eschatology, so thoroughly informed and pervaded the historical outlook of Franck and Illyricus that their histories must be regarded as confessional rather than partisan in nature, that is, intensely personal accounts of the way God has revealed Himself and justified man in history. In summarizing, comparing, and criticizing the two works Verheus offers numerous insights that should be especially valuable for students of historical theology, although historians will find useful some of the remarks in the introduction and the "Comparative Evaluation" (pp. 95–113) of the two works as histories.

But there are difficulties. The book is repetitive and sometimes obscure or confusing. It could certainly have been tightened up. Partly as a result of this want of coherence Verheus does not convincingly demonstrate the centrality

of eschatology for Franck or Illyricus; it remains, as did secular history for Illyricus, but an "appendage." The treatment of Franck can be questioned in two respects: first, sole reliance on the *Geschiedtbibel* to the exclusion of Franck's other works, particularly the *Pardoxa* (1534), which could clarify points in Franck's conception of the church; and, second, Verheus's disputable belief that Franck adhered more closely than Illyricus to the Reformation view of history, whatever that was. These problems are compounded by a translation that is more than occasionally ungrammatical and cumbersome. Fruits there are in this book, especially for students of theology, but it is a pity they can be gathered only with considerable effort.

LAWRENCE G. DUGGAN  
University of Delaware

WINFRIED BECKER. *Der Kurfürstenrat: Grundzüge seiner Entwicklung in der Reichsverfassung und seine Stellung auf dem Westfälischen Friedenskongress*. (Schriftenreihe der Vereinigung zur Erforschung der Neueren Geschichte, 5.) Münster: Verlag Aschendorff. 1973. Pp. ix, 419. DM 80.

For more than a decade German scholars have labored to compile and publish under the general editorship of Max Braubach and Konrad Repgen a massive document collection entitled *Acta Pacis Westphalicae*. The research interests of Winfried Becker represent a natural outgrowth of his collaboration on this extended project. His monograph on the *Kurfürstenrat*, written initially as a doctoral dissertation, originated in connection with preparation for the edition of that body's records from the Congress of Westphalia. Pointing out that a study of the *Kurkolleg* has not yet been written Becker offers a general justification for his own work by arguing that certain facets of German constitutional history have been neglected as a result of the tendency to concentrate upon the development of the territorial rather than the imperial estates. In attempting to compensate for this relative neglect he has produced an exhaustive treatment of his subject through the use of a variety of archival materials as well as published sources and secondary authorities. Becker traces with meticulous care the early evolution of the *Kurkolleg* and its impact on the imperial constitution and the place of the electors in the

literature of constitutional law, but spends most of his time discussing the electors' role in the various stages of the Westphalian peace negotiations. Constantly stressing their relationships with the emperor, the other German princes and free cities, and key foreign powers, Becker sees the imperial estates functioning as valuable mediators during the final stages of the congress.

Winfried Becker has produced what will undoubtedly remain the definitive treatment of the *Kurfürstenrat* for some time to come. He has brought together in a single volume a wide range of information not easily accessible elsewhere and has supported his data with elaborate documentation, an extensive bibliography, and a comprehensive index. But if he is to be admired for his painstaking scholarship, he must also be faulted for his lack of imagination and insight. Becker's style is heavy and stiff, and those who manage to read this meticulous study from cover to cover are likely to find few stimulating ideas or conclusions to reward their perseverance. Certainly their views on the Congress of Westphalia will not be altered in any substantial fashion. Nonetheless, the fact that Becker has managed to fill out details of an already established picture with care and precision will make his book a welcome addition to the library of the specialist.

JOHN A. MEARS  
Southern Methodist University

SELMA STERN. *Der preussische Staat und die Juden*. Part 3, *Die Zeit Friedrichs des Grossen*. Volume 1, *Darstellung*; volume 2, *Akten*, in two parts. (Schriftenreihe wissenschaftlicher Abhandlungen des Leo Baeck Instituts, number 24, parts 1 and 2.) Tübingen: J. C. B. Mohr (Paul Siebeck). 1971. Pp. xv, 426; v, 814; v, 815-1615. Cloth DM 480 the set, paper DM 450 the set.

While Selma Stern's earlier works on Anacharsis Cloots (1914), on the Court Jew (1950), and Rosel von Rosheim (1959) were fascinating studies of individual Jews, her monumental work *The Prussian State and the Jews* distinguishes her as a leading contemporary historian of German Jewry. Her first two volumes of *The Prussian State and the Jews* have found admirers and critics. But frankly speaking, any historian interested in the evaluation of Ger-

man, and especially Prussian, society will read Selma Stern's final volume in the series, *The Age of Frederick the Great*, with fascination, for it was precisely in this period that the social and economic position of the Jews in Prussia began to undergo a profound change. Frederick II and his bureaucrats initiated and accelerated a process that eventually was to turn a foreign-appearing minority, restricted to the ghetto, into citizens of a modern state. Selma Stern's book analyzes this slow process in practically all aspects of life. Apart from Frederick II himself, two government agencies mainly dealt with Jewish affairs: the Generaldirektorium in Berlin, in particular its second and fifth departments, and the provincial administration, the Kriegs- and Domänenkammern. The enormous amounts of material emanating from these bureaucratic bodies, plus, of course, the correspondence of other local government officials and petitions of representatives of Jewish groups of letters of individual Jews form the documentary basis of the book. Selma Stern traces the ambivalent role Frederick II played in the crucial transformation of the life of the Jews in Prussia. Actually, his attitude was not so ambivalent. In spite of all his philosophical talk he viewed the Jews with deep suspicion and hostility but was also keenly aware of the vital contributions they could make to his policy of turning Prussia into a first-rate European power. Contrary to Frederick II, the bureaucrats often showed an understanding and even sympathetic attitude toward the Jews, partly because they had been trained in the new philosophy of natural law, and surely also because they were practical-minded men.

After a discussion of the special and economical position of the Jews in the newly-acquired provinces of Silesia, eastern Friesland, and West Prussia, Selma Stern describes the various forms of taxation to which the Jews were subjected and the general policy of the Prussian government—summarized in the General-Privilegium of 1750, which until 1812 laid down the guidelines as to how many Jews were allowed to live in the monarchy, which types of commerce and industrial production they could practice, and how elections within the congregations were to be organized. The general tendency of Frederick's policy was to limit or, if possible, reduce the number of Jews in Prussia.

Frederick's absolutist state could not tolerate the existence of a practically autonomous Jewish jurisdiction by rabbis and therefore tended to subject the Jewish population to the jurisdiction of the ordinary courts. It is along these lines that the slow processes of assimilation—often against resistance by the Jews themselves—moved in eighteenth-century Prussia. Two instructive chapters are devoted to the roles played by Jews in Frederick's mercantile policy: the eminent rôle in the textile trade, especially with Poland, and the great contribution to the development of the manufacture of cotton and silk in Prussia. I found the discussion of the trading connections of the Jews in West Prussia and Silesia with those in Poland and Russia especially instructive. In order to exploit these connections to the advantage of the Prussian state, the bureaucrats seriously proposed the establishment of an autonomous Jewish city in eastern Pomerania. While Frederick simply rejected this scheme, his encouragement of commerce and manufacture by Jews explains the rise of a very prosperous and financially powerful bourgeoisie of Jewish manufacturers, merchants, and bankers—especially in Berlin but also in the provincial cities.

The two concluding chapters go far beyond a social, economic, or political history of the Jews in Prussia. In the second half of the eighteenth century the position of the Jews in Prussian society was publicly debated, and it is within this framework that Selma Stern subjects the views of Kant, Lessing, and Herder on the Jews to a searching analysis. The last chapter is devoted to the conflicts the increasing influence of eighteenth-century philosophy and its assimilation to traditional Jewish thought caused to Moses Mendelsohn and other Jewish intellectuals.

Selma Stern's book on the gradual social and economic transformation of the Jewish community in Prussia and its beginning intellectual emancipation is a truly outstanding work of scholarship. Indeed, the painstaking analysis of documentary evidence can easily compare with the works of Hintze, Hinrichs, or Rosenberg on eighteenth-century Prussia. Miss Stern's book is an irenic piece of work, without angry denunciations or diatribes. Its graceful style lends to the book a humanistic and even aristocratic touch: indeed, there is a tendency to dwell on

the families of eminent rabbis, wealthy merchant princes, and powerful court bankers of the Berlin congregation. We hear little of the ordinary cattle trader or the poor fellow who had to be taken care of by a congregation. Fortunately, Selma Stern still gives younger historians trained in the social sciences, a chance to prove their talents. How many Jews were actually in the monarchy? And how many congregations in how many towns and villages? Miss Stern emphasizes the economic and social differences between the Jews in West Prussia and Berlin or East Friesland, but as our age is addicted to statistics, we love to have figures on occupation, income, and the like. Miss Stern has offered young historians a chance to enlarge our knowledge of the social position of the Prussian Jews in the reign of Frederick II by supplementing her monograph with two marvelous volumes of documents, among them numerous detailed statistical tables of the Prussian bureaucrats. In short, the great strength of Miss Stern's book is its lucid interpretation of the enormous mass of political documents and its truly sovereign handling of the philosophical issues. Although Miss Stern has lived many years in the United States, she has remained a German historian in the tradition of *Geistesgeschichte*.

CLAUS-PETER CLASEN  
University of California,  
Los Angeles

CHRISTOPH WEBER. *Aufklärung und Orthodoxie am Mittelrhein, 1820–1850*. (Beiträge zur Katholizismusforschung. Series B: Abhandlungen.) Paderborn: Verlag Ferdinand Schöningh. 1973. Pp. 270. DM 18.

Weber treats the relationships of enlightened reformism and of religious orthodoxy in early and mid-nineteenth-century Rhenish Catholicism both to each other and to the political events of the years 1848–50. In the bulk of the book he recounts various internal struggles within the Church. He moves back and forth within a chronological schema between the cities of Koblenz and Trier, dwelling heavily on the networks of personal relationships among competing members of the Church hierarchy and their supporters among the laity. During the decades leading up to 1848 there was a growing

polarization between the upholders of doctrinal orthodoxy and their opponents, who increasingly included among their ranks the Hermesians. These men had originally sought to stake out a middle position between the theological extremes, only to find that the efforts of their teacher, the Münster theologian Georg Hermes, to reconcile Catholicism with the rationalism of Kant, forced them to cast their lot with the reformers against the ultramontanes.

Weber insists that the conflict between reformers and conservatives in Church affairs cannot be equated with the opposition between democrats and conservatives in the political arena. In his chapter on the years 1848–50, he asserts that the forces of radical Church reform and those of orthodoxy stood closer to each other politically than did either group to the Hermesians. Not only the extreme reformers, but also many of the ultramontanes supported liberalism and democracy. Ultramontanes adopted such tactics largely out of a desire to secure greater autonomy for the Church from the Prussian bureaucracy. In contrast, the Hermesians' advocacy of reform from above and their desire to integrate the Church into the state as a means of facilitating the process of liturgical and doctrinal rationalization pointed them toward acceptance of the existing political order. Enlightened Catholicism thus split apart again into two wings, one of which was genuinely radical while the other joined forces with the reactionary Prussian state.

The most interesting parts of the book deal with the Hermesians, whose significance in the history of the Rhineland, Weber asserts, has been seriously underestimated. However, Weber himself points out that their influence in the Church was declining during the years preceding the revolution, and he never makes entirely clear the nature of their wider impact outside the Church. His contention that Hermesianism was a particularly middle-class form of Catholicism, which reflected the desires of the Rhenish bourgeoisie for both a theological and a political *juste milieu*, seems plausible enough in the light of what he tells us about the positions adopted by the movements' leaders, but he does not really say enough about the social setting in which the movement grew up to support this part of his argument conclusively. More attention to these matters instead of quite so much

detail on the jockeying for position among the different factions within the Church would have been welcome.

ANDREW LEES  
Rutgers University,  
Camden

TADEUSZ SEWERYN WRÓBLEWSKI. *Slawistyka w NRD i w NRF na tle jej historycznego rozwoju* [Slavic Studies in the German Democratic Republic and the German Federal Republic in the Light of Their Historical Development]. (Prace Instytutu Zachodniego, 45.) Poznań: the Instytut. 1973. Pp. 266. Zł. 45.

During the last 130 years Slavic studies in Germany went through different stages of development. In this book the author concludes that after the Second World War Slavic studies in Eastern and Western Germany developed along separate ways. Thus, in West Germany Slavic studies focused mainly on Slavic philology, and the wider aspects of the subject are still left largely to the system of *Ostforschung*. In East Germany, on the other hand, *Ostforschung* is viewed as the expression and continuation of German aggressive policies, and Slavic studies there encompass a wide range of subjects.

The author points out that until 1871 Slavic studies in Germany, under the influence of liberalism, took a positive and friendly attitude toward Slavic questions, but during the closing decades of the nineteenth century the rising nationalism interjected an element of hostility into the German approach to Slavic studies. It was the appearance at the turn of the century of the *Ostforschung* systems that brought a political element into the subject. The author further argues that today the tradition of *Ostforschung* is reflected in West Germany by the attempts to reduce the concept of European culture to its West European dimensions, which is expressed in terms of Germanic-Roman tradition, and which presents Slavic cultures as foreign to the Western tradition. But as the author himself indicates, many of the German Slavists themselves try to free Slavic studies from such political standpoints.

Here Wróblewski might have a point. The time has come for us finally to stop viewing history from its parochial and self-adoring point of view and to concentrate more on seeing European history and culture within its most com-



prehensive dimensions as an entity and a historical continuum. But, of course, it works both ways.

This book is a useful survey of Slavic studies in Germany; it is valuable as a reference source—it contains a large amount of bibliographical information within its body and some interesting biographical data about the leading German Slavists and the different Slavic programs.

C. M. NOWAK

Bridgewater State College

FRITZ BLAICH. *Kartell- und Monopolpolitik im kaiserlichen Deutschland: Das Problem der Marktmacht im deutschen Reichstag zwischen 1879 und 1914.* (Beiträge zur Geschichte des Parlamentarismus und der politischen Parteien, number 50.) Düsseldorf: Droste Verlag. 1973. Pp. 329. DM 72.

Arranged more like a handbook than a history, this volume describes, in considerable detail, the policies of the Reichstag toward the problem of monopoly power in the German economy before the First World War. Readers will wonder if the Reichstag can be said to have had policies and if it could possibly have made any difference in that constitutional setting. The answers to both questions are not the simple negatives that might have been expected. Hostility in the Reichstag toward those thought to be abusing their market power fused after 1900 into a demand for public regulation of cartels, and the government yielded to pressure from the Reichstag majority in presenting the registration law of 1912. The pressure for regulation was neither constant nor consistent, however. The question of market power appeared sporadically as a subsidiary issue in other debates or in response to immediate economic problems. In 1879 and 1902, for instance, groups pressing for increased tariffs were accused of planning a monopoly of the domestic market, and cartels were blamed for the shortage of coal in 1900 and the severity of the depression of 1908.

The hesitancy of the Reichstag is easily explained. On the one hand most members shared the opinion of professional economists that unrestricted competition was harmful, or at least outdated, and that cartels represented a progressive attempt to increase cooperation in economic life. Abuses of market power by cartels were "childhood diseases" of a new social order

and hence not justification for general restrictive legislation. On the other hand most Reichstag parties were directly beholden to one or more of the cartelized interests and, therefore, not disposed to see their exercise of market power as abuses at all. The Socialists recognized abuses, but favored concentration in general as a harbinger of the collapse of capitalism. Only the Catholic Center possessed the combination of size, commitment to reform of the existing system, and independence of cartelized interests to make it an effective opponent of concentrated economic power, and it was the Center, supported by the Socialists, that pushed the registration law of 1912.

Blaich shrewdly delineates the relations of parties to interest groups, but presents a series of case studies rather than a coherent story. The volume will aid those interested in particular cartels or interest groups, but would have to be expanded to include both government officials and public opinion to provide a complete picture of the role of cartels in the politics of imperial Germany.

FRANK B. TIPTON

Wesleyan University

EUGENE LUNN. *Prophet of Community: The Romantic Socialism of Gustav Landauer.* Berkeley and Los Angeles: University of California Press. 1973. Pp. x, 434. \$13.75.

Germany, as an industrial nation, has gone through several anguished appraisals of modernization and its ills. These appraisals have not generally shown much receptivity for the anarchist vision of society. That indifference, even hostility, underscores the tragedy of Gustav Landauer's life, a unique German anarchist visionary who perished in the Munich revolution of 1919.

Landauer embraced anarchism in the early nineties, inspired by distaste for the industrial state as well as by what he took to be its analogue, the well-organized, authoritarian Social Democratic party. Landauer's influence rose after 1893 when he became editor of *Der Sozialist*, Germany's only anarchist newspaper. His energies also propelled him into efforts to organize workingmen's cooperative communities. The experience had a lasting effect on Landauer's thought: only mutual self-help

organizations could overcome man's political dependency so as to enable him to build, by wholly peaceful means, an alternative to industrial capitalism and the state. Landauer vigorously condemned the anarchist terrorism of the nineties; indeed, his qualms about violence made for some fateful hesitations in the critical revolutionary years, 1918-19.

Landauer's magnetic charm attracted distinguished friends and associates who shared his romantic, populist, mystical, and communitarian ideas. They included Constantin Brunner, the Spinoza scholar; Fritz Mauthner, the linguistic critic; Erich Mühsam, subsequently active in the Bavarian revolution; and, as an especially close friend, Martin Buber. The latter's Chassidic mysticism interacted creatively with Landauer's insistence that the mystic sense of community is first experienced in the individual consciousness.

Mysticism and the anarchist philosophy of history expressed in *Die Revolution* (1908), which extolled medieval organicism, temporarily left Landauer ill prepared to understand the thrust of working-class activism both on the eve of the war and in its latter stages. The Bolshevik methods appalled him; a revolution carried out by a popular surge did not. By November 1918 Landauer became convinced that a libertarian revolution could be sustained by the workers' and soldiers' councils (*Räte*). South Germany—so Landauer thought—offered the best prospect and he hastened to Munich. From there, a platform rather than a political base, he worked tirelessly for the democratic and decentralized reordering of Germany. By February 1919 his championship of the *Räte* as the only popular source of genuine revolutionary change made him a conspicuous figure on the radical Left.

Despite some disillusioning experiences with Munich's radicalized industrial workers, Landauer pledged his support for the *Räterepublik* proclaimed in Munich on April 7. In its short life his democratic and humanitarian appeal became lost in right-wing calumny. Within a week the "republic of aesthetes and intellectuals" gave way to a Communist *Räterepublik*. Unreason and vengeance accompanied its downfall. The right-wing troops that had recaptured Munich callously ignored Landauer's anti-Com-

munist stand—they brutally murdered him while enroute to prison.

In an otherwise excellent study, distinguished by its breadth of research, some questions can be asked: Does the text's subordination of Landauer's Jewishness do justice to his subsequent influence on the agrarian-utopian elements in Jewish social thought? And since Landauer made his mark primarily as a writer, should the writings themselves have been given more prominence? We might also ask for more evidence of the verve and charm that entranced Landauer's companions, both men and women. Is it enough to say that Landauer's thought is an amalgam of German romantic, idealist, and populist ideas with overtones of modern anti-urban pessimism? Perhaps secondary figures are always more responsive to composite influences. That is Professor Lunn's view and he has sustained it in a well-wrought biography.

WILLIAM O. SHANAHAN  
Hunter College,  
City University of New York

REINHOLD KNOLL. *Zur Tradition der christlichsozialen Partei: Ihre Früh- und Entwicklungsgeschichte bis zu den Reichsratswahlen 1907*. (Studien zur Geschichte der österreichisch-ungarischen Monarchie, number 13.) Vienna: Hermann Böhlau Nachf. 1973. Pp. 319. DM 60.

In the late nineteenth century Austria developed a party system of particular interest to the student of history. With a general revulsion against political and economic liberalism came an abandonment of the traditional parties of notables and the formation of new types of mass parties. One of these was the Christian Socialist party. It is high time that it found a historian. And now, building on the conceptual framework of his teacher Adam Wandruszka (the Christian Socialist movement as a *Lager*), Reinhold Knoll has set out to trace the intellectual and sociopolitical development of the party through the general elections of 1907.

The author ably takes the Christian Socialist tradition back to the background of social romanticism (Adam Müller) and to the political Catholicism around Klemens Maria Hofbauer. Both failed to cope with the problems of the industrial worker and, in general, modernity, and thus they anticipated the course of the Christian Socialist party, a course much lamented by the

author. Knoll's preference goes clearly to Karl von Vogelsang, who indeed launched a Catholic approach to the social problem with the object of deproletarizing the workers and unifying capital and labor. For the rest, the whole history of the party seems but an anticlimax, a renegeing on Vogelsang's premises. This was the course followed by virtually all of its chief leaders—Schindler, Lueger, Funder, Weiskirchner, and Seipel, who presided, Knoll argues, over the "embourgeoisement" of the party and its transformation into a party of notables after all. Lueger, the great leader who, to quote the *Neue Freie Presse*, mixed "holy water with gasoline," was left, according to Knoll, without gasoline, the symbol of modernity. He held the party together with his charismatic personality. But after his death it had to give pride of place to the Socialists as a result of its middle-class course.

It is quite true that in the course of one decade of its history the Christian Socialist party had become the imperial party and that its leaders veered increasingly toward the Belvedere—all this at the expense of the focus on the social issue. In effect, however, the *haute bourgeoisie* had won out over the lower bourgeoisie. As for the industrial workers, it would have been too much (despite the efforts of men like Kunschak) to expect this particular party to have won them over and become a true *Volkspartei*. The author's somewhat excessive, though honorable, commitment has drawn him, so it seems, to this particular thesis, whereas a more detached understanding might have told him that within a parliamentary system no one party can be everything to everybody.

Overcommitment is the chief flaw of this book. It leads to a conclusion that is only half right, namely, that the little man was left out and turned to the Social Democrats. Much of this orientation was inevitable. But the fact is that the Christian Socialist party was essentially a middle-class party and not a workers' party, and it lost its natural electorate not to the Socialists but, eventually and for various reasons, to the Nazis. In all fairness, however, the time span of Knoll's volume does not extend into this period. All in all the book is tightly argued, well documented—partly from materials in the possession of the author. It constitutes an impressive scholarly achievement and

will be useful to future students of Austrian history.

KLEMENS VON KLEMPERER  
Smith College

*Year Book XVI.* (Publications of the Leo Baeck Institute.) London: East and West Library, for the Institute; distrib. by the Leo Baeck Institute, New York. 1971. Pp. xvi, 334. \$9.50.

The *Year Books* of the Leo Baeck Institute have become much more than a series of tributes to those European Jews murdered by the Nazis. Rather they are a valuable source for a historical understanding of, in particular, German Jewry and provide useful essays and documentation over a wide variety of topics. As Herbert Strauss rightly remarks, the impressive intellectual heritage of the German Jews and the paradigmatic lessons to be learned from the German-Jewish experience are of significance not only to scholars and students of German-Jewish history. This latest volume concentrates on the period of the Weimar Republic, but Strauss's penetrating contribution on immigration and acculturation of Jews in the United States is perhaps the most valuable and stimulating article, opening up new areas that deserve investigation and providing a framework of sociological analysis that offers new insights.

The position of the German Jews in the Weimar Republic is excellently handled by George Mosse and Donald Niewyk. No one now needs to be convinced of the many contributions they made to the new Republic, and the argument is rather whether this creativity was due to their deep commitment to democracy and humanism, or whether the overly critical tone, especially of the writers associated with the *Weltbühne*, was due to a continued feeling of alienation. Niewyk believes that Siegfried Jacobsohn, Kurt Hiller, and Kurt Tucholsky were among those irresponsible ideologues who did their best to discredit free institutions in Germany at a time when they needed every possible support. His view is taken up by Andrew Whiteside, who extends the parallel by pointing to the role of alienated young intellectuals—representing a continuing and peculiar characteristic of the emancipated Jewish intelligentsia—in the destructive radicalism of

America's New Left. Whiteside sees this as a perversion of the Jewish tradition of championing progress and social justice, but George Mosse's article on socialists and the Jewish question shows how ambivalent the attitude of many socialists was to those often unwanted allies. Even if the German Social Democratic party did not adopt Marx's pejorative view of the Jewish heritage, they were still liable to share the opinion that, if the Jewish connections with capitalism were broken by political action, the Jewish problem would be solved.

William Jenks and Walter Simon make useful contributions on the Jews in Austria. Both stress the wishful thinking that anti-Semitism was only a primitive survival of a vanishing dark age, and the reluctance with which the Jews engaged in a defense of Jewish particularity. But George Mosse rightly points out that it was one of the successes of the Nazis, and of their sympathizers in Austria, that they succeeded in forcing their adversaries to argue within a framework they themselves had laid down, however absurd this may have seemed to those who opposed them. More might have been made of the point that the middle-class Jews were caught in a time of social crisis that was to sweep away familiar landmarks and undermine confidence in all established institutions. Future research will have to investigate how far their selection as a scapegoat by both the extremes of Right and Left was due to their belated emancipation/assimilation or to the exposed and often deliberate alienation of a disproportionately visible and talented group of Jewish critics of post-1918 Germany.

The *Year Book* concludes with a valuable collection of Jewish petitions to the German National Assembly in Frankfurt, 1848-49, with some interesting notes on resistance to nazism among clandestine Jewish groups, and with an excellent bibliography of works published in 1970.

J. S. CONWAY  
University of British Columbia

HERBERT SCHWARZWÄLDER. *Bremen und Nord-westdeutschland am Kriegsende 1945*. Volume 1, *Die Vorbereitung auf den "Endkampf."* (Bremer Veröffentlichungen zur Zeitgeschichte, 5.) Bremen: Carl Schünemann Verlag. 1972. Pp. 205.

In Germany, where World War II was most nearly total, the tendency at the national level still is to view the conflict from the aspect of the Fuehrer headquarters that, sequestered in an east Prussian forest or buried under the *Reichskanzlei* in Berlin, was notoriously out of touch with the country. The war on the home front has been left to local historians, who are now apparently discovering a kind of nostalgic appeal in the miseries of the 1940s. It is too bad that their products often pass unnoticed and unfortunate that their efforts are, in fact, frequently most distinguished by earnestness and diligence.

The present work is a good example of the genre. It deals with events in and around Bremen roughly from September 1944 to March 1945, the period, as the title puts it, of "preparations for the last battle." These were months of nightmarish uncertainty as a faltering government tried half-heartedly to persuade itself and the population that old men and boys and a few third-rate troops could somehow stand off forces for which the front line armies were manifestly no match. The author has dutifully tracked down all of the projects and plans, including some of the most rapid busy work either side produced in the war.

The real story is not in what was committed to paper through official stupidity, but in the total impression of hopelessness and futility. From Berlin showers of orders to defend the city, on the coast, on the land approaches to the west, on all sides, drifted down to staffs that would have been somewhere else in the first place if they had really been capable of directing battles. The debate over whether to destroy the ports and docks on the Weser River, cripple them temporarily, or leave them alone went on so long that Eisenhower's command eventually joined in with propaganda leaflets pointing out that the Germans could do as they pleased—they would be the ones to starve if ships could not bring in food.

Finally, by April 1945, nobody in his right mind (but not all were) could see any sense in the plans. One problem had been solved, though. The Allied bombing had done almost as much damage to the ports as the Germans could have. The "last battle" was still in the

offing, however, and it will be the subject of another volume.

EARL ZIEMKE  
University of Georgia

FRANÇOIS JEQUIER. *Une entreprise horlogère du Val-de-Travers: Fleurier Watch Co SA. De l'atelier familial du XIX<sup>e</sup> aux concentrations du XX<sup>e</sup> siècle.* (Le passé présent: Études et documents d'histoire.) Neuchâtel: La Baconnière. 1972. Pp. 406. 30 fr. S.

The Jequier, like many other inhabitants of the valleys of the Jura mountains in western Switzerland, began to supplement their meager farming income by "handicrafting" watches back in the early nineteenth century. Like many other families they soon earned their livelihood from this trade, but unlike most they succeeded in growing with the industry and maintaining their family enterprise through good and bad times.

François Jequier belongs himself to the sixth generation of Jequiers who were engaged in the making of watches. He had access to company and family papers ordinarily closed to the researcher and used this opportunity on the whole to good advantage, covering his story in six long chapters with a prodigious scholarly apparatus of footnotes, appendixes, a detailed bibliography, and indexes.

Watch making was well suited for small scale work, individual initiative, and the family enterprise. Several members of the Jequier family had their own ateliers, which they directed with Protestant frugality, hard work, and considerable business acumen. "Every man his own master" was the motto through most of the nineteenth century, and competition even among brothers and in-laws was the order rather than the exception. "These men carried the conviction that their duty consisted in increasing their capital, their desire to make money was equaled only by their reluctance to spend it." Realizing that ferocious competition was hurtful to all, they began to make accords among themselves that culminated in the fusion of the two principal family groups into the Fleurier Watch Co. in 1915. The fusion made the Jequier enterprise the most important watch manufacturer in the Val-de-Travers, even though on a Swiss scale it never grew beyond a small to middling outfit.

Due to the character of the company and the sources used by the author most of the book is taken up by detailed accounts of the financial ups and downs of the family and their business. Annual reports are quoted at length and too often the story is little more than a running commentary on factual data supplied. Very little is said, for example, about the employees, presumably because they are only rarely mentioned in the annual reports. Working hours were long, salaries low, organization slow to come. The first response to every crisis was to lay off workers, apparently without ever creating any adverse repercussions. Post-World War II prosperity, however, led to a rapid change in the paternalistic relationship between workers and employers, as the growing shortage of labor forced the family to make more and more concessions in order to attract young people into their enterprise.

Jequier's book is a pioneering study into a hitherto well protected, secretive corner of Swiss business life. What emerges is a picture of frugal, dedicated people who through generations built up a business that weathered bad times and grew increasingly prosperous. Basically conservative, these people nevertheless were able to adjust to the changing times and managed to maintain themselves in enviable positions.

HEINZ K. MEIER  
Old Dominion University

FREDERIC C. LANE. *Venice: A Maritime Republic.* Baltimore: Johns Hopkins University Press. 1973. Pp. xi, 505. Cloth \$17.50, paper \$6.95.

We are offered the best one-volume history of Venice in any language. Representing in some respects a lifetime of work this book carries the marks of a personal statement, above all, in its new and thoroughly pondered appreciation of the sea in the fortunes of Venetian history. But it is presented in a cool, plain-speaking manner and has none of the impressionism or self-indulgence of the volume by P. Braunstein and R. Delort, *Venise: portrait historique d'un cité* (1971).

Professor Lane outlines the history of Venice from its origins among simple fishing folk to its current industrial problems and rate of sinkage. The overriding interest and glory go to the

medieval and Renaissance periods. The Middle Ages, to the end of the fourteenth century, get about 200 pages; the next two centuries get another 200; and most of the remaining text (some 70 pages) is devoted to the seventeenth and eighteenth centuries. Historians of the early modern period may regret the brevity of the third part, but in the treasure hoard of Venetian history the lion's share belongs to the period from the eleventh through the sixteenth centuries. The city made and spent itself then. Music aside, eighteenth-century Venice was a theatrical and painterly lark: not the most challenging game for our heavy methodological artillery.

The author is selective in the best sense: "I have put nautical affairs in the center of my story . . . because I believe they were important in determining Venetian social structure and the city's fortunes" (p. v). He details the effects of ships, rivers, and trade in the long arc of the Venetian economy. So doing, he is also able to trace the history of a ruling class, as its members grappled their way out of the lagoons to become pirates, long-distance merchants, shipping magnates, and empire builders. Their energies and organizing abilities, like those of the Genoese, were astounding. At first supple and open to the entry of rich new men, the ruling class grew increasingly exclusive after 1300 and became a caste well before the end of the century. The fifteenth century brought an apparent balance between the city's overseas interests and its aggressive expansion into the Italian mainland. In the course of the sixteenth century the old ruling caste abandoned the sea, retreated from maritime trade, and became a landowning, stay-at-home patriciate. Henceforth noblemen lived off rents, agricultural produce, money-lending, the spoils of office, and investments in the public debt. It is an old story but it has yet to be retailed as a transformation in sensibility: a transformation in social and economic attitudes first, then also in moral, artistic, literary, and intellectual values. Between about 1470 and 1515 the upper-class mind of Venice suffered the impact of momentous political and economic trials, the consequences of which were incalculable for Venetian culture. In striving to understand the realignments of the early sixteenth century certain Venetians adduced the putative

effects of sinful luxury, but most noblemen, if they could afford it at all, gave themselves to opulence—the high, frenzied mark of caste. Although Professor Lane does not study the intersections between cultural and social trains in Venetian experience his particular emphases served to help us understand the new currents surging through Venetian manners and purviews around 1500.

As a field for historical study Venice has always recruited detractors or partisans of the *mito di Venezia*—the myth of Venice's stability, concord, and political sagacity. Venetian history brings ideologies out into the open. Professor Lane does not stand with the detractors. This would require a New or Old Left historian with a more polemical view of oligarchy. Paradoxically, however, contributions to the myth of Venice can be more on target than historical debunking of the sort that takes one improbability for granted: namely, that ruling classes can at any time be more enlightened or more generous than they are in fact.

LAURO MARTINES  
University of California,  
Los Angeles

ERIC COCHRANE. *Florence in the Forgotten Centuries, 1527–1800: A History of Florence and the Florentines in the Age of the Grand Dukes*. Chicago: University of Chicago Press. 1973. Pp. xiv, 393. \$12.50.

Eric Cochrane has organized his history of Florence around portraits of six men who represent successive generations of Florentines from the mid-sixteenth century to the end of the eighteenth century—Cosimo I, Scipione Ammirato, Galileo Galilei, Lorenzo Magalotti, Giovanni Lami, Francesco Maria Gianni. He tries to place each in his full historical context, and in that process of rounding his portraits out he touches on social and economic conditions, politics, art, music, and other aspects of their respective ages, and at the same time establishes the linkages between the portraits that give the book its continuity and the justification for its title as a history of Florence. The book is addressed to the "educated layman," and the author assumes that scholars will want only to consult, not to read it. That condition precludes a fair treatment in the hands of a re-

viewer for this journal, who must evaluate such books in terms other than the author's, that is, in terms useful to scholars (and perhaps also to those educated laymen who may want to know what scholars think about such books).

Cochrane, who has published a monograph on the Tuscan academies, has read widely and is familiar with manuscript materials, and the text clearly reveals an extraordinary breadth of knowledge. The fifty-page critical bibliography is in itself an important contribution to scholarship. He is most at home in the intellectual world of his subjects, and he need not think that other scholars will only consult what he has to say about his eminent Florentines—they will find his analysis interesting and informative. But as he gets beyond his central portraits and dutifully takes up all those topics usually deemed necessary to fill in a historical background, his treatment is often unsystematic and impressionistic, too brief to be useful and sometimes even uninformed—for example, in the areas of art and economics (which, considering the importance of these activities in the earlier history of the city, one would have thought deserved more than just a few pages every now and then in the course of such a large book). Such are the pitfalls of any survey, but the problem of this book is compounded by the lack of the kind of systematic organization we usually adopt for the presentation of our materials and by the author's discursive, overconscious style that often precludes coherence in the treatment of the secondary subjects he touches on in extending his portraits. These limitations will be only too apparent to the reader who uses the index to track down any number of subjects one would expect to be treated in a history of Florence. In short, this is not a traditional history and it cannot be used as such. Yet, in view of the gaps in the scholarly literature Cochrane's effort can be considered nothing less than heroic, and we can be grateful to him for what is in fact the only survey we have of these forgotten centuries of Florentine history.

RICHARD A. GOLDTHWAITE  
Johns Hopkins University

DEREK BEALES. *The Risorgimento and the Unification of Italy*. (Historical Problems: Studies and Documents, number 11.) New York: Barnes and Noble, 1971. Pp. 176. \$7.50.

This volume in the Historical Problems series, edited by G. R. Elton, is intended for British undergraduates, and it consists of sixteen brief sets of documents, introduced in a forty-thousand-word essay on the unification of Italy. Dr. Beale's concise clarity, like the small print, permits a remarkable sweep in small compass. The result is an admirable, balanced, and provocative introduction to Italian history from the eighteenth century to 1860.

Derek Beales very explicitly presents an English view that first rejects the myths of Italian patriotic historiography and then dismisses the philosophical idealism long influential in Italy as a "high flown metaphysic [that] clashes violently with English pragmatism." For him the central question is the relationship between Italian unification and a national *risorgimento*; posing that problem enables him to underscore the importance of a special international situation in making unification possible despite limited popular participation and uncertain Piedmontese intentions. This is, then, a matter-of-fact and largely political account that skillfully uses bits of demographic, social, and economic data to provide an effective general introduction to Italian history. The accompanying documents are useful illustrations of the essay and similarly understandable, fresh, and evocative.

Such an approach, however, even in Beales's judicious hands, has its own dangers. First, it can, as Italian scholars will undoubtedly note, sound overly negative (Modena in 1831 may have witnessed a "trivial coup," but surely historical interest rests rather on its significance and impact). Second, and more serious, concern to right the balance leads to comments, for example, about the few signs of nationalist enthusiasm, or the high proportion of brigandage, or the personal ties among Italy's leading moderates that imply norms we do not have. We have no measure of how many nationalists of what fervor would be a lot in Italy, Ireland, Turkey, or Morocco; we have no standard to indicate when the proportion of brigands is high in Italy, Tennessee, Scotland, or Spain. Third, suspicion of historians' rhetoric easily becomes a tendency to disdain patriotic ideas, to note how much is "fantastic" in Mazzini and Gioberti, but overlook the need to explain why there really were revolutions in 1848 or patriotic demonstrations and thousands of volunteers

in the 1850s. Finally, this emphasis and the omission of economics leads away from recognition of how much the *risorgimento* was part of a general Western historical process. If many of the events central to Italian unification were fortuitous, the growth of liberalism, the cooperation of aristocratic and bourgeois reformers, and the need for efficient and orderly government were not.

One doubts that many of Professor Beales's readers will have the biases against which he protects them, but this shrewd and solid volume should lead them to an interest in Italian history and, ironically, a Trevelyan-like delight that the Italian national movement accomplished so much against such odds.

RAYMOND GREW  
University of Michigan,  
Ann Arbor

ROMANO UGOLINI. *Cavour e Napoleone III nell'Italia centrale: Il sacrificio di Perugia*. (Biblioteca scientifica. Second Series: Memorie, volume 28.) Rome: Istituto per la Storia del Risorgimento Italiano. 1973. Pp. vii, 427. L. 7.950.

Unlike the "five days" of Milan in 1848 and the "four days" of Naples in 1944, the "seven days" of Perugia from June 14–20, 1859, have not attained the features that elicit instant recognition in Italian and European historical memory. Yet, as Romano Ugolini lucidly, almost "definitively" illustrates in this finely wrought study, the tragic Perugian revolt, which failed against papal temporal domination in the course of the great war of 1859 then furiously raging in the plains of Lombardy, became as crucially influential within the fabric of European international politics and for the direction of the Neo-Bonapartist-Cavourian diplomacy of the Italian national revolution as the other two more celebrated explosions within their own contexts. The June 1859 rising in Perugia was correctly viewed by Cardinal Giacomo Antonelli, Pius IX's éminence grise at the helm of papal politics, as the one link in the subtle Cavourian chain of national-revolutionary "subversion" that had to be broken—paradoxically by recapturing and retaining it within the dominions of the pope. On this occasion, at least, Antonelli overwhelmingly defeated his Italian liberal-realist antagonists by literally exemplary use of the Machiavellian resources of the fox and the lion.

Ugolini has carefully reconstructed how Cavour gathered under his direct command, despite La Farina's nominal leadership, the network of pro-Piedmontese liberal conspirators in central Italy who operated through the aegis of the *Società Nazionale*. When, on June 14, 1859, the Perugian liberal elite proclaimed a junta of provisional government as a *de facto* ruling body after the flight of Monsignor Luigi Giordani, the weak and frightened pontifical delegate, it did so on the not illogical expectation that political and military aid would be forthcoming from Turin via Florence and Bologna. But no help, indeed hardly any response, came from Cavour's agents in Tuscany and Emilia nor, for that matter, from the other Umbrian towns or the Perugian countryside. For seven hope-filled days Perugia was "free" but totally isolated. In the meantime a papal army of counterinsurgency was rushing by forced marches through Umbria from Rome, and it broke into the city walls on the rain-drenched day of reckoning, June 20. Dispatched by Antonelli, mercenary troops under the command of the very able Swiss colonel, A. M. Schmid, burst into the ancient city, captured the leaders of the remaining resisters—fearing threatened "decapitation" if caught, the members of the junta had just barely escaped on the way to Florence—and then proceeded to sack, with truly fine irony, some of the poorer quarters of the old town. On that very day, as Perugia was falling, Pius IX declared in an allocution to the cardinals assembled in Rome that he had excommunicated all insurgents as well as those who, within and without the frontiers of the Papal States, sought to disrupt the territorial integrity of the Holy See. Spiritual punishment was thus poured upon the political and physical wounds inflicted through the "perfect" combination of Antonelli's strategic sagacity and Colonel Schmid's military *ricognista* of Perugia. For another year all was ominously quiet on the central Italian front of Cavour's wars of national liberation. As his keen-eyed antagonist Mazzini had prophetically proclaimed since early spring of 1859, a "new Campoformio" would mark the dissolution of the Franco-Piedmontese military struggle against Austria in the north, and the "new Campoformio" almost mathematically occurred less than three weeks after the fall of Perugia at Villafranca (July 11).



Among the many merits of Ugolini's reconstruction of the revolt of Perugia at least two or three should be noted. Ugolini's principal achievement lies in this subtle but extremely well-documented elaboration upon the theme that the fate of Perugia in June 1859 had a direct connection, if not quite a causal relationship, with the Bonapartist separate peace with Austria initiated by the armistice of Villafranca, with all its immediate and long-range consequences on both the Italian and European levels. Though he may have ignored the extent, Louis Napoleon did not misjudge the ultimate objective of Cavour's diplomacy. Whatever lay, politically and logistically, behind Cavour's "defection" at Perugia, the attempted revolt was viewed by Louis Napoleon as an inescapable function of dark Cavourian machinations to "steal" central Italy from his own as well as the pope's grip and thus reduce to shambles the pan-Italian partition plans of Plombières and the secret Treaty of Turin. The "sacrifice of Perugia" became a clear bifrontal sign of great troubles ahead if the emperor's Italian policy was pursued after that point in mid-summer 1859, according to the diplomatic-military blueprints of the previous fall and winter. The characterization "bifrontal sign" is, of course, mine and not Ugolini's, and I have used it as a stenographic phrase to call attention to another outstanding contribution, almost a novelty within the context of the young Italian scholar's analysis. For, upon the basis of an exceptionally rich documentation and, I am tempted to say, almost beyond a shadow of historical doubt, Ugolini shows how, after the disaster of Perugia, the emperor of the French became more than ever convinced that he had been in mortal political danger of being agent and not master, means and not end, and horse and not rider of Cavour's Italian nationalist policy. At Villafranca Louis Napoleon revealed that it was time for a great change, even if it cost the "sacrifice" of Cavour himself. When and if Louis Napoleon would be ready to "return" to the Italian question, it had to be by way of "Europe," that is, through European consensus, a congress of the great powers he hoped to manipulate to his own ends, but never again alone. Connected with this revision of French diplomacy, indeed as one of its central motivations, was the harsh lesson of events brutally

reiterated by the manner and meaning of the papal reconquest of Perugia. For at Perugia the Roman Curia had once again unmistakably gone on international record before all Europe and the Catholic world, and the Italian national-revolutionary movement would hereafter proceed only at the grave risk of the Church's irremovable resistance, within and without its temporal possessions in Italy, with all the military and political and religious arms at its disposal. From Perugia 1859 to Porta Pia 1870 and thence, on a different level, to the Lateral Pacts of 1929, the "warning" was not belied in Rome.

A final comment on Ugolini's procedure may be found useful by Risorgimento scholars. Through exhaustive but imaginative analysis of a relatively marginal episode in Risorgimento history, Ugolini has, I believe, succeeded, among other things, in helping to sweep away one or two lingering vogues of ideological approaches to the Italian national revolution. His work reveals not mere intimations but clear evidence of a methodological sophistication that contributes toward genuine historiographical originality. His insights run deep and prove most illuminating. He has apparently for long plunged and then come up from recesses of archival inquiry and, through diligent and intelligent labors, has turned what might have been just another specialized monograph into a real work of history. Particularly in the last three chapters of his volume Ugolini tantalizingly explores the interplay between historical event *wie es eigentlich gewesen* and the creation and function, the use and abuse, of historical myth. For this reason, too, his reconstruction of the "seven days" of Perugia constitutes a superb critical piece of work. Through microscopic analysis of a little revolution that failed during the crucial year of Cavourian politics, Ugolini illumines the hard realities and the grand illusions that darkly churned within the macroscopic parameters of the Italian Risorgimento.

A. WILLIAM SALOMONE  
University of Rochester

FRANK J. COPPA. *Planning, Protectionism, and Politics in Liberal Italy: Economics and Politics in the Giolittian Age*. Washington: Catholic University of America Press; distrib. by Con-

sortium Press, College Park, Md. 1971. Pp. x, 280. \$11.95.

This work is a welcome addition to the historical debate on the Giolittian era, a debate spurred by works such as A. William Salomone, *Italian Democracy in the Making: The Political Scene in the Giolittian Era, 1900-1914* (1945, revised in 1960) and Luigi Albertini's memoirs, *Venti anni di vita politica* (5 vols., 1950-53).

Coppa ranges himself alongside Giolitti's ardent defenders. The author, in a generally well-argued, well-documented study based on archival research in Italy, draws attention to the realities of Giolitti's economic and commercial policies and their relationship to political practice. Coppa attacks the charges made by ruling class theoreticians Vilfredo Pareto and Gaetano Mosca and contemporary Neo-Marxists such as Giampiero Carocci that Giolitti manipulated and exploited an alliance between industrialists and Socialists, as well as the accusation that Giolitti had an understanding with southern latifundists concerning agrarian protectionism. Coppa traces Giolitti's economic and commercial policy from early in his career to 1921, documenting that his ideal of a positive state and his protectionist stance were constant and consistent with the political, fiscal, and military needs of the Italian liberal state. Giolitti, in trying to preserve this state, believed that active government intervention to support Italian industry and agriculture and to uplift the masses was indispensable to his program. According to Coppa, Giolitti always held this belief and never departed from it.

But Coppa's method of immersing himself in a contest with Giolitti's vast array of critics almost consumes his broader goals. Industrialists (except perhaps in Piedmont and Genoa), workers, conservatives, democrats, and others all emerge as enemies of *giolittismo*. Yet, whereas Giolitti does not deserve categorical condemnation as the man responsible for destroying liberal Italy, neither does he stand virtually alone as the man who fought the liberal fight to save the Italian state of the Risorgimento. Because of Coppa's rigid attention to Giolitti's critics and his identification of Giolitti as the last representative of liberal Italy, the crisis within the liberal ruling class

is buried. Albertini (and his organ, the *Corriere della Sera*) is thus described at various points as the mouthpiece of Lombard industrialists, a conservative, the moral leader of the Giolittian opposition, a part of the capitalist camp, a supporter of fascism, and, accurately, a conservative liberal.

A more fruitful avenue of approach would be to see Giolitti and Croce as representatives of democratic liberalism locked in a mortal and ultimately tragic struggle with the conservative liberal ruling class represented by Salandra, Sonnino, and Albertini. In spite of differences on economic, political, and social doctrine, both elements initially supported fascism to save the liberal state from socialism and to reassert its authority in a national revival. Everyone lost.

PAUL J. DEVENDITTIS

Nassau Community College

GUIDO QUAZZA *et al.* *Fascismo e società italiana*. Edited by GUIDO QUAZZA. (Piccola Biblioteca Einaudi, 200.) Turin: Giulio Einaudi Editore. 1973. Pp. 253. L. 1,600.

Recently many scholars have questioned whether, in fact, the fascist period formed a historical "parenthesis" or was basically continuous with what came before and after. The present volume, a collection of papers given at the 1972 Seminar of Contemporary History under the auspices of the University of Turin, is an important contribution to this debate. Its main focus is upon links between fascism and Italian society and especially several components of that society: big business, the army, the judiciary, the Church, and Italian cultural life.

Two initial essays by Guido Quazza and Valerio Castronovo seek to provide a groundwork for re-evaluating Italian fascism. Both deal with the economic underpinnings of the regime, and both contend that fascism was essentially a stalking horse for big business. This, of course, is a canon of Marxist interpretation; what is new is the degree to which the present authors downplay the lower middle class and agrarian origins of the movement and its capacity for autonomous initiative. Indeed, Mussolini as a historical actor and fascism as an independent force have little place in these pages. The outlines of the fascist state, the authors contend,

date basically from prefascist liberal Italy, and while fascism changed the political guard, still "the old hierarchy of economic power remained firmly entrenched, [and] the authoritarian nature of the 'system' [also] remained" (p. 12).

Although these essays make an interesting prima-facie case for the continuing political strength of important economic groups, they do not define such groups adequately, falling back on oversimplifications about "dominant classes" and the "big bourgeoisie"; moreover, they lack a well-stated model of Italian social structure (indeed, a shortcoming of the entire volume); and, one must note, they are written in a prolix style. However, Quazza convincingly questions the myth of the Resistance as a united movement of national purification, arguing that it provided a means for traditional groups and fascist collaborators to re-establish themselves in postwar Italy. Castronovo ably discusses the economic alliance of state, industry, and banking under fascism and its continuation after 1945.

More limited in scope, but ultimately more satisfying, are the other essays in this volume. Essentially they analyze several institutions whose basic outlines remained largely unaltered during the fascist era, and they explore various modes of institutional continuity. For example, Giorgio Rochat argues in his excellent piece on the Italian military that in return for political support and connivance in fascist propaganda, the officer corps retained a wide degree of autonomy and preserved its role as final arbiter of political life. Similarly, Guido Neppi Modona contends that the judiciary continued its previous relationship with the state: subservience in policy matters and an independent hierarchical structure. The Catholic Church, according to the provocative contribution of Giovanni Miccoli, freely supported the regime, seeing fascism as a step toward a conservative theocratic order. In return the Church gained concessions in civil matters and retained the Catholic Action movement as insurance in the event of a fascist collapse. A concluding essay by Norberto Bobbio deals with academic culture and contends that it maintained its basic outlines in still another way: the regime, little fearing university intellectuals, required *pro forma* cooperation but not strict discipline.

These essays focus upon institutions that pre-

served continuity and remained to varying degrees outside the regime. But what of those that could not? A different story might be told of trade unions, political parties, and the media. In short, it is far too simple to treat the fascist period as essentially continuous with the liberal era before and the republican era afterward. Fascism, in fact, made a real difference in many areas of Italian life, and we need a history that deals with the regime both in its singularity and its relationship with the Italian past and present.

DONALD H. BELL

Massachusetts Institute of Technology

EDWARD R. TANNENBAUM. *The Fascist Experience: Italian Society and Culture, 1922-1945*. New York: Basic Books. 1972. Pp. vi, 357. \$12.50.

GLEN ST. J. BARCLAY. *The Rise and Fall of the New Roman Empire: Italy's Bid for World Power, 1890-1943*. New York: St. Martin's Press. 1973. Pp. 210. \$8.95.

In his study of the fascist experience Dr. Tannenbaum uncovers the roots of the totalitarian movement and traces its ramifications in every field. He notes that "any other regime would have been preferable" to fascism and that the "most sensitive thinkers and artists" were alienated by its "repressive tolerance." Although the author strives to give the fascist devil its due, his painstaking analysis of educational reforms, cultural activities, and the mass media tends to dwell on the negative side of the ledger. One would wish he had given more consideration to the opening of new national parks, the archeological and restoration work, the trend-setting Venice film festival, the *Maggio Fiorentino*, and the Vivaldi and Monteverdi revivals.

The author arrays massive documentation on the regime's social legislation, the role of fascist unions, and the poor rate of growth of the gross national product. He charges that wages were kept low while big business profited from the policy of autarchy, despite Mussolini's assertion that the fascist economy was "more concerned with the public interest than with private profit." The author's evaluation of fascist economic policies does not give sufficient weight to the saving of the tottering lira from the fate

that befell the Weimar mark, the establishment of thriving new rural centers in Southern Italy and Libya, the drive to wipe out the Sicilian *latifondo*, the impact of United States immigration quotas, and the role played by Britain and France's preferential tariffs in providing a rationale for Mussolini's dirigisme. Tannenbaum's poor view of the fascist government's pioneer work in the development of the mixed economy is at variance with the findings of a number of economists and historians: Andrew Shonfield and Nigel Harris have noted that Britain, France, and other countries have adopted corporatist innovations while Roberto Ducci and Roland Sarti have said that the forced-draft industrialization plans of the late nineteen thirties built the foundations of Italy's postwar "economic miracle."

Tannenbaum gives credit to Mussolini's improvement of relations with the Vatican but also stresses the ambivalent attitude of the Catholic hierarchy and youth toward fascism. The author pays little heed to the charge by some Italian historians that the party's conservative leaders sabotaged Mussolini's social program. And yet, the hope that Mussolini would eventually overcome the conservative forces and bring about a drastic social revolution was one factor that led many young fascists to keep their faith in the movement, as shown by the writings of Enzo Pezzato, Carlo Ravasio, and other fascist militants. Wider treatment of their views would have helped strengthen the author's presentation of the fascist experience as seen by "die-hard fascists" as well as by the antifascists he considers "the true heroes of the Mussolini years." However, all in all, this and other disappointments are minor flaws in a work based on impressive documentation and written with uncommon felicity of style and sympathetic understanding of the Italian psyche.

Tannenbaum's study is well complemented by Professor Barclay's book on "the Second Roman Empire." Barclay focuses his research on diplomatic, political and military developments germane to an understanding of the factors that led to the initial success and the ultimate failure of fascism. He sets out to show that in the early twentieth century Italy provided a "consistently dynamic element in the development of Europe." He says that the last and least of the great powers, Italy "led the Con-

tinent in aviation and automotive technology for more than ten years, was the first to develop a truly modern navy and the first to use aircraft as weapons of war. Barclay presents valuable data on Italy's demographic, geopolitical, and strategic problems under Crispi, Giolitti, and Mussolini and gives vivid descriptions of the main military engagements, from the battle of Lissa to the fall of Sicily. He notes that the Italians' uphill fight in both world wars was because of the country's inadequate industrial structure and the ensuing weakness in armament, especially artillery and armor. And he says that, despite its handicaps, Italy made a most useful contribution to the Allies' victory in 1918 and in July 1942 was "one torpedo away from victory in the Mediterranean," thanks to the successes of its torpedo bombers and the sinking of two British battleships by Borghese's frogmen.

Barclay faults Britain and France for their "short-sighted" policy toward Italy in Africa and the Adriatic. The author asserts that Orlando was shabbily treated at Versailles and that Mussolini was given little support by France and Britain in his attempt to stop the *Anschluss* after the murder of Dollfuss. Barclay charges that Paris and London allowed the Four-Power Pact and the Stresa Agreement to come to naught and claims an accord was "perhaps still possible even in 1939 but only at a price the French refused to pay."

A severe critic of the foreign policies of France and Britain, Barclay is equally rigorous in his treatment of Italian imperialism—from the first beachhead in East Africa to the disastrous entanglement with the Third Reich. Although his revisionist approach is often controversial, he presents his case effectively and will help stimulate further challenges to the conventional assessment of a turbulent era.

A. GALATOLI LANDI  
University of Maryland,  
College Park

MATEI D. VLAD. *Colonizarea rurală în Țara Românească și Moldova (secolele XV–XVIII)* [Rural Colonization in Wallachia and Moldavia (15th–18th Centuries)]. Biblioteca istorică 37. Bucharest: Editura Academiei Republicii Socialiste România. 1973. Pp. 186. Lei 11.50.

In recent years Romanian historians have given

a great deal of attention to the social and economic history of their country. This study, which appears in the series *Biblioteca istorică* published under the auspices of the Academy of the Socialist Republic of Romania, examines the demographic and other changes resulting from the settlement or colonization of rural Wallachia and Moldavia in three stages from the fifteenth to the eighteenth century, that is, in the period of transition from a feudal natural economy to a money economy. Frequent incursions by Tatars and Ottomans and the wars fought on the soil of the Romanian principalities devastated numerous rural communities and killed or dispersed many of their inhabitants. The Romanian princes, the large landowners, and the Ottoman government favored colonization of the deserted lands for various reasons, not the least important being the desire to put the land back in use, increase production, and assure regular income and tax revenues.

As a result of the developments just mentioned, one finds in Moldavia and Wallachia three kinds of villages—conventual, manorial (seignorial), and princely—each of which the author describes. The author notes among other things that the colonized villages were open to anyone wishing to settle in them; consequently they acquired larger populations than the autochthonous villages had. The documents reveal basic differences in the fiscal regimes of the colonized villages and the older villages. The colons were exempted from many taxes and obligations that weighed heavily on the native peasantry. The colons were, however, supposed to pay "the princely or imperial tax," but they often managed to evade it. The princes also granted to the colonized villages freedom of speech and religion and a large degree of autonomy in the management of internal administrative and judicial affairs.

The author comments on the mobility of the population and the directions of the migratory movements, along with various social, political, and economic ramifications of such mobility. He observes with patriotic fervor that the largest number of colonists in the principalities hailed from Transylvania. This he considers very significant, serving as it did to increase the Romanian element in the two principalities. As the colonists and the autochthonous popula-

tions spoke the same language, shared the same faith and common customs, and influenced one another, they strengthened Romanian culture as well as their awareness of the common origin of the Romanians. The author notes that the landlords found it easier to bring Romanians from Transylvania into the principalities than foreigners.

Despite the author's effort to fit the subject under discussion into a preconceived Marxian mold, he has given us a good study of a complex problem, based on extensive use of documentary materials. The value of the book is enhanced by a seven-page discussion of sources and an equally valuable ten-page bibliography.

WAYNE S. VUCINICH  
Stanford University

A. I. BABII. *Formirovanie moldavskoi intelligentsii vo vtoroi polovine XIX—nachale XX v.* [The Formation of the Moldavian Intelligentsia in the Second Half of the 19th and Beginning of the 20th Century]. (Akademiia Nauk Moldavskoi SSR, Institut Istorii.) Kishinev: Izdatel'stvo "Shtiintsa." 1971. Pp. 105.

A. I. Babii's study on the formation of the Moldavian intelligentsia is an important document. Its primary aim is to prove, through extensive use of raw statistical data, the backwardness of the Bessarabian population between 1861 and 1917. A corollary aim is to demonstrate that retardation was particularly manifest among the Romanian-speaking population.

Babii has no difficulty in proving his essential contention that the intellectual leadership in tsarist Bessarabia and by extension also in contemporary Soviet Moldavia always belonged to nationalities other than the Romanian. An impressive list of 657 intellectuals who attended institutions of higher education between 1861 and 1917 contains very few Romanian names; the majority of the patronymics are in fact Ukrainian. The list also reveals that most Moldavian intellectuals specialized in law or in medicine and that they received their advanced degrees primarily in St. Petersburg or in Moscow.

The appearance of this volume in 1971, at the height of Soviet-Romanian polemics over the national character of Soviet Moldavia, does not detract from its value as a statistical compendium. It does, however, point out the es-

sential character and purpose of scholarship in Soviet Moldavia. Denigration of the tsarist regime's educational and social policies and, by inference, those of the Romanian monarchic regime of the interwar years is not conclusive evidence for the contentions expressed in the preface to the effect that the USSR alone was able to solve the problems of cultural retardation in the various socialist republics. Nor can this method and the historical data, whose avowed purpose is to identify intellectual primacy in Moldavia with Ukrainians and Russians, negate the contributions made by Romanian and Jewish intellectuals in the twentieth century merely by assigning them the status of "nonpersons" in Soviet Moldavia.

Babii's volume is representative of the best values of contemporary Moldavian historical scholarship.

STEPHEN FISCHER-GALAȚI  
University of Colorado,  
Boulder

A. I. PUSHKASH *et al.*, editors. *Istoriia Vengarii* [History of Hungary]. Volume 3. (Akademiia Nauk SSSR, Institut slavianovedeniia i balkanistiki.) Moscow: Izdatel'stvo "Nauka." 1972. Pp. 965.

Hungary in the period after World War I must certainly qualify as the classic land of revolution and counterrevolution. This third volume of a Russian language study of Hungarian history in its most recent period, put together by a team of Hungarian historians under joint Hungarian and Russian auspices, is a massive undertaking, almost a thousand pages in length, that ponders the ebb and flow of revolutionary upheaval and counterrevolutionary reaction in a turbulent period. The methodological approach is a traditionally Soviet-Marxist one. Its preoccupation with political events and their interpretation is relieved by several chapters on art, music, and literature. Of the two most interesting sections, the first, the 1917-20 Revolutionary era, begins with the return from Russian prisoner of war camps of the Hungarian revolutionaries Bela Kun and Tibor Szamuely, with the blessings of Lenin, to incite revolution in their native land. The struggle to maintain the fragile Hungarian Soviet Republic against the counterrevolutionary onslaught of Czechoslovak, Romanian, and Hor-

thyite forces and the pressures of the Entente is recounted in detail. All this has been absorbed into the historical blood stream, and the conclusions can be accepted by most historians with a certain equanimity.

The section dealing with the 1956 uprising is another matter and raises large emotional and ideological questions about the meaning of that episode—as between conservatives, liberals and leftists—but even larger questions that have bedeviled and fragmented the international Left ever since. Was the Hungarian Revolution of 1956 really a counterrevolution as this study assumes—a suggestion that must boggle the Western bourgeois mind with its standardized view of Hungarian freedom fighters struggling against the Communist bad guys? Was it Imre Nagy's intent to turn back the clock and return Hungary to capitalism and the odious prewar regime, as had been done after the destruction of the 1919 Hungarian Soviet Republic? Even Tito, an enemy of Stalinism and the previous bureaucratic Communist regime in Hungary, found that Nagy's revisionism had gone too far and had become counterrevolutionary. Interestingly enough this study does not scruple to lay blame for the uprising upon bureaucratic and dogmatic errors committed by Matyas Rakosi and Enre Geröe, the pre-1956 leaders, but Imre Nagy is equally castigated for slipping the knot of economic and social controls—the breakup of the cooperatives and handing back of land to the former owners, curtailment of investment in industry, etc.—that resulted in the stagnation and disintegration that precipitated the revolt. All this, of course, points up the absence of anything like the New Left revisionist assessment of World War II and its aftermath going on among our American history colleagues and the prevailing grip of cold war interpretations upon contemporary East European history.

PAUL N. HEHN  
State University of New York,  
College at Brockport

I. M. MESHKO. *Hromads'ko-politychnyi rukh i suspil'na dumka v Uhors'kchyni na rubezhi XVIII-XIX st.* [The Public-Political Movement and Social Thought in Hungary at the Turn of the XVIII-XIX Centuries]. Lvov: Vydavnytstvo L'vivs'koho Universytetu. 1971. Pp. 188.

This monograph seeks to explain from Marxist-Leninist perspectives the birth of class struggle and revolutionary thought in Hungary from 1794 to 1830. Since it is based primarily on previously published material (mostly Hungarian) it provides no startling revelations. The volume opens with a brief review of socioeconomic conditions that prevailed in Hungary at the time, citing selected examples on the growth and operation of large estates, burdens of serfdom, and the discontent among petty nobles and intellectuals. Meshko attributes the rise of the latter phenomenon partly to the "bankrupt" policies of the Vienna government and partly to the *Weltanschauung*, economic power, and arbitrary behavior of Magyar aristocracy.

The remaining four chapters discuss ideas of selected radical members of the Magyar nobility, whom Meshko identifies as "bourgeois revolutionaries and progressive thinkers." Heading the list is Miklós Skerlecz (1731-99), an outspoken critic of the nobility's privileges and a forceful advocate of protectionism in order to create "a single Hungarian market." He is followed by Gergely Berzevichy (1763-1822), Hungary's dedicated follower of Quesnay, Turgot, and Adam Smith; István Széchenyi (1791-1860), an aristocrat turned "vulgar economist," a critic of feudalism, and a bitter opponent of Austrian colonial policy in Hungary; and Ignác Martinovics (1755-95), Hungary's most outstanding Jacobin and the hero of the volume. Meshko traces Martinovics's career and carefully reconstructs his revolutionary philosophy, emphasizing his admiration of the French, his contempt for the Austrians, his criticism of social and economic inequities in Hungary, his trial and execution, and his subsequent treatment by Hungarian revolutionaries and scholars.

For obvious reasons Meshko condemns the treatment Vienna authorities meted out to these "bourgeois revolutionaries" and calls it the clearest evidence of "political bankruptcy of the old regime." In some ways this is an appropriate characterization, but in fairness to these authorities it should be pointed out that they had no monopoly on political bankruptcy.

BASIL DMYTRYSHYN  
Portland State University

ILIE CORFUS. *Agricultura Țării Românești în prima jumătate a secolului al XIX-lea* [Agriculture in Muntenia in the First Half of the 19th Century]. (Academia Republicii Socialiste România, Institutul de Istoria "N. Iorga." Biblioteca istorică, number 21.) [Bucharest:] Academia. 1969. Pp. 403. Lei 20.50.

This book is a careful discussion of three key aspects of the agrarian history of Romania in the first half of the nineteenth century. These problems are: first, the evolution of relations between landholders and their dependent peasantry and its ramifications; second, agricultural production; and third, the economic and social transformation of the Romanian village. Much previous treatment of these questions, as the author shows in his useful historiographical introduction, was conjecture and analysis that lacked the boring but fundamental archival investigations necessary. What Professor Corfus has done is to take an enormous amount of his own painstaking archival work and produce a well-organized volume that results in a considerable revision of previous findings, both Marxist and non-Marxist. At the same time he provides a wealth of description and information including eighteen tables of statistics.

Among the most significant of the conclusions that Corfus suggests are, first, that there was no development of the so-called "second serfdom." This was because of the successful resistance of the peasantry to persistent legislative attempts to restrict their rights and privileges. Second, the small peasant producer was the principal source of agricultural production for commercial purposes. Third, the evolution of peasant production, coupled with the ascendancy of winter over spring wheat, led to an immense expansion of agricultural production between 1831 and 1848. And fourth, the peasant was not free, however, from exploitation because predatory merchants utilizing loans on future production during drought and other hard periods drove him to the brink of ruin. The intervention of the state to prevent fraud put an end to these practices.

The author's hopes that his work will both correct previous misconceptions and open the way to future studies on this important subject are well served by this book.

PAUL E. MICHELSON  
Indiana University,  
Bloomington

K. A. POGLUBKO. *Ocherki istorii bolgaro-rosiiskikh revoliutsionnykh svyazei (60-70-e gody XIX veka)* [Studies in the History of Bulgarian-Russian Revolutionary Connections in the 1860s and 1870s]. (Akademiia Nauk Moldavskoi SSR, Institut Istarii.) Kishinev: Izdatel'stvo "Shtiintsa." 1972. Pp. 309.

With more methodological sophistication Poglubko's study could have been made into an important book. Despite its lack of a systematic quantitative analysis, however, it enables the careful reader to visualize and chart the Russo-Bulgarian revolutionary paths of the 1860s and 1870s along the routes of long-distance trade—the Mediterranean, Danube, and Dnieper water routes, and Europe's new railway system. Bulgarian students and revolutionaries were thus concentrated in Kherson, Kiev, and Smolensk along the Dnieper; in Nikolaev and Odessa on the Black Sea; at the Danube's mouths (Izmail, Tulcea, Galați, and Brăila); and in neighboring Jassy, Kishinev, Komrat, and Bucharest. They were likewise present in Kharkov, Moscow, and St. Petersburg, and they were in contact with Russian revolutionaries in Istanbul, Athens, Corfu, Trieste, Zurich, Geneva, Paris, and London.

In Russia they were ultimately most numerous in Odessa, where they rose from 20 students on the eve of the Crimean War to 233 male and 24 female students on the eve of the Russo-Turkish War of 1877-78. In Kiev there were more than 30 Bulgarian students already in the late 1840s, and in Nikolaev there were 14 Bulgarian scholarship holders in 1875. In Moscow, on the other hand, there were only 11 young Bulgarians in 1857, 19 in 1860, and only a few more in the 1860s.

Bulgarian students and revolutionaries were numerous, too, in Moldavia, Wallachia, and Bessarabia. Bucharest, the Romanian and Bessarabian Black Sea ports, and Ottoman Istanbul duplicated Odessa's function of re-exporting revolutionary propaganda eastward and northward to Russia and westward to Europe.

Prior to the Polish insurrection of 1863 Slavophilism was the prevalent ideology among Bulgarian students. The subsequent radicalization, culminating in the formation in 1869 of the Young Bulgaria group of associations, promoted a shift to liberalism, populism, and anarchism, and an appreciation of the teachings of Michael Bakunin, Nicholas Ogarev, Serge

Nechaev, and of Peter Lavrov's London *Vpered*. It was also a stimulus to the idea of peasant revolution.

The uprisings of 1875 and April 1876 in Hercegovina and Bulgaria redoubled the aspiration of Russian radicals to diffuse peasant revolution across the Balkans to the Ukraine by way of the multitudinous fairs and to link it on the one hand with the national liberation movements and on the other with the Russian anarcho-populist and Paris commune ideologies. It is well known, indeed, that an Odessa revolutionary group organized the insurrection of the village of Korsun in Kiev province in the spring of 1876 and succored the *druzhina* movement of Ukrainian peasant bands by supplying them, during 1876 and 1877, with spurious imperial manifestoes calling for the dispossession of the great landowners. Nationalism, political conservatism, and technological change conspired, however, to set limits to the old radicalism and pave the way for a new one.

TRAIAN STOIANOVICH  
Rutgers University,  
New Brunswick

NORMAN DAVIES. *White Eagle, Red Star: The Polish-Soviet War, 1919-20*. Foreword by A. J. P. TAYLOR. New York: St. Martin's Press. 1972. Pp. xviii, 19-318. \$10.00.

The Soviet-Polish conflict of 1919-20 has long been a neglected or misinterpreted topic in English-language historical literature. Within the last four years three books appeared in this country largely devoted to the study of its political and diplomatic aspects: M. K. Dziewanowski, *Pilsudski: A European Federalist*; P. S. Wandycz, *Soviet-Polish Relations 1917-1921*; and R. H. Ullman, *The Anglo-Soviet Accord*. There was need for a good military history of the war, and a young British historian, Norman Davies, undertook to fill this gap. His vividly written account ought to appeal to the general reader and, with some minor reservations, to the specialist of the period.

Davies's forte is his narrative, which makes the conflict come alive. Making good use of contemporary stories, notably Issak Babel's recollections about the *Budyenny Konarmiia*, and such important new material as the directives of the Red Army high command and unpublished British documents, the author pre-



sents a good picture of the conflict. His interpretation of such controversial issues as the origins of the war, the battle of Warsaw, and Allied aid or lack of it to Poland, carries conviction and disposes of some of the legends still persistently repeated by uninformed or prejudiced historians. Stressing that the Soviet-Polish War was not the "third campaign of the Entente" (a description originally invented by Stalin), but largely an independent Polish venture, Davies rightly castigates some British writers: "In 1919, when the Polish-Soviet War was vital only to Poland, they pretend it did not really exist, in 1920 when it became vital to Russia also, they suddenly discover an 'outbreak.'"

For all the merits of the book, and it has many, one cannot ignore its weaknesses. It is strange indeed that Davies, concentrating on the military aspects of the war, ignored the chief Polish archive—also available in London on microfilm—which contains the papers of the high command (over thirty-seven thousand pages) for the years 1918–22. Other omissions dwindle in comparison to this big gap. There are minor, although occasionally irritating, errors in this book: slips, odd transliterations of some Russian names, inaccuracies. The handling of Petliura and the Ukrainians is superficial and somewhat unfair. Although most of these errors may be ascribed to carelessness there are some that make me wonder whether Davies has an unerring grasp of the nuances of Polish politics.

It may be that Davies writes too easily and tends to sacrifice accuracy of detail for the sake of turning a good phrase. His inclination to affix labels to chief characters of his story makes for amusing reading, but raises doubts about the depth of his analysis. Piłsudski resembles a "rhinoceros—indestructible, myopic, unpredictable"; Petliura looks "the perfect image of a rising corporal"; Daszyński has "renounced his aristocratic title" [sic]; General Henrys is supposedly viewed by Poles as a "feather-brained busybody." These nonchalant mannerisms might put off a serious historian and damage the value of a good, convincing, and much needed work on the Polish-Soviet War. I, for one, would have preferred a less jazzy approach, but this is a matter of taste. There is no doubt that Davies's study deserves to be read carefully

and will profit those who wish to get an up-to-date and generally objective view of this important war.

PIOTR S. WANDYCZ  
Yale University

HERBERT S. LEVINE. *Hitler's Free City: A History of the Nazi Party in Danzig, 1925–39*. Chicago: University of Chicago Press. 1973. Pp. xii, 223. \$7.50.

When the National Socialists came to power in the Free City of Danzig in 1933, shortly after they had in Germany, they reversed a trend. Until then Danzig politics had mirrored every turning in German politics; now the new Nazi government was unable to transform the Free City into a miniature Third Reich. Unlike the Nazis in Germany, who quickly established one-party rule, the Nazis in Danzig were obliged to remain a "fighting party" and contend with a fairly vigorous political opposition until 1937. The non-Nazis made common cause against National Socialism at the polls; the non-Nazi press risked confiscation and proscription to attack the government; and Catholics resisted the subversion of their Church and their organizations. This opposition was possible because Danzig's status as a free city made local politics a matter of international concern. The League of Nations, represented by a resident high commissioner, guaranteed the city's constitution and its territorial integrity. And it was only because the League—ultimately, of course, Britain and France—failed to exercise its responsibilities, leaving Danzig an object of direct German-Polish agreement, that the opposition failed to prevent a Nazi triumph.

Levine reconstructs the slow nazification of Danzig with great skill. He shows that the Danzig Nazis encountered obstacles unknown to the German Nazis, and that they were forced to adjust their sights and modify their practices. He describes the personalities and rivalries of the leading Danzig Nazis (Forster, Greiser, Rauschning) and assesses the rather ambiguous role of Carl J. Burckhardt, the last high commissioner. Throughout, he places his observations in a larger context—Berlin's control of the local Nazi party, the Free City's inevitable dependence on Germany, the decline of the League powers. The book is further evidence that

nazism was no monolithic or totalitarian system. And while it confirms many of the conclusions of other local studies of Nazi rule (W. S. Allen, E. N. Peterson), it differs from these and adds another dimension because it treats nazism outside territorial Germany.

Levine's scholarship is of high quality. He has made good use of the rich archival holdings in Germany, London, Geneva, and Gdansk. He has interviewed or corresponded with participants in the events he recounts. He argues cogently and writes cleanly and economically. His note on source materials includes useful information on the archives he consulted.

CHRISTOPH M. KIMMICH  
Brooklyn College,  
City University of New York

DOMINIQUE EUDES. *The Kapetanos: Partisans and Civil War in Greece, 1943-1949*. Translated from the French by JOHN HOWE. New York: Monthly Review Press. 1972. Pp. 381. \$11.50.

Dominique Eudes is a young Frenchman who has written several film scripts for French and American television. This is his first book, an attempt to explain why the National Liberation Front (EAM) failed to transform the Greek resistance during World War II into a successful revolutionary movement. The book creates a mood that is reminiscent of Marcel Ophüls's brilliant movie, *The Sorrow and the Pity*.

The book contains three premises: (1) The British were determined to destroy the EAM in order to re-establish a postwar position of influence; (2) The Greek politicians of the prewar order realized that the British could not succeed without restoring them, and in this equation the Germans were only a transitory enemy; (3) Nevertheless, and ironically, the EAM was destroyed not by its enemies, Germans, British, or Greeks, but by its own leadership, the Central Committee of the Greek Communist party (KKE).

The author's main argument is that the phenomenal growth of the EAM and its Peoples Army (ELAS) was beyond what the KKE could handle. The ELAS was a spontaneous peasant army of perhaps 40,000 regulars. It was organized in the mountains and led by a warrior breed of *Kapetanos*, the most notable of which

was the legendary Aris Velouchiotis. This being the demographic reality of the resistance, the *Kapetanos* wanted to develop a peasant-based revolutionary consciousness. Further, the *Kapetanos* wanted to eliminate or incorporate the other resistance bands—which were not comparable to the ELAS—in order to make the resistance more effective from a military point of view and in order to prevent the British Military Mission from using the bands as a Trojan Horse. But the Central Committee consisted of a handful of doctrinaire Stalinists who feared a peasant revolutionary base as a threat to the party. Instead, they looked forward to the development of a revolutionary proletariat after the war. Consequently, they designed a strategy of cooperation based on patriotism and a political line of legalism that gave the British the opportunity to destroy the EAM in December 1944. The events of the civil war, 1946-49, are described according to the same formula with the difference that the Americans, under the Truman Doctrine, replace the British.

This is a valuable book because it gives a rare description of how decisions were made inside the EAM by quoting heavily from survivors. Paradoxically, this is also the book's weakness. Eudes seldom identifies his sources for fear of placing people in jeopardy, a concern that is very understandable. But even with this restriction documentation of this type has been all too rare. The book fills an important void and must be strongly recommended. In fact, *The Kapetanos* and John Iatrides's recent *Revolt in Athens* (1972) together represent a significant advancement in the study of the Greek resistance. The original French edition of *The Kapetanos* (1970) is well written, and John Howe's fine translation has preserved that quality.

HARRY C. CLIADAKIS  
University of South Carolina

A. A. GUBER *et al.*, editors. *Feodal'naiia Rossiia vo vsemirno-istoricheskoi protsesse: Sbornik statei, posviashchennyi L. V. Cherepninu* [Feudal Russia in the Process of World History: A Collection of Essays Dedicated to L. V. Cherepnin]. (Akademiia Nauk SSSR, Institut Istorii SSSR.) Moscow: Izdatel'stvo "Nauka." 1972. Pp. 438.

This *Festschrift*, dedicated to the productive

Soviet medievalist and "scholar-patriot," L. V. Cherepnin, contains forty-five brief studies of Russia and its relations with other states and peoples from the ninth to the mid-nineteenth century. The stated objective of the volume is to explore through the comparative historical method the place of Russia in world history, and the editors, at least, express satisfaction with the work, claiming that it demonstrates "the prominent role of Russia in history." Forty-five essays dealing with Russia's past could hardly fail to do otherwise.

Only a few of the essays could be considered examples of comparative history, if by that term one means the systematic analysis of historical phenomena in two or more societies with the goal of discovering parallel or contrasting themes from which useful generalizations might emerge. The most serious effort at comparative study comes from V. D. Nazarov, who examines the leadership of the Bolotnikov uprising of 1606-07 and contrasts it with the leadership of peasant wars in Central and Western Europe. But this effort is badly flawed by a total reliance on Marx and Engels for its analytical framework and its interpretation of events in Western Europe.

The majority of the essays deal with commercial, intellectual, and diplomatic contacts of Russia with foreign states. In them the focus is narrow: a specific event, the contribution of a new document, a historiographic review, a study of select features of an institution. Overall, it is not an outstanding collection of essays, but there are several solid pieces of scholarship: Pavlova-Silvanskaia on foreign loans in late eighteenth-century Russia, Kazakova on certain sources of the ideas of the nonpossessors, Khashanov on the procedural rules governing the drafting of treaties between Rus' and Byzantium, Florovsky on the efforts of Austria to avoid recognition of the newly proclaimed Russian empire, and Troitskii on the social composition of the Russian diplomatic corps in the mid-eighteenth century. Precise contributions to our knowledge of specific problems contained in these and other essays constitute the main value of the work. Students of medieval Russia will find particularly helpful the complete bibliography of Cherepnin's work.

Those who look to this volume for new interpretations arising from the comparative historical approach are certain to be disappointed.

The very brevity of the articles precludes sophisticated analysis of major themes, but beyond that, most authors appear uncomfortable with the comparative approach. They use foreign scholarship gingerly—in some essays only Russian works appear in the notes, although the nature of the topics clearly calls for consultation of foreign scholarship. They frequently fall back in their concluding paragraphs on the Marxist tag-endings so typical of Soviet scholarship but scarcely relevant to the content of the essays. More often than not, judgments of a comparative kind have been replaced by defensive statements about the merits of Russian culture and about the extent to which Russia's experience has conformed to the "lawfully regulated" (*zakonomerno*) historical process experienced by other countries, particularly those of Western Europe. The battle against bourgeois historiography—the code name for scholarship done outside the friendly socialist nations, and in this volume most often the work of West German scholars—is a persistent theme, leading on occasion to chauvinistic excess. Kiev Rus', for example, becomes in the article by V. T. Pashuto, a nation-state whose coherent foreign policy assisted in "the struggle of the peoples [of Europe] against Arab, Byzantine, and German dominion," at the same time that it fended off attacks from the Turkic steppe nomads. But this didactic service of Russia's cultural heritage should come as no surprise in a book whose dedicatory preface hails the work of Professor Cherepnin as "a noble example of the patriotic service of Soviet science."

J. MICHAEL HITTLE  
Lawrence University

IAN GREY. *Boris Godunov: The Tragic Tsar*. New York: Charles Scribner's Sons. 1973. Pp. 188. \$8.95.

Ian Grey is well known for his serious and generally well-researched popularizations of Russian history. His *Boris Godunov* is a work in the same tradition, one that on first acquaintance makes a favorable impression for its clarity and declared aim to avoid some of the romantic excesses of those who have portrayed the subject in the past (notably Karamzin, Pushkin, and Musorgski).

The reader will not find in *Boris Godunov*

solid biography, however, since sources for Muscovite history are such that even with proper examination of all of them the historian is generally unable to provide a full portrait of even the most important individuals. A good portion of Mr. Grey's book deals with events that occurred prior to the advent of Boris around 1570—material that the author has covered in previous books but apparently not brought up to date for this volume. Even when the author gets into the career of Boris, he should perhaps have been more cautious than he is in relating what "Boris did."

In examining a new popular history of Tsar Boris, one thinks immediately of earlier such popularizations, in particular the classic by the eminent scholar S. F. Platonov, published in Russian in 1921 (as *Boris Godunov*) and recently translated into English. Where Platonov is sober and judicious, Grey remains too attracted to Karamzin's colorful but not necessarily historical elaboration. But this is not to say that Grey ignores Platonov's work, which he cites in a number of places. In fact, in many instances Grey relies very heavily on Platonov, structuring whole paragraphs after the latter's paraphrasing, and teetering on the edge of something much less forgivable, often without giving adequate credit. While there is not room here to provide parallel texts, one may take as examples the first full paragraph on page 83 in Grey's book, which is little other than a quotation from Platonov (1921, p. 24), and this is followed by a long paraphrase from the same; Grey, beginning at the bottom of page 86 (two sentences), is almost exactly the same as Platonov (p. 25); Grey's sentence on page 88, "Dionysii found himself in a false position," is surely from Platonov, "Dionisii . . . okazalsia v lozhnom polozhenii" (p. 29); Grey's page 108 is cut and spliced from Platonov, pages 60-61; and so on.

The general reader may find such questions of little consequence—Mr. Grey's account is, after all, a readable tale, a bit more accurate than a lot of the popular rubbish one can find on Russian history. The scholar will await Professor R. G. Skrynnikov's full-scale study of Muscovy in the time of Boris Godunov soon to be published in Leningrad.

DANIEL CLARKE WAUGH  
University of Washington

I. S. SHARKOVA and A. D. LIUBLINSKAIA, editors. *Russkii diplomat vo Frantsii (Zapiski Andreia Matveeva)* [A Russian Diplomat in France (The Notes of Andrei Matveev)]. (Akademiia Nauk SSSR, Institut Istorii SSSR, Leningradskoe Otdelenie.) Leningrad: Izdatel'stvo "Nauka." 1972. Pp. 295.

Andrei Matveev (1666-1728) was a co-worker of Peter the Great and son of the Westernized statesman Artamon Sergeevich Matveev. Exposure to the ways and learning of the West enabled the younger Matveev to serve his country as a diplomat. As Russian ambassador to Holland from 1699 to 1712, he utilized Dutch and English concern over the War of the Spanish Succession to prevent them from aiding Sweden in the Northern War with Russia. Later, while also ambassador to London (1706-08), Andrei Matveev attempted unsuccessfully to persuade England to act as an intermediary in peace negotiations between Russia and Sweden. His most ambitious project, however, came in 1705, when he tried to lead France (allied with Sweden in the Spanish war) to intervene in Russia's favor to secure peace in the north. To this end Matveev undertook a secret embassy to Louis XIV in September 1705. Matveev did not attain his peace objectives, but his discussions led to improved commercial contacts between Russia and France in the next two decades.

This volume contains the diary Andrei Matveev kept in France from September 5 to November 10, 1705. It has extensive comments on the king, his court and army, the parlement of Paris, the French Academy, the nation's administrative system, and the sights, roads, and historical monuments of France and Western Europe. This is valuable information and all the more usable because of a large introductory essay, extensive notes, a glossary, name and geographical indexes, and several well-selected plates. What a pity, then, that so few Western students of French history read Russian. In the West this book will be of interest only to a few Russian historians ready to discover in Andrei Matveev yet another Russian more learned and observant than prevailing norms prepare us to expect. One trusts the labors of Mdms. Sharkova and Liublinskaia will be more appreciated at home, where this edition is available in 23,500 copies. They might next consider publishing Andrei Matveev's numer-

ous diplomatic reports from 1700–15, preserved in TsGADA (Tsentral'nyi Gosudarstvennyi Arkhiv Drevnikh Aktov SSSR), or his account of the *streltsy* uprising in 1682 that took his father's life.

JOSEPH T. FUHRMANN  
Tusculum College

L. I. NASONKINA. *Moskovskii universitet posle vosstaniia dekabristov* [Moscow University after the Decembrist Uprising]. Moscow: Izdatel'stvo Moskovskogo Universiteta. 1972. Pp. 341.

The reign of Nicholas I is infamous as the apogee of the Russian autocracy. It opened with the persecution of the Decembrists, the creation of the dreaded Third Section, the drafting of the reactionary Educational Rescript of 1828, and the suppression of Polish freedoms after the rebellion of 1830–31, all of which represented a fundamental conservative reconstruction. Out of regimentation, repression, and censorship eventually arose the vanguard of the new order: the circles of Stankevich and Herzen, which were both the first flowering of the intelligentsia and the models for conspiratorial action in Russia for subsequent generations.

Nasonkina's exciting study focuses on this initial period of Nicholevan repression between 1825 and 1831 and on the supposed hiatus in revolutionary activity. Her careful examination of the archives of Moscow University and of police archives dealing with its students reveals that conspiracy and antigovernmental activity at the university was continuous after 1825 and that Herzen's circle was only the best known of a long series of student protest groups.

In an initial chapter Nasonkina discusses the administrative and pedagogical structure of Moscow University and its faculty. She finds that many faculty members and administrators were competent scholars who were committed to their teaching and to community service. They were protectionist in their attitudes toward student rebels, frequently resisting governmental pressures on them. The remaining two-thirds of the book tells the story of student protests. A high point occurred in the spring of 1831 when a food strike rooted in cholera-control measures, the harrassment of the incompetent Professor Malov by an organized

army of students, the appearance of the first truly political *kruzhki*, and the fantasies of N. P. Sungurov, who claimed that he represented a nationwide conspiracy of students, Polish officers, army troops, and factory laborers, all combined to throw a major scare into the government. Herzen and his friends had the misfortune of forming their circle just as the government was investigating these affairs.

This is superb educational history, rich in statistics, statutes, course descriptions, and student-faculty ratios. It is also excellent social history, which carefully notes the influence of the cholera epidemic, gives the social origins of each rebellious student, and even traces their lives into middle age when, like proto-Jerry Rubins, they embarked on respectable careers in medicine and law. It is finally a good intellectual history of the early intelligentsia, although Nasonkina slights foreign ideas and the non-Russians on the faculty, overemphasizes the modernity of Lomonosov's science in the 1820s, and gives herself an unfair advantage by consistently referring to the *iuridecheskoe otdelenie* of the university as *politicheskoe*. The book has a useful index.

This is, in short, an excellent study that makes delightful reading. It entirely supplants the educational histories of Nicholas Hans and Patrick Alston on the topic. Nasonkina's book belongs on every Russian historian's bookshelf along with the English-language studies by McGrew, Riasanovsky, Malia, Monas, and Squires, all of which it complements.

MAX J. OKENFUSS  
Washington University

PHILIP POMPER. *Peter Lavrov and the Russian Revolutionary Movement*. Chicago: University of Chicago Press. 1972. Pp. xix, 250. \$7.95.

There are no surprises in Professor Pomper's interpretations, based on extensive research both abroad and in the United States, of the career of the Russian revolutionary publicist Peter Lavrov (1823–1900). In this first book-length biography of the publicist the author has commendably accomplished his purpose of analyzing the character of Lavrov's thought and its relationship to his revolutionary career. The product of intellectual trends that influenced the young radicals of the 1840s, Lavrov became

prominent only in the 1860s after others of his generation had largely passed from the scene. His scholarly, relatively unimpassioned approach to issues and his rather sheltered life presented a striking contrast to the style of the committed young radicals whom he inspired but never led. As Pomper points out, it was largely by default that Lavrov became a major intellectual influence on Russian populism by publishing his *Historical Letters* (1869) at a crucial moment when the radical intelligentsia were receptive to an alternative to Pisarev's nihilism and Nechaev's conspiratorial duplicity. Lavrov's highly ethical philosophy was attractive to the radicals since they identified themselves with the critically thinking minority that was the dynamic factor basic to his doctrine of progress. Moreover, the young radicals were given a cause, that is, to fulfill their obligations to the mass of the people and to form a party of progress. Lavrov made it clear that, in that stage of history, the dynamic minority was in his view socialist and, in Russia, based on populist ideals. From 1870 to 1900 Pomper demonstrates the essential continuity of Lavrov's system of thought as he successively associated himself with the party he judged to be the revolutionary vanguard of the period. The high point of his career as a publicist for the cause came when he served as editor of *Vpered!* (1873-76).

Pomper places Lavrov's career in excellent perspective until 1877, when he begins to pass over the later years too rapidly. Little space in particular is given to the debates between Marxists and the *narodovoltsy*, for example, in 1883 when both parties sought to gain the prestigious support of the old man. Moreover, too little attention is devoted to the differences between Lavrov and N. K. Mikhailovskii, also a key publicist of populism in this period. Nonetheless, together with the translation of the *Historical Letters* by James P. Scanlan (1967), scholars now have at hand two solid works basic to the study of Lavrov's role in the Russian revolutionary movement. As Pomper notes, probably only a team of scholars could do full justice to Lavrov's entire range of scholarship in developing his anthropologism, ethical sociology, and history of thought.

JAMES A. DURAN, JR.  
Canisius College

REGINALD E. ZELNIK. *Labor and Society in Tsarist Russia: The Factory Workers of St. Petersburg, 1855-1870*. (Sponsored by the Russian Institute, Columbia University.) Stanford: Stanford University Press, 1971. Pp. viii, 450. \$15.00.

Reginald Zelnik's new book is rich, lucid, and judicious. He draws upon a wide range of published sources and, in the latter chapters, on extensive research in archives at Leningrad and Moscow. Anyone with a serious interest in Imperial Russia or labor history will want to read *Labor and Society*. Some readers may complain that it is longer than the material warrants, but they will find it rewarding throughout. Zelnik is open to reproach for a misleading title, however, while his broadest conclusion is open to question.

In essence this monograph is an extended preamble to a projected second volume covering labor in St. Petersburg in the 1870s. This first volume deals with the labor question, not with labor. Its real subjects are the journalists, commissions, police officials, statisticians, and physicians who articulated ideas and conclusions about industrial labor. Workers appear only through prisms held up by members of educated society; even in the last chapter, devoted to the strike at the Nevskii cotton-spinning factory in 1870, the trial of the strikers provides the substance, rubrics, and narrative structure.

Yet for the prepolitical phase of the Russian labor movement, any work of synthesis on "labor history" is necessarily a study of the labor question. It is easy to say that historians should praise E. P. Thompson less and imitate him more, but the historian of Russian labor does not have at his disposal a range of sources and secondary works comparable to the underpinnings of *The Making of the English Working Class* (1963). Knowing the material available as well as anyone does, Zelnik has chosen to focus on the labor question; other approaches are not feasible. At the same time, he does not struggle against the exigencies of his subject matter; he makes no major effort to evoke the quality of life in industrial St. Petersburg, while his discussion of wage rates and the level of subsistence is fairly casual.

*Labor and Society*, then, is a subtle and comprehensive study of a major aspect of Russian public opinion. Zelnik shows rare sensitivity to the parameters of public opinion between

1855 and 1870. In this era ambitious civil servants, heavy-handed policemen, and prudent journalists could still discuss labor legislation, working-class organizations, and even strikes on their abstract merits. These and other components of urban industrial life had not yet acquired labels, which no one could disregard, denoting the promise or peril they were believed to hold for social and political stability. Alexander II and his contemporaries tried to learn from West European experience, but the lessons were hard to apply to Russia. Even as modern industry was emerging in St. Petersburg, the labor question was still in a preindustrial phase. This being the case, Zelnik might have been more cautious in making a broad political inference from the state of public opinion. The period of his study, he finds, was "a sort of pristine moment in which the government could act constructively by lifting the barriers to the development of an independent working class." Confusion and wishful thinking played a role in the government's deliberations, as Zelnik shows very well, but was a real opportunity missed? The very instances on which Zelnik dwells, especially the suppression of the Sunday school movement and the Nevskii strike, indicate that the regime could be vaguely tolerant of workers' organizations only until they actually emerged in Russia.

DANIEL FIELD  
Barnard College,  
Columbia University

P. S. GUSIATNIKOV. *Revoliutsionnoe studencheskoe dvizhenie v Rossii, 1899-1907* [The Revolutionary Student Movement in Russia, 1899-1907]. Moscow: Izdatel'stvo "Mysl'" 1971. Pp. 262.

Soviet scholars have customarily either ignored the outburst of student protest at the turn of the twentieth century or dismissed it as a "liberal-bourgeois" development. Professor Gusiatsnikov's study is an attempt to give the student movement its due. He stresses the importance of the three nationwide student strikes in 1899, 1901, and 1902 not only in weakening the prestige of the autocracy but also in strengthening the workers' revolutionary zeal by showing them that they were not alone in their struggle

and by demonstrating the political effectiveness of strike action. Furthermore he points out how Lenin and the Bolsheviks encouraged and utilized the revolutionary student movement, and the author finishes with a detailed account of the students' role in the Revolution of 1905, especially their use of school buildings for revolutionary purposes and their brave deeds on the barricades. These activities, the author argues, should entitle the students to the "third place" in revolutionary significance, right behind the workers and peasants (p. 222). Although the book does not present sufficient evidence to support so sweeping a conclusion, it provides, based as it is to such a large extent on archival sources, a useful survey of radical student activity from 1899 to 1907.

In general the book is more valuable for the information it contains than for the analysis it offers. While Gusiatsnikov avoids the Soviet obsession with proletarian revolutionary elements and with the Bolshevik party, he rarely escapes from the traditional restrictions of Soviet scholarship. For example, the Leninist press is constantly used to the virtual exclusion of other contemporary periodical sources; the author's interpretations rarely go beyond Lenin's pronouncements on the problem, and the treatment of the tsarist government and of the rival opposition parties like the Kadets, Social Revolutionaries, and Mensheviks is at best simplistic, at worst no more than a caricature. Yet even within the confines of Soviet historiography Gusiatsnikov does not push his analysis very far. Despite the significance he attaches to the relationships between the worker and student movements, he hardly explores this relationship except to note the presence of workers at student demonstrations and meetings. Equally important, the nature and composition of the revolutionary student body are never fully investigated. The author carefully indicates the future Bolsheviks involved in the student movement, but he makes no attempt to classify the radical students according to social class and origins, faculty and year of study, or type of institution attended. As Gusiatsnikov himself admits, much work remains to be done before a comprehensive picture of student opposition forces during this period can emerge.

ALLEN SINEL  
University of British Columbia

EDWARD CHMIELEWSKI. *The Polish Question in the Russian State Duma*. Knoxville: University of Tennessee Press. 1970. Pp. vi, 187. \$7.50.

Professor Chmielewski has written the first English-language study of the Polish Question in the Russian Duma and the State Council. The book is based mainly, as the author admits (p. vi), on the stenographic reports of these two bodies. The period covered is 1906-14, so it does not include the work of the Fourth Duma beyond the outbreak of the First World War.

Students and scholars interested in Russo-Polish relations will be grateful to the author for this faithful record of debates on the vexed and complicated Polish Question in Russia. If Chmielewski's objective was merely to print the record in summary form, he has certainly attained it. If he intended to give an explanation in depth, however, it must regrettably be stated that his book fails to provide it. The background sketch of Polish-Russian relations is very brief and does not give the reader a grasp either of the attitudes of major Polish parties toward Russia or of the complex nuances of Russian liberal attitudes toward the Polish Question.

As far as parties in the Kingdom of Poland are concerned, it should be noted that Polish Socialists and Piłsudski in particular rejected any possibility of cooperation with Russia. In 1904, after the outbreak of the Russo-Japanese War, both Piłsudski and Dmowski traveled to Japan—the former to ask for Japanese aid for Polish armed resistance to Russia, the latter to dissuade Tokyo from any such idea. The swift end of the war put paid to these efforts. However, during the 1905 revolution in the kingdom, Socialists and National Democrats clashed openly, sometimes with loss of life.

In Russia itself the Polish Question was the most delicate of the whole explosive nationality problem of the empire and should be seen in this context. For most educated Russians Polish autonomy was only the thin end of the wedge to Polish independence that, in turn, would severely weaken the empire and signal the beginning of its demise. While the Cadets were willing to support it in principle as part of their liberal ideology, they refused to push it for fear of alienating public opinion. It is un-

likely, even if peace had continued for twenty years more, that Polish autonomy would have been granted, though there was a chance to obtain further reforms in the existing system. It was, in fact, impossible for Imperial Russia to solve her own Slavic problem (p. 174) without adopting genuine federalization. This was patently impossible as long as the major aim was centralization and assimilation. The same was true of the Polish problem in Germany. In both parliaments, it should be noted, the Poles often found support in the Conservatives, the Catholic Center party in Berlin, and the Octobrists in Moscow when the government proposed measures threatening conservative interests, for example, expropriation. Within this framework the logical allies of the Poles were the Conservatives, not the Liberals. Nonetheless, the Russian Liberals, for all their reservations and hesitations sympathized with and supported Polish demands.

It is surprising that the author dismisses Polish studies of the Polish Question in the Duma as "tendentious" (p. vi). Zygmunt Łukawski's work on the Polish Circle in the Russian Duma, 1906-09 (*AHR*, 73 [1968]: 1577-78) as well as Mirosław Wierzbowski's study of this subject in the Third and Fourth Dumas, which was published in 1966 and listed in the bibliography, are both based on solid research. One may disagree with their view that the National Democrats' "natural allies" were the Social Democrats in Russia, but their work cannot be dismissed as "tendentious."

In conclusion, we should be grateful to Chmielewski for presenting in English the record of Duma and State Council debates on the Polish Question from 1906-14. We still await a major study of the Polish Question in Russian politics in this period. Such a study is badly needed; it will illumine Russian attitudes toward Poland not only at this time but also in the years that followed.

ANNA M. CIENCIALA  
University of Kansas

M. SH. SHIGABUDINOV. *Rabochee dvizhenie na Severnom Kavkaze v gody novogo revoliutsionnogo pod'ema i pervoi mirovoi voiny (1910-fevral' 1917 gg.)* [The Workers' Movement in the Northern Caucasus during the New Revolutionary Enthusiasm and the First World War



(1910–February 1917)]. (Dagestanskii Filial Akademii Nauk SSSR, Institut Istorii, Iazyka i Literatury im. G. Tsadasy.) Makhachkala: [the Institut]. 1970. Pp. 270.

This is the first full study of the workers of the North Caucasus during the critical period between the Revolutions of 1905 and 1917. The area, embracing the Kuban, Terek, and Dagestan regions together with Stavropol and Chernomorskaia provinces, contained an extremely heterogeneous population—Georgians, Armenians, Azerbaijanis, Dagestanis, Russians, Ukrainians, Tatars, and a variety of Caucasian mountaineers—among whom national differences accentuated social and political discontents. Following accepted Soviet historiography, the author perceives a new revolutionary wave taking shape after 1910, with a rising number of strikes of an increasingly political character, and reaching its crest on the eve of the First World War. In profuse detail he describes the industrial development of the area, the activities of revolutionary groups, and the growth and social composition of the working class, buttressing his account with useful quantitative data and statistical tables. The result is a work of impressive scholarship that is based on a wide range of printed and archival sources, listed in the comprehensive bibliography at the end.

The book, however, has some serious deficiencies. There is a disproportionate emphasis on the role of the Bolsheviks to the neglect of the Mensheviks and other radical elements of whom we receive a distorted as well as an incomplete picture. City by city, district by district, Bolshevik groups are subjected to a minute examination, and lists are provided of their most active members, prominent among whom was Sergei Kirov, the future party secretary of Leningrad whose assassination in 1934 precipitated the Stalinist purges. All too often the book becomes a mechanical recital of facts, a catalog of numbers and names in a dreary encyclopedic style, with only brief interpretive sections tacked on at the end of each chapter. Yet, as a compendium of valuable information, it is a welcome contribution to the history of Russian labor in a remote but important region of the empire.

PAUL AVRICH  
Queens College,  
City University of New York

V. I. SELITSKII. *Massy v bor'be za rabochii kontrol' (Mart–iul' 1917 g.)* [The Masses in the Struggle for Workers' Control (March–July 1917)]. Moscow: Izdatel'stvo "Mysl'" 1971. Pp. 234.

T. A. IGNATENKO. *Sovetskaia istoriografiia rabochego kontroliia i natsionalizatsii promyshlennosti v SSSR (1917–1967 gg.)* [Soviet Historiography of Workers' Control and the Nationalization of Industry in the USSR (1917–1967)]. (Akademiia Nauk SSSR, Institut Istorii SSSR.) Moscow: Izdatel'stvo "Nauka." 1971. Pp. 257.

The term "workers' control," common among British guild socialists at the turn of the century, became popular in Russia only in 1917. Within days of the March Revolution it became both a broad slogan demanding higher wages and shorter hours and a more narrow call for worker participation in management: hiring and firing policies, work rules, working conditions, production line methods, and even financial matters. The phrase was especially popular among the factory committees, organized by workers themselves throughout the country. When Lenin endorsed workers' control in *Pravda* on May 17, he approved an independent mass movement that generally wanted employee supervision more than an anarchist takeover or managerial responsibility. After the Bolsheviks penetrated the factory committees and made their own revolution in November, Lenin amalgamated the committees as an All-Russian Council on Workers' Control; within a month it was subordinated to *Vesenka*, the Bolsheviks were supporting the trade unions, and workers' control survived only as a rallying cry of the left opposition in the early 1920s.

Between 1957 and 1968 the Leningrad historian V. I. Selitskii argued that workers' control in 1917 meant both general labor demands and specific involvement in production, that it was widespread in Russia well before Lenin arrived at the Finland station in April, and that it was a mass movement among the factory committees independent of the Bolsheviks or anyone else. In 1964 he argued in *Voprosy istorii* that virtually nothing of value had been written on the subject since the 1920s, for which academician I. I. Mints roundly scolded him. This did not deter Selitskii from publishing the results of his 1965 dissertation in *Istoriia SSSR* in the spring of 1967. It is not surprising that Selitskii's earlier views, expressed amid the management reforms of Evsei Liberman and

the "Prague Spring" of Alexander Dubcek, should now suffer the penalties of relevance.

In his book under review Selitskii recants many of his earlier beliefs. Like his dissertation and 1967 article, the new volume deals with workers' control between March and July 1917. Now, however, Bolshevik leadership and Leninist theory are far more visible. Even in March, Selitskii argues, Bolshevik organizations "appeared as initiators of broad practical measures of the working class in areas of production." Lenin's notion of workers' control was "essentially different from the direct practice of workers' control at the time," not simply a tactical encouragement of an existing movement. The book utilizes the same archival research as before, fleshed out by more examples of Bolsheviks in the factory committees and examples from Lenin's writings showing that he had workers' control in mind for years prior to 1917. In passages that follow word for word the 1967 article, Selitskii has now omitted phrases and sentences that suggested that the factory committees were created before the March Revolution, not after; that workers' control often meant outright confiscation of supplies, nothing more; and that the factory committees were frequently in sharp conflict with the soviets, upon whom Lenin was urging power.

Selitskii's book contains many new examples of workers' control in 1917 that will interest the specialist, but only in the context of his earlier and more serious work. Despite the title, Selitskii's workers' control is no longer a spontaneous mass movement but the result of Leninist theory and practice.

T. A. Ignatenko's volume on the historiography of workers' control is also an expansion and revision of a 1967 article in *Istoriia SSSR*. Unlike Selitskii she argues that much of value appeared on the subject before 1956. To prove her point she includes as "historical" works the early writings of left Communists like N. Osinskii (V. V. Obolenskii) and more general emigré writings by David Dallin, Nikolai Sukhanov, Paul Miliukov, and Peter Struve. Ignatenko then skips over the New Economic Policy debates and Bukharin's writings to observe that in 1931 Stalin "correctly oriented historians to a deeper study of the history of the Soviet working class in the post-October period." Later she admits that under Stalin "factual research material during those years was inadequately

interpreted and often served as illustrations of conclusions already reached."

In discussing the wealth of new studies since Stalin, Ignatenko criticizes Selitskii's earlier research and interpretation. In her view Selitskii "clearly exaggerated the importance of the slogan of struggle for workers' control, considering it essentially higher than the main slogan of the revolution, 'All Power to the Soviets!'" Despite touches of Neo-Stalinist revisionism, Ignatenko still provides a useful historiographical and bibliographical guide to the literature on workers' control.

Both of these books mark a retreat from serious research on a topic that belongs to the history of anarchism as much as bolshevism.

ROBERT C. WILLIAMS  
Washington University

MATTHEW STACHIW, and JAROSLAW SZTENDERA. *Western Ukraine: At the Turning Point of Europe's History, 1918-1923*. Volume 2. Foreword by ROBERT WELCH. Edited by JOAN L. STACHIW. (Shevchenko Scientific Society, Ukrainian Studies, English Section, volume 6.) New York: the Society. 1971. Pp. 311.

The second volume of the history of the Western Ukraine (Halychyna or Galicia) consists of three parts: first, the struggles of the Western Ukrainian Republic for peace at Paris in 1919; second, how the Western Ukrainian Republic prevented the spread of communism to the West; and third, the decisive battles on the military and diplomatic fronts.

Dr. Stachiw, who was an officer in the Ukrainian Galician Army (UHA) and a participant in the Polish-Ukrainian War (see *Ukrainska Halytska Armija* [1958], 1: 116-19, 657), and Jaroslaw Sztendera attempt to fill in our sparse knowledge of the history of the Western Ukraine, which was the Piedmont of the Ukrainian movement for independence and unity. The Western Ukraine was proclaimed an independent state on November 1, 1918, after the fall of the Austro-Hungarian Empire, and it had to struggle against the Poles for survival from the very beginning.

This book contains valuable information about attempts by the government of the Western Ukraine to make peace with the Poles, to obtain Allied recognition of the right of self-determination of the Ukrainian people, and

to expose Polish countermeasures in Paris. It discusses as well the attempts of the Allied Powers to settle the Polish-Ukrainian War.

The authors present the historical developments from the end of April 1919, on diplomatic and military fronts, describing how the Ukrainian diplomats (Vasyl Panejko, Michael Lozynskyj, and Dmytro Witowskyj) made efforts to defend the Ukrainian cause before the Supreme Council of the Paris Peace Conference. The Western Ukrainian Army (UHA), being "decimated, without any reserves of equipment, ammunition or men, not only refused to surrender, but with small forces" continued to fight against the Poles, the Soviets and the Romanians.

The decisive move of the UHA was the so-called "Czortkiw counteroffensive," which started on June 7, 1919, but was not successful, and Eugene Petruszewycz, the president of the Ukrainian Western Republic, ordered the UHA on July 5, 1919, to cross the Zbruch River to unite themselves with Symon Petlura's Ukrainian National Army, despite the Soviet-Russian "friendly proposal of alliance" against Poland.

Although the bibliography is impressive, the authors wrote their work in a narrative style, and the book is more a compilation of facts and historical developments than a research work. Unfortunately the authors, when publishing documents of that time in English translation, do not give the source of their information or where the source can be found. On the other hand, they quote some irrelevant sources—for example, V. Antonovycz's works about the Cossacks of the sixteenth through eighteenth centuries—but they do not use, for example, the valuable fourth volume of the late Theophil Hornykiewicz, *Erreignisse in der Ukraine 1914–1922, deren Bedeutung und historische Hintergründe* (1969), where important documents regarding the Western Ukrainian Republic from the Haus- Hof- u Staatsarchiv in Vienna have been published. In fact this second volume is merely a condensed translation of Dr. Stachiw's outline of the history of the Western Ukraine (*Zakhidnia Ukraina* . . . [1961]). Finally numerous typographical errors and poor editing do not enhance the work. I therefore agree with Ivan Rudnytsky in his evaluation of the first volume (*AHR*, 76 [1971]: 1573–74) that "a his-

tory of the Western Ukraine still remains to be written."

THEODORE MACKIW  
University of Akron

#### NEAR EAST

G. ERNEST DAWN. *From Ottomanism to Arabism: Essays on the Origins of Arab Nationalism*. Urbana: University of Illinois Press. 1973. Pp. ix, 212. \$8.95.

A decade ago diligent students of Arab nationalism would have been certain to encounter C. Ernest Dawn's pioneering essay on Amir Husayn and the origins of the Arab revolt. This and a half dozen other useful pieces from the same era are conveniently reprinted in this collection, and in one case translated from the Italian. The essays are of two types: hard facts on the evolution of the nationalist movement and more general studies on the nature of its ideology, particularly the evolution of "Arabism" from within the wider and more comprehensive "Ottomanism"—as the title implies, the main thrust of the collection as a whole.

The best essays are well worth resurrection, for example, "The Rise of Arabism in Syria" (1962). Of considerably lower quality, however, is the only previously unpublished chapter, which treats Hashimite aims in the light of recent scholarship. The essay adds little information on Hashimite motivation but rather focuses upon British intentions and promises to the Arabs as perceived through the works of Elie Kedourie, Aaron Klieman, A. L. Tibawi, and others. It is surprising, to say the least, to read an essay that depends so much on the exact language, word for word, of official documents and yet approaches those documents only at second hand. This is particularly so when the relevant Foreign Office material is readily available in the Public Record Office, together with the substantial collections from the Cairo Residency, the Jidda Agency, and the Arab Bureau, none of which has been fully exploited for purposes similar to Dawn's. Even then, however, without the lost original Arabic texts of the McMahon correspondence addressed to Husayn, there is a limit to the utility of the sort of semantic argument in which Dawn lets himself become enmeshed.

But the central weakness of the collection as a whole is that it has not been revised to include the results of a decade's advances in both available material and—save for the one chapter—the works based upon that material, despite some additions to the notes and a general disclaimer in the preface. "Ideological Influences in the Arab Revolt" (1959), for example, was soon superseded by Albert Hourani's *Arabic Thought in the Liberal Age* (1962); it is not really sufficient simply to add the title to a citation. Unfortunately Dawn has not chosen to apply his obvious interest and expertise to the task of reworking these articles into a needed, comprehensive study of the relationship of ideological origins to practical revolt, which might at long last replace George Antonius's *The Arab Awakening* (1938), although there is no question that these essays made, and in some cases still make, an important contribution to that end.

BRITON C. BUCH  
Colgate University

MARVIN ZONIS. *The Political Elite of Iran*. (Princeton Studies on the Near East.) Princeton: Princeton University Press. 1971. Pp. xvi, 389. \$12.50.

Social scientists interested in elite analysis will find Marvin Zonis's scholarly book a fine addition to the long list of elite studies. Those interested in the politics of Iran will find this volume exceptionally useful and valuable.

By using an impressive array of social science methods and approaches Zonis has attempted to analyze the social background and political orientation of those few Iranians (that is, the political elite) who exercise an inordinate amount of power over the behavior of others. The political elite of Iran were identified by using a two-stage reputational method. From a wide-ranging list of approximately 3,000 individual holders of important political and social positions, 167 were finally selected for structured interviews with the author. These were subjected to an extensive social-psychological questionnaire of over two hundred items. The elite's responses were then categorized into three areas dealing with their social background, general character orientation, and political and social attitudes. The variables in

each category were in turn factor analyzed for further study.

The basic guiding framework for the analysis of the data was based on a tripartite model linking the elite's social background to their general orientation and political behavior. Zonis claims that the political attitudes of the elite "are shaped by their backgrounds, the lives they have led, and the diffuse orientations they have adopted. All in turn influence the political behavior of the elite and the direction of the political system" (pp. 16-17).

The most interesting and controversial of the author's findings concerns the elite's character orientation. According to Zonis the psychological dispositions of the elite and their general orientation have produced four attitudinal clusters of political cynicism, manifest insecurity, personal mistrust, and interpersonal exploitation. These character orientations intervene between the elite's social background and their policy orientations, which shape the political life of Iran (p. 263).

The author's other findings underline the wide gap that separates the elite from the masses in Iran. The elite on the whole have been raised by parents who belonged to the landed gentry. They are well educated, speak one or two foreign languages, have traveled and lived abroad (mostly in Europe and the United States), and pursue a multiplicity of occupations. These contrast sharply with the life experience of the masses of illiterate and poor Iranians.

A few important shortcomings exist in this otherwise fine volume. In the first place the author has not demonstrated in a satisfactory manner how the elite's character orientations are in fact manifested in their political behavior and decisions about politics. Second, some key members of the elite (such as the military) had to be excluded because of factors beyond the author's control. Third, this volume suffers from a problem common to most elite studies—the assumption that the politics of a nation can be understood fully by concentrating on the attitudes and behavior of its political elite. Even partial exclusion of basic social, economic, and political factors and middle and lower classes is bound to present a distorted picture of the political system. These criticisms should not diminish the value of this volume. Zonis's work

is a pioneering and sophisticated book and is highly recommended to scholars of comparative politics and specialists on the politics of the Middle East.

FARHAD KAZEMI  
New York University

#### AFRICA

M. A. KWAMENA-POH. *Government and Politics in the Akuapem State, 1730-1850*. (Legon History Series.) [Evanston:] Northwestern University Press. 1973. Pp. xiii, 177. \$9.00.

Johan Trane, who was commandant of the Danish fort Christiansborg on the coast of West Africa (1699-1703) "continually bemoaned the everlasting wars; and finally he passionately excused himself for not being able to make the trade more profitable" (Georg Nørregård, *Danish Settlements in West Africa 1658-1850* [1966], 64). These disruptions of trade were frustrating to the European merchants then and later proved no less incomprehensible to many European historians. Georg Nørregård decided that "it is evident that the quarrels of the native peoples profoundly influenced the trade of the Europeans on the coast" but that it was "impossible to disentangle all the threads of this pattern and trace the causes and effects of every incident."

Out of the wars of Trane's time and later was born the state of Akuapem that subsequently contended with its Akyem, Asante, and Krobo neighbors. Mr. Kwamena-Poh achieves a great deal in his attempt to disentangle all the threads in the multifaceted activities of Akuapem's leaders and those with whom they interacted.

This study draws upon documents in Danish, British, and Ghanaian archives and translations of some Dutch documents in the Furley Collections at the University of Ghana. Oral traditions were collected by the author from the Akuapem (his own people) and some of their neighbors for whom he had also some earlier recordings, some published in the original Twi, and he was able to use a limited amount of archeological data.

Establishing equivalences has its problems; Sir Charles MacCarthy becomes in the oral tradition *Mankata* (p. 91), which is fairly ob-

vious, whereas Frederick Siegfried Mørch becomes *Sum*, which in Twi means darkness, apparently equating the name to the Danish *mørke* and translating it (p. 109 n. 5), but how does one explain the transformation of J. N. Flindt to *Ku* (p. 144 n. 3)?

When the sources support each other the task is relatively easy, though the events are indeed entangled, but when one contradicts another, or seems to, the author shows his considerable ability for weighing the various possible interpretations and arriving at a judicious assessment.

The Legon History series, of which this is the third volume to appear, promises soon to provide a substantial survey of the major peoples of Ghana.

DANIEL F. MCCALL  
Boston University

E. A. BRETT. *Colonialism and Underdevelopment in East Africa: The Politics of Economic Change, 1919-1939*. (Studies in East African Society and History.) New York: NOK Publishers. 1973. Pp. xi, 330. Cloth \$16.00, paper \$4.95.

Although neither the method nor the ideology employed in this book ever turn out to be quite as radical as the author sometimes claims, it is still a work that significantly advances the study of colonial Africa. In his effort to determine the development impact of British rule over East Africa, Brett has combined two approaches hitherto maintained in separate academic compartments: administrative history and development economics. The economics in earlier historical works has usually been limited to the issues perceived by colonial administrators, centering on a "good guys-bad guys" account of African peasant versus European settler agriculture. Development economists and their critics, on the other hand, often seem convinced that nothing worth discussing happened in Africa before World War II. Brett is correctly convinced that pre-World War II African development can and must be studied in terms of both the arguments advanced by contemporary European decision makers and the questions we now recognize as most critical for African development or underdevelopment.

As it turns out, Brett applies his approach mainly to European colonial policy rather

than to African economics in its root sense. Thus his most original and detailed chapters deal with white settlers in Kenya. Brett follows the standard historiography in viewing settler agriculture as the key threat to African welfare. He even adds to this interpretation by characterizing the settlers as "essentially parasites" and noting how, despite some administrative and missionary opposition, they were able to influence "most of the critical areas of policy" in their own favor. Unlike earlier critics, Brett attributes settler exploitation less to the appropriation of land than to the manipulation of more indirect economic advantages. He also notes that the "economic nationalism" of local whites helped create some degree of autonomy in Kenyan development.

While granting a certain amount of moral credit to the colonial "paternalists" who maintained peasant-dominated systems in Tanganyika and Uganda, Brett shows how the implementation of such policies also favored the interests of British economic groups, concerned this time with commerce rather than agriculture. Moreover he claims that long-term development in these territories was stymied by unwise or even pernicious transport and marketing policies.

This criticism of the paternalist regimes suffers particularly from the limitation of Brett's documentation, which is confined almost entirely to the metropolitan records of colonial governments and economic lobbying groups. Thus the quantitative data presented are insufficient to construct even the simplest econometric model against which the rationality of such undertakings as railway construction might be measured. Likewise evidence concerning African participation in this system is far too thin to support Brett's conclusions concerning indigenous class formation and the significance of anticolonial political agitation.

Brett introduces and concludes this work by defining his own ideological position as "classical Marxist." For the most part, however, this posture seems inspired less by the detailed teachings of Marx than by the desire to stake out a middle ground between the "modernization by diffusion" view of orthodox Western development specialists and the "development of underdevelopment" views expressed by such leftist third world spokesmen as Andre Gunda-

Frank. Despite his sharp attacks against the colonial economic system, Brett insists that it did, by a dialectical process of both positive stimulus and the instigation of African opposition, provide the ultimate basis for true African development. Brett himself admits that his own research has not provided conclusive evidence for such a far-reaching argument. However the questions he has pursued concerning the colonialist "politics of economic change" may usefully be extended by other researchers into areas he has still left untouched.

RALPH A. AUSTEN  
University of Chicago

ANTHONY SILLERY. *John Mackenzie of Bechuanaland, 1835-1899: A Study in Humanitarian Imperialism*. (South African Biographical & Historical Studies, number 8.) Cape Town: A. A. Balkema. 1971. Pp. xii, 236. \$11.00.

This work is a welcome addition to the literature of imperialism since it redresses in part the older negative view of Reverend Mackenzie. Although the book focuses upon his role in Bechuanaland between 1878 and 1890, the author devotes enough space to Mackenzie's earlier career to present a full picture of the man. Driven by economics, personal commitment, and evangelicalism, the young Mackenzie became a member of the London Missionary Society and was sent to Africa in 1858 to work among the Kalolo. The Kalolo scheme failed, and Mackenzie in 1862 was posted to Shushong, beginning a career of service to the Tswana that lasted until Mackenzie's death. In contrast to most missionaries Mackenzie understood the customs and aspirations of the differing Tswana groups. This empathy early convinced him that Britain ought to protect them from Boer incursions. He perceived that British economic and evangelical objectives could also serve African interests, and in the early 1880s he became an eloquent spokesman against those Boers in Bechuanaland who had established themselves at Stellaland and Goshen and continued to violate the boundary defined by the Pretoria Convention. In 1884 Britain reluctantly moved to declare a protectorate over Bechuanaland, and Mackenzie accepted the post of deputy commissioner. At odds with the Cape government and given no armed force to pacify the

area, his mission failed despite his success in persuading African leaders and the Boers in Stellaland to accept the protectorate. Cecil Rhodes and other politicians who wanted the area controlled by the Cape rather than London hastened Mackenzie's failure. The author is undoubtedly correct when he says that the Colonial Office sent Mackenzie "a man they did not trust to administer a country they did not want on terms they did not define." His successor Rhodes failed to solve the impasse with the Boers, and ultimately Sir Charles Warren and troops were ordered to pacify the area. Warren, an old friend, depended heavily upon Mackenzie's advice. Later Mackenzie wrote, lectured, and lobbied fruitlessly to get Britain, rather than the Cape or a company, to accept the responsibility of governing as far north as the Zambezi. In 1891 Mackenzie, again a missionary, was posted to Hankey in the eastern Cape where he spent the last years of his life still trying to convince the government to accept his views of British responsibility and African rights.

Mackenzie, who devoted his life to acting against Britain's shortsighted policies, was a man with great, if paternalistic, love for Africans. His problems were the clarity of his vision and his failures, perhaps explained by the adage that "a double-minded man may be unstable in all his ways, but his position is security itself compared with that of a single-minded man at the mercy of politicians."

HARRY A. GALEY

San José State University

JAMES CHAPMAN. *Travels in the Interior of South Africa, 1849-1863: Hunting and Trading Journeys. From Natal to Walvis Bay & Visits to Lake Ngami & Victoria Falls*. In two volumes. Edited from the original manuscripts by EDWARD C. TABLER. (South African Biographical and Historical Studies, number 10.) Cape Town: A. A. Balkema. 1971. Pp. xiii, 258; xiii, 244. R15.00 the set.

During the mid-nineteenth century at a time when David Livingstone was making his epic journeys to Lake Ngami and from coast to coast via the Zambezi River, James Chapman, a very young South African trader and hunter-turned-explorer, was doing much the same and in the

same area. Chapman almost anticipated Livingstone's view of the Victoria Falls and his travels down the Zambezi, but poor luck, the hostility of Africans, and Chapman's own want of that combination of vision and doggedness that so propelled Livingstone has consigned him to the ranks of those lesser pathfinders of southern Africa.

This new edition of *The Travels* (originally published in 1868) retrospectively helps to elevate his status. It is based on Chapman's voluminous original manuscript and diaries, only a portion of which were included in the original and subsequent editions of the work, parts of which were also badly bowdlerized. Tabler, the fifth editor, has tried to remain faithful to the originals, introduced material not included in the first and later editions, and cut more judiciously than his predecessors.

The modern reader thus receives a fuller account than before of Chapman's trading journeys in Natal and the Transvaal in the early 1850s, his hunting forays into what is now Botswana, his long treks to the Chobe River and Lake Ngami, and his several attempts with Samuel Edwards to pioneer a satisfactory route from the Zambezi to Walvis Bay. In 1856, when Livingstone was completing his great journey from west to east across trans-Zambezia and emerging at the mouth of the Zambezi, Chapman and Edwards were taking ox wagons to Walvis Bay. Later with Thomas Baines, the artist, Chapman traveled virtually to the Zambezi from Hereroland via Lake Ngami and Wankie's Town. This lengthy trek failed to open up a useful road to the interior, but it produced Baines's important sketches and Chapman's many unique photographs.

Tabler's cursory introductions to this two-volume version of the *Travels* and his rather stingy editorial comments on the text whet our appetite for a proper study of Chapman. Chapman's explorations, the details of his life, his reactions to Africa and Africans, his field observations of fauna and flora, his good humor during times of great privation in the interior, and the overall variety of his experiences deserve to be more fully charted and assessed. This unattractively produced edition can be but a temporary substitute.

ROBERT I. ROTBERG

Massachusetts Institute of Technology

## ASIA AND THE EAST

C. P. FITZGERALD. *The Southern Expansion of the Chinese People*. New York: Praeger Publishers. 1972. Pp. xxi, 230. \$8.95.

The presence of a substantial Chinese minority in Southeast Asia has given rise to many misconceptions as to its role and its relation to the main body of the Chinese people to the north. *The Southern Expansion of the Chinese People* performs a most valuable task in relating this Chinese minority to historical forces affecting China.

The author, who has an outstanding reputation as a scholar in the field of Chinese history, has used the past two and a half millennia as a backdrop for his study. He first traces the southern march of the Chinese people until an approximation of the present frontiers was reached, except for the Red River basin in Vietnam, which was to fall under Chinese control for a thousand years, and for Yunnan.

The pattern of expansion in Yunnan is given attention since it may presage future possible expansion in Southeast Asia. Fitzgerald shows how a non-Chinese kingdom came under Chinese suzerainty, and he points out the growing cultural and Chinese immigration developments that led to the incorporation of Yunnan into the Chinese empire. Yunnan had settlement possibilities that were not open in the Red River basin, already densely populated with Vietnamese. In Vietnam, Chinese culture was accepted but Chinese immigrants and political domination were not. A situation was created by the tenth century under which further expansion by land, across Yunnan into Burma or into Vietnam, was not possible, and the stage was set for expansion by sea, a policy followed by the Sung, the Mongols, and the Ming. The later abandonment of this policy by the Ming and the Manchus made possible European expansion in Southeast Asia and in the China Sea.

The author traces the development of the Chinese minority in Southeast Asia after the major immigration waves in the nineteenth century, and he assesses their current political and economic strength. He points out that the People's Republic has rejected the overseas Chinese policy of the Nationalists and officially considers the Southeast Asian Chinese as na-

tionals of their respective countries rather than Chinese nationals.

In spite of the efforts of the People's Republic, the Southeast Asia Chinese still face continuing discrimination, aimed at the Chinese language and their economic and political power. The "survival policy" has proved a failure, and the apprehension over long-run trends among Southeast Asian leaders has not been allayed.

Within the traditional historical framework it is possible to see the development of the Northern frontier problem, which will take China's attention from Southeast Asia. There are new variations, such as the growth of "Northern" sea power in the Indian Ocean and political and economic interests in Southeast Asia, and this together with a potential rise of Japanese sea power in Southeast Asia could make a neutralization of the region feasible.

It is a mark of the author's accomplishment that he has been able to draw together this whole rich historical background of China and Southeast Asia and its indications for future trends.

G. RAYMOND NUNN  
*University of Hawaii*

SHIGERU NAKAYAMA and NATHAN SIVIN, editors. *Chinese Science: Explorations of an Ancient Tradition*. (M.I.T. East Asian Science Series, volume 2.) Cambridge, Mass.: MIT Press. 1973. Pp. xxxvi, 334. \$12.50.

A symposium volume such as this that deals exclusively with premodern Chinese science could hardly have been dreamed of even ten years ago. What has made the difference, of course, has been the cumulative impact (1954 onward) of the successive volumes of Joseph Needham's gigantic *Science and Civilisation in China* (four volumes so far, out of a projected seven). Yet the backward state of Western scholarship in this field even today is clearly illustrated by the fact that Nathan Sivin, co-editor of *Chinese Science*, is presently the one and only American scholar exclusively concerned with the history of Chinese scientific thought and that of the twelve contributors to this English-language publication, no less than four are Japanese. These, besides coeditor Shigeru Nakayama, consist of Saburō Miyasita, Kiyosi Yabuuti, and Mitukuni Yosida. Of the



other multinational contributors, three come from the University of Malaya in Kuala Lumpur (Ho Peng Yoke, Beda Lim, and Francis Morsingh), three from the United States (William C. Cooper, Derek J. de Solla Price, and Nathan Sivin), and two from England (A. C. Graham and Joseph Needham).

This book, although not termed a *Festschrift*, was compiled in honor of Joseph Needham's seventieth birthday (1970), and the first third of its pages (headed by a frontispiece portrait of Needham) should particularly hold the attention of anyone interested in his work and philosophy. The book begins with Sivin's excellent general preface in which he discusses, among other matters, possible ways of approaching the study of Chinese scientific thought and the categories into which the Chinese themselves have traditionally divided their sciences. Then comes a brief but illuminating "meditation" by Needham himself, "The Historian of Science as Ecumenical Man." Three essays about Needham follow: a biographical memoir tracing his career as a scientist, Sinologist, and historian of science (Price); a study of the philosophy of organism that he developed as a young biochemist and now continues to apply to his understanding of Chinese science (Nakayama); and finally a critique of some of the social, economic, and intellectual factors that Needham at various times has suggested or implied may have bearing on the question of why modern science arose only in the West and not in China (Graham). These essays are all of high quality and by no means invariably laudatory. That of Nakayama, for example, discusses at some length criticisms of Needham's work made both by historians of science and by Sinologists, while Graham attempts somewhat iconoclastically to demonstrate that several of Needham's suggested factors may not, after all, have been really relevant or at least decisive.

The remainder of the book consists of papers of unequal value on varying aspects of Chinese science. Yosida's essay, "The Chinese Concept of Nature," is both too diffuse and too sketchy, and it contains several statements of dubious validity (such as its references to laws of Heaven and laws of nature on pages 73 and 89). Yabuuti's paper on Chinese astronomy is better unified but still overly sketchy, and it repeats a fair amount of what has already been

stated elsewhere. The physico-linguistic attempt by Graham and Sivin to bring meaning into the cryptic and highly corrupt writings of the Later Mohists on optics (ca. 300 B.C.) is enormously impressive in erudition and ingenuity but, alas, is by no means always convincing, at least to me. The rhymed translation by Ho, Lim, and Morsingh of a manual (from the fourteenth or fifteenth century) of plant substances used for elixirs of immortality provides a useful addition to the scant store of translated alchemical documents, but it suffers from an almost total lack of analysis of its data. Quite the opposite applies to the paper by Cooper and Sivin on medicinal uses of drugs derived from the human body (hair, nails, blood, and so on) as recorded in Li Shih-chen's *Pen-ts'ao kang-mu* (Great Pharmacopoeia) of 1596. Here an annotated translation provides the basis for a stimulating discussion of the roles of philosophical theory and folk belief in Chinese medicine. It constitutes a truly excellent introduction to the subject. Following this comes a brief but significant report by Miyasita on a medical text of 1343 wherein a mixture of *datura alba*, aconite, and other drugs is prescribed as an anesthetic for treating multiple fractures. The book proper ends with the fourth of Sivin's fine contributions: an annotated bibliography of writings in Western languages on traditional Chinese science. This in turn is followed by an excellent index by Muriel Moyle.

In sum, this book, despite inequalities, is an outstanding addition to a little-explored but highly important field of learning. It deserves wide reading.

DERK BODDE

*University of Pennsylvania*

JEN YU-WEN, with the editorial assistance of ADRIENNE SUDDARD. *The Taiping Revolutionary Movement*. New Haven: Yale University Press. 1973. Pp. xxiii, 616. \$19.50.

In Jen Yu-wen's epic English-language narrative China's mid-nineteenth-century Taiping civil war has come as close to its Homeric or Tolstoyan formulation as it shall for some time. Mr. Jen's lucidly written and superbly edited account is an updated condensation of the massive Taiping studies he has published in Chinese over the past half century. And one of the

many excellent features of this volume is its continual, careful reference back to Mr. Jen's multivolume Chinese works for those who wish more detailed and technical information. The author's commanding presence, his unabashed partisanship for the Taipings, the painstaking accretion of fine detail, and the extraordinary tempo and dynamics of the narrative all contribute, for the first time in a Western language, to making palpable this massive social upheaval aimed at overthrowing the Manchu monarchy and instituting an array of striking, comprehensive reforms. A sympathy for the anti-Manchu "nationalist" and "revolutionary" goals of the Taipings informs the whole work; but Mr. Jen clearly did not see his assignment here to be any theoretical grappling with the nature of nationalism or revolution. He draws freely and instructively from Taiping historians of Chinese Marxist persuasion as well as from Taiwan-based historians. But he wishes a plague on both of their respective houses when he feels that ideology interferes with intelligent and honest history. Hence, in direct conflict with Chinese mainland history Mr. Jen asserts (he never debates but leaves the data of his narrative or his footnotes to make his case) that the Taiping movement was neither a class war nor a peasant revolution. And in conflict with the Nationalist's line (from 1929 down to the present) and with some American scholarship, he asserts that the movement was revolutionary and that its leadership and goals were far more desirable to anti-Taiping gentry and their Confucian orthodoxies. In harmony with some mainland history, he sees, but makes no case for, the Taiping anticipation of twentieth-century revolutionary nationalism. The bulk of the narrative, quite appropriately, is devoted to detailed military history, seasoned with the author's evaluation of heroisms, betrayals, brilliant stratagems, and, more often, tragic blunders. Nowhere else in a Western language is there conveyed with such force the horrible destructiveness, carnage, and personal grief this movement left in its boiling wake. It is ironic that because of Mr. Jen's success here the reader may early on find irrelevant his obtrusive attempts to show that the atrocities of the Imperialist troops were far more extensive and frequent than those of the Taipings—though it is appreciated that the issue of the popular

support for the Taipings may have been in part a function of such factors. And the quantity and nature of popular support is one of the many points that the book details. Indeed, a great value to the nonexpert or aspiring researcher is its clear indication of basic points of contention over detail and interpretation that have troubled historians of the Taipings. In those sections that give welcome relief to the bloody details of warfare there are excellent and often impassioned evocations of key personalities as well as fine summaries of the role of women in the movement, the nature of foreign intervention, Taiping ideology and programs, and a noteworthy characterization of the unique anti-insurgent Hsiang army. All this is done masterfully without interrupting the chronological flow or the sense of compulsive movement that the narrative assumes in imitation of the Taipings themselves. Here then is the definitive Western-language account of the Taipings: it could be drawn on for classroom use to great effect; it is for all students of modern China, for "comparativists" of social movements, for anybody who wants to know about the Taipings. The author and publisher have made a complete effort and achieved a stunning success.

LAURENCE A. SCHNEIDER

*State University of New York,  
Buffalo*

STUART R. SCHRAM, edited and with an introduction by. *Authority, Participation and Cultural Change in China: Essays by a European Study Group*. (Contemporary China Institute Publications.) New York: Cambridge University Press. 1973. Pp. viii, 350. Cloth \$17.00, paper \$4.95.

LUCIAN W. PYE *et al.* *China: Management of a Revolutionary Society*. Edited by JOHN M. H. LINDBECK. (Studies in Chinese Government and Politics, number 2. Sponsored by the Subcommittee on Chinese Government and Politics of the Joint Committee on Contemporary China of the American Council of Learned Societies and the Social Science Research Council.) Seattle: University of Washington Press. 1971. Pp. xiv, 391. Cloth \$7.95, paper \$3.45.

These two volumes have but two articles with similar topics: both contain analyses of educational change and economic policy of Communist China. Otherwise their contents are diverse.

*Authority, Participation and Cultural Change in China* is edited by Stuart R. Schram and includes the contributions of seven scholars. A collection of essays by a European study group, the work is a result of meetings of the contributors in London in April 1971, in Hamburg in February 1972, and in Leiden in May 1972. Revised versions were then again presented at a week-long conference at Urchfont Manor, England, in September 1972. The object of this work, very similar to its American counterpart's efforts, is to grasp the dynamics of change in Chinese politics and government as a result of the Cultural Revolution. The contributors are Stuart R. Schram, Jack Gray, Marianne Bastid, Jon Sigurdson, Christopher Howe, John Gardner and Wilt Idema, and Andrew J. Watson. Their topics include alternative strategies of social change and economic growth; levels of economic decision making; rural industry; labor organization; education; and family and interpersonal relations. In addition Schram contributed an extensive introductory essay designed to provide historical perspective to the Cultural Revolution.

*China: Management of a Revolutionary Society*, edited by the late John M. H. Lindbeck, is a collection of ten essays on Communist Chinese politics written under the sponsorship of the Subcommittee on Chinese Government and Politics of the Joint Committee on Contemporary China of the American Council of Learned Societies and the Social Science Research Council. The work is a selection of the papers presented to the 1969 summer conference on government in China. Lindbeck indicates that the subject of this volume is contemporary China, the topic is government, and the theme is change. Readers must bear in mind that the Cultural Revolution was at its height while these papers were being prepared and the conference was held. The purpose of this volume was to examine "the ways in which the Chinese Communists have handled the management of their enormous and diverse society." The political scientists authoring the essays are Lucian W. Pye, Chalmers Johnson, Michael C. Oksenberg, Frederick C. Teiwes, Peter Schran, Victor H. Li, Donald J. Munro, Donald W. Klein, Ellis Joffe, and Gabriel A. Almond. The topics related to Communist Chinese politics and government are mass participation, leadership,

policy making, provincial politics, economic management, the legal system, education, foreign affairs, and the army.

A significant essay found in this work is Almond's "Some Thoughts on Chinese Political Studies," which discusses research methods used by contemporary political scientists in their analyses of current changes in Communist China. Despite "inaccessibility of the country to direct research, and the dependence of our scholarship on newspaper material . . . [and] radio broadcasts . . . we may expect improvement only if the knowledge we have . . . is more effectively utilized." Schram states that "all the chapters in this book have thus, to some extent, an historical dimension." From my point of view, however, the ideas Almond expresses are most highly pertinent. His thoughts show us the problems facing the historian, and they provide the practical explanation of why more works on Communist China are written by political scientists and fewer by historians.

Readers probably will agree with the editors that these volumes are not texts on Communist Chinese government, administration, and politics; rather they are the products of the thought and current research of the authors. Readers might also agree that these volumes are not historical works, but to some extent they are of value to history students interested in contemporary Communist China.

SIDNEY CHANG  
California State University,  
Fresno

PAUL AKAMATSU. *Meiji 1868: Revolution and Counter-Revolution in Japan*. Translated from the French by MIRIAM KOCHAN. (Great Revolutions.) New York: Harper and Row. 1972. Pp. xiv, 330. \$8.95.

The subtitle of this book is misleading. From it the reader assumes that the author subscribes to the theory current among historians in Japan that a movement with revolutionary content was frustrated and replaced by a counterrevolution that established the authoritarian Meiji state. Akamatsu, however, is more cautious than the subtitle suggests. In his introduction he rejects "restoration" as a term to describe the upheaval of 1868, since the imperial dynasty

never ceased to exist and had never relinquished ultimate political authority. "Revolution" is retained tentatively, for the author states that whether the term is applicable to the event must remain "very ambiguous."

The book is divided into two parts, and in organization and treatment the "revolution-counter-revolution" thesis emerges, though never clearly stated. Part one contains the social, economic, and ideological background of the late Tokugawa period (with emphasis on the economic problems and peasant unrest) and a narration of the political events through 1865. Part two deals with the downfall of the *Bakufu* and the establishment of the new state. The Meiji period is sketchily treated, almost like an aftermath, but Akamatsu does not regard it as one of unmitigated reaction; in the policies of Okubo the author finds elements of modernization along with authoritarian trends.

Akamatsu seems caught in the dilemma of an honest scholar who seeks to reconcile historical realities with his theoretical predilections. His attachment to the idea of a popular movement burgeoning in the years prior to 1868 beguiles him; he finds support for his idea even in the 1865-67 activities of the Christians, though their importance in the national scene must be considered miniscule. But he notes that no significant leadership emerges from among the peasants, and he sees no revolutionary program. Hence he shies away from categorical statements of doctrine and prefers to present a detailed description of events.

Herein lies the strength and weakness of the book. It is full of useful and interesting details, and readers who do not have access to Japanese sources will be introduced to several personalities who usually do not make the pages of Western textbooks on Japan. On the other hand, the author is hesitant about coming to grips with the problem posed by his subtitle. He asserts that the Meiji change of regime was a political revolution because a small group of lower-ranking samurai decided to supplant the authority of the *Bakufu* and the daimyo, but the explanation begs the question of what constitutes a revolution. He is even more equivocal as to whether the revolution proved abortive and whether a counterrevolution had set in. His final conclusion is that the Meiji revolution in the long run "made possible the completion

of the economic revolution and of a social revolution." The notion of counterrevolution thus seems to have been dissipated by the facts, which reflect well on the author.

ROBERT K. SAKAI

University of Hawaii

RICHARD H. MINEAR, *Victors' Justice: The Tokyo War Crimes Trial*. Princeton: Princeton University Press. 1971. Pp. xv, 229. \$7.95.

In contrast to the judgments at Nuremberg, the Tokyo war crimes trial has been forgotten by Western scholars and laymen alike. Those few who can recall the events that led to the hanging of seven and the imprisonment of eighteen Japanese leaders in 1948 have a vague notion that justice has been done. In the first book-length study of the affair in English, Richard H. Minear sets out "to challenge this prevailing image . . . to demolish the credibility of the Tokyo trial and its verdict" (p. ix). He achieves his goals in a lively, passionate work of "political scholarship" (p. xiii).

Some will undoubtedly be irritated by the manner in which Minear makes explicit personal biases and preferences that more decorous historians bury between the lines. Nevertheless, his intelligent and professional use of the evidence available in several languages as well as his objective consideration of the issues brilliantly illuminate the travesty that was "victors' justice."

This is not a history of the Tokyo proceedings. Unfortunately, perhaps, Minear offers little information about the backgrounds of the defendants, their testimony, or even the highlights of the dramatic two- and one-half-year trial. Rather, he limits himself to a handful of questions relating to international law, judicial procedure, and the ultimate verdicts. He throughout relies heavily upon the angry dissent of one of the tribunal's eleven judges, Radhabinod Pal of India.

Turning first to international law, a difficult subject that he handles with admirable clarity, Minear doubts the possibility of ever proving the primary charge against the defendants, conspiracy to conduct aggressive war. Further, the ex post facto character of the Tokyo charter, which established the ground rules for the

tribunal, compromised the legality of the proceedings.

The court's handling of procedural matters was even more questionable. According to the author, most of the judges were either unqualified, hopelessly biased, or both. Moreover the criteria for the selection of the defendants were arbitrary. Industrialists as a class as well as other seemingly culpable figures were not brought before the Tokyo bar. Finally, the court's decision to jettison the traditional rules of evidence made it easier for the prosecution to build its case against the defendants.

Miner's own case is not without its weaknesses. He is unsympathetic to those who attribute the blindness of Allied justice to the highly charged emotional climate of the early postwar years. In addition, not all of his conclusions are entirely convincing. His historical-legal brief for the defense still goes a long way toward destroying the credibility not only of the Tokyo trial but political war crimes trials in general.

MELVIN SMALL  
Wayne State University

SRI RAM SHARMA. *The Religious Policy of the Mughal Emperors*. 3d rev. ed.; New York: Asia Publishing House. 1972. Pp. xi, 245. \$6.50.

A new edition of this reliable study is welcome. With clarity and succinctness the author discusses religious policy during the sultanate and devotes compact, informative chapters to the vicissitudes of that policy in the reigns of Mughal leaders from Baber to Aurangzeb. Sharma traces the decline of Akbar's religious liberalism and "comprehensive state" under three succeeding Mughals. Where the former had neutralized the power of Muslim theologians, the latter gradually restored it, until by the time of Aurangzeb their pre-eminence was undisputed, resulting in a "Golden Age of mulādōm." The book rests squarely on original sources, has incorporated the best of earlier scholarship, and combines close analysis with judicious synthesis. Sources are used critically and with discretion. Several useful lists are included, such as Hindus recruited into public service as mansabdars in the reigns of Akbar and Aurangzeb, Hindu converts to Islam under Aurangzeb, and noted Sanskrit writers who

flourished under each of the great Mughals. Chapters are organized generally around a few key topics: policies concerning the jizya, the pilgrimage tax, sumptuary laws, interfaith conversions, apostasy, Hindu participation in court ceremonies and public service, and the like. The author draws many apt parallels and contrasts between religious climate and practice in Mughal India and Europe in the sixteenth and seventeenth centuries. Thus he suggests that even under the repressive and brutal sultanate, Hindus tended to fare better than European religious communities whose convictions departed from that of established authority. Errors and distortions of earlier scholarship (the charge, for example, that Akbar persecuted Muslims or the notion that he was a free thinker) are reviewed and carefully rebutted.

This third edition contains revised public service lists and new appendixes on Aurangzeb's rebellion against Shah Jahan and the nature of the Mughal state. The author's treatment of Mughal polity includes a critique of the view that Mughal emperors ruled by "divine right." There is no real parallel, he judges, with the claims of European kings, for the notion of divinity in the Mughal context means no more than "the usual Muslim belief that whatever happens in this world is ordained by God." The characterization of Mughal government as an Oriental despotism is explored, the conclusion being that it was "a despotism of a limited nature" that left considerable latitude to the average citizen, chiefly because emperors were constrained by legal sanctions and various traditions, both Muslim and Hindu. Thus Aurangzeb's ruthless attempt to promote Muslim over Hindu seems to imply unrestrained power, while in fact he fell more completely than his predecessors into the controlling hands of orthodox Muslim theologians.

K. R. STUNKEL  
Monmouth College

JOHN ALLEN LAPP. *The Mennonite Church in India, 1897-1962*. (Studies in Anabaptist and Mennonite History, number 14.) Scottdale, Pa.: Herald Press. 1972. Pp. 278. \$8.95.

In attempting to analyze the contributions of a work such as this, one is bound to start with the question as to why the author wrote the book.

Dr. Lapp spent many long hours at the archives of the Mennonite mission so that he could explicate the rise, if not the growth, of the mission's activities in Dhamtari, in present-day Madhya Pradesh, from the time of its foundation in the famine year of 1897 to the present. In the past, mission history has been written very much in the hagiographical tradition, and although this study escapes that difficulty to some extent, the author has failed to go beyond the very parochial findings that he unearthed in the printed sources or in the archives.

The Mennonite missionary work in Dhamtari was from the start identified with a large missionary community recruited from America who worked among a very underprivileged Indian group attracted to the Church literally as "rice Christians" in the 1897 famine in central India. The low priorities that the mission established for education as an essential element in its missionary goals, the isolation of the mission station from centers of urbanization or culture, the high proportion of missionary personnel, and the very authoritarian style adopted by the missionaries all made this missionary enterprise, at least from the point of view of the missionaries, hard to devolve into Indian hands when the time came for the missionaries to leave. From the point of view of the Indian Christians, the policy of the missionaries had been wrong from the start. As one leader of the Indian Christian community put it in 1949, "in our view the Mission has a moral if not a legal responsibility to see that we are enabled to stand on our own legs. You must remember that most of us were taken into institutions when we were dying of starvation during times of famines. We were uprooted, so to say, from our native soil. Granted that it was very poor soil, but we had our roots in it. Can we say the same thing about our present situation? Our house is built on sand" (p. 184).

The difficulties of the book are actually a function of the author's misperception about the goals of modern scholarship on India (or any other country, for that matter). The importance of the Mennonite mission is surely part of a much wider change that occurred in many areas of India during the twentieth century as a result, in part at least, of govern-

ment and missionary work. Perhaps we can expect more from the author in the future.

EUGENE F. IRSCHICK  
University of California,  
Berkeley

DINESH CHANDRA JHA. *Indo-Pakistan Relations (1960-1965)*. Patna: Bharati Bhawan. 1972. Pp. xiii, 418. Rs. 30.

The study of Indo-Pakistan relations, according to the author of this work, has been mainly a study of conflict. During the period of study, 1960-65, "their relations were particularly marked by almost a continuous process of deterioration, the only exception to this being the period between April and September 1962." Even then, a few pleasantries aside, the basic stands of the two countries remained the same (p. 144). The relations were no better before 1960, nor did they improve after 1965.

Several studies of the same nature have been published over the years in South Asia and elsewhere by scholars from both countries. They list more or less the same causes for the conflict, approach the subject as judiciously as possible, but somehow end up presenting a case approximating their respective national positions. Exceptions are rare.

The present author has tried to maintain a balance in presenting the positions of the two countries; yet, referring to the 1970 Indo-Pakistan War (in a review of post-1965 events), he states that the war among other things "liberated Bangla Desh finally from the Pakistani yoke" (p. 11), or that the suspicion of Pakistan about India seeking to undo it is so strong that it views with alarm any Indian suggestion to form an Indo-Pakistan confederation (p. 354). It is interesting that he quotes many Indian leaders, including Nehru, who, while accepting the partition of the subcontinent, "carried in their mind the dream of a United India" (p. 12).

Such literature, even if it shows a tilt toward the author's national political position, has its value. My objection to this and similar works is basic. Writing a history of current events is a risky job to begin with. When a scholar chooses to depend for his sources primarily on government releases or newspaper reports of the statements of public figures supporting

government positions, his work is hardly a study of the conflict. It is merely a narration of the publicly stated positions of the two governments. This factor becomes more important when we realize that in India and Pakistan the power, freedom, and resources of the press are extremely limited.

Two more points may be made in relation to this work: first, the author has completely ignored the vernacular press and, second, the internal conflicts and complexities of the decision-making processes of the governments in both countries have not been considered at all.

The author seems to have worked hard in putting before us in a well-organized chronological order the positions that the governments of India and Pakistan took on various issues that caused the conflict including two wars between them during 1960-65.

MASOOD GHAZNAVI

Rosemont College

PAUL F. LANGER and JOSEPH J. ZASLOFF. *North Vietnam and the Pathet Lao: Partners in the Struggle for Laos*. Cambridge, Mass.: Harvard University Press. 1970. Pp. xiv, 262. \$5.95.

"We fought together—shoulder to shoulder," remarked an officer of the Lao People's Liberation Army about the much-appreciated assistance of North Vietnamese soldiers during a campaign in Laos in 1961. This officer's testimony and that of dozens of other Lao and Vietnamese defectors and prisoners have been utilized, along with field research and captured documents and other literature, to reveal in greater detail than ever before the story of the relationship between the Lao Communists and their brethren in Vietnam. The story is told competently, clearly, and concisely.

The theme of the book is, as the title indicates, limited. The book does not have the range of Arthur Dommen's *Conflict in Laos* (1971) or Hugh Toye's *Laos: Buffer State or Battleground* (1968). Yet the theme is vital to the understanding of events in Laos. While the remarks of one discouraged Vietnamese adviser to Laos—"If the Vietnamese went home, the Lao wouldn't know what to do"—may be something of an overstatement, it contains an essential truth regarding the crucial nature to the Pathet Lao of the Lao-Vietnam partnership.

*North Vietnam and the Pathet Lao* is divided into two parts. Part one, "The Past," recounts the history of the Lao revolutionary movement to mid-1962. Part two, "The Present," discusses the North Vietnamese-Lao relationship to 1970. The Lao revolutionaries broke away from the Lao nationalist movement in 1949 when the majority of nationalists decided to accept a French compromise. The revolutionaries, a very small group without resources, needed support for survival. Their logical allies were the North Vietnamese, who had the interest and the experience that would be helpful and who shared with the Lao revolutionaries a bitter hatred of the French. The Pathet Lao-North Vietnamese partnership over the years has assumed many forms, from direct military assistance in campaigns to political advice on the formation of a party (the semi-secret People's party of Laos, patterned on Vietnam's Lao Dong party), the organization of a mass front (the Neo Lao Hak Sat, patterned on the Viet Minh), and the institution of political cadres permeating the civil and military administrations. The Lao position has been, of necessity, subordinate to Vietnam. Yet remarkably little Lao antipathy has developed; the Vietnamese have been well trained to respect Lao sensitivities.

The long-range future of the partnership is hard to predict, but in the short run it seems durable. For North Vietnam profits from the free use of the Ho Chi Minh Trail, from the security gained by the presence of a Lao Communist buffer, and from the psychological lift that comes from providing aid to a beleaguered ideological brother. And the Pathet Lao profits from Vietnamese material and advisory and moral aid, which it feels has not compromised its legitimacy as a nationalist movement.

There is a useful appendix that includes a thirty-page chronology of important events in Lao communism.

WALTER F. VELLA

University of Hawaii

DOUGLAS OLIVER. *Bougainville: A Personal History*. Honolulu: University Press of Hawaii. 1973. Pp. ix, 231. \$4.50.

Douglas Oliver, emeritus professor of anthropology at Harvard and Pacific Islands professor

of anthropology at the University of Hawaii, is a noted specialist on the cultures of the Pacific. He first did research at Bougainville in 1938-39, returning to the island in a government post during the Second World War, and later, since 1968, he has occasionally acted as adviser for the Bougainville copper company. The term "personal history" in the title of this volume is not clear. Oliver's account is not autobiographical or anecdotal, rather it is an unpretentious yet solidly informative description of an important Melanesian island society with emphasis on its indigenous institutions, from land tenure to marriage and residence patterns, and on the effects of the "sudden and massive intrusion of advanced industrialism into an archaic Melanesian peasantry."

It is inevitable in such a general survey that one finds sections that have little more depth than the average travel or tourist guide (Bougainville may not have one, and Oliver's little book will do very well for any newcomer to the island), but other chapters, such as "The Indigenes in pre-European Times," are more substantive, offering some valuable insights into language structure, methods of food production, and family and lineage organization. The Bougainvillean heritage comes to life in these pages, and here and there Oliver draws appropriate contrasts: the "thoughtful islander leafing through the advertisements in a popular European magazine" can not fail to be struck by the Westerner's "pre-occupation with comfort," whereas Bougainville tradition, on the other hand, sees the good life as one of "freedom from fear, fear of unfriendly spirits and of hostile humans," and so on.

Though large libraries today can no longer contain the flood of what has been published in recent decades on the contact between Western and colonial peoples, Oliver's book offers a useful, brief synopsis of the classical pattern of such contact. The Bougainvillean's labor and, increasingly, his land as well was and is coveted by Western enterprise. Already by the end of the German era "some 283,000 hectares of land had become permanently alienated from indigenous ownership," and the Bougainvillean had entered the colonial plantation economy not only in his own home island, but also earlier on the sugar estates of Queensland. The discovery of copper deposits in 1960 soon

brought new demands for land by the mining company: "land for the mine itself, for roads and power lines, for disposal of tailings, for port and storage facilities, and for employee housing." Despite safeguards and reasonable official concern for compensation, the mining interests' land acquisitions seriously dislocated the Bougainvillean social economy and traditional culturally integrating mechanisms. Oliver does not gloss over these unsettling effects and other problems of modernization. But he offers little perspective on how they might be mitigated, especially now that the island, along with the rest of Papua-New Guinea, is on the threshold of independence. And yet Bougainville secessionist sentiments remain strong. Photographs, drawings, maps, and an extensive bibliography add to the value of the book.

JUSTUS M. VAN DER KROEF  
*University of Bridgeport*

ROBERT LACOUR-GAYET. *Histoire de l'Australie*. (Les grandes études historiques.) [Paris:] Fayard. 1973. Pp. 558. 50 fr.

Even after its discovery by the Western world, Australia remained a terra incognita to most of the world. Geography and colonial control together with an inward-directed population conspired to make few outsiders privy to the continent's development. The sweeping, comprehensive account of this book reveals the saga of Australia from a conjecture in some curious Western minds and the abode of a handful of primitive aborigines to a highly civilized, though not yet highly cultured, society. The author achieves a dramatic effect by skillful reporting of events, which are judiciously chosen, and a brief characterization of people, individuals or groups, behind these events.

The main value of this book is in its total impact. It covers the main stages of Australia's history. It provides a good impression of the hazardous course of Australia's development from a scarcely viable prison society to a community with an enviable standard of living. But it adds nothing new either in fact or interpretation. Indeed, the author relies mainly on secondary sources (not likely to be generally known in France). In a comprehensive account of this kind, beginning with prehistoric times and ending with current events, argument is pos-



sible over the omission of details or emphasis on particular events. There is, for instance, only a fleeting reference to the White Australia policy. Nothing is said about Australian attitudes toward Asia, and very little about the perpetual fear of Australians of foreign invasions. Yet these psychological conditions of the Australian people could help in explaining some social policies, much of Australian politics, and a good deal about Australia's role in the Pacific and the world. Nevertheless, the book achieves its purpose very well: the reader will become well acquainted with Australia and her people. The one annoying feature of the book is an abundance of quotations in the text without an indication of their sources.

WERNER LEVI  
University of Hawaii

#### UNITED STATES

JOHN UPTON TERRELL. *Pueblos, Gods and Spaniards*. New York: Dial Press. 1973. Pp. xxiii, 358. \$10.00.

John Upton Terrell is a seventy-three-year-old ex-newspaper correspondent. Since 1962 he has written nineteen books, most of which deal with borderlands and exploration themes. During this same period he has written in addition twelve books for juvenile readers in which he has sketched the history of major departments of the federal government. In the past several years Terrell has turned his attention to Indian themes: Navajos, Apaches, and now Pueblos.

Like Dee Brown, Terrell is a popularizer and a good one. His research consists of reading a dozen or so standard sources. His writing consists of linking together largely unacknowledged excerpts from these sources in a loosely chronological manner. This particular volume focuses on the period of Pueblo history from Coronado's expedition in 1540 to de Vargas's reconquest in 1693. In his opening chapters Terrell improved slightly upon his historical sources by appending information on Pueblo culture taken from archeological and anthropological sources. This worthy idea was then abandoned in the remainder of the book.

*Pueblos, Gods and Spaniards* contains no new information, no novel interpretations. Terrell's sources, which are chiefly the volumes in

the Coronado Quarto Centennial Publication series, edited by George P. Hammond, are familiar to all historians of the Southwest. Approximately one-third of the "selected" bibliography, which is not annotated, consists of books that have little relevance to the topic. What is most disconcerting, however, is that except for the preface and the epilogue there is precious little about the Pueblo Indians, who play a decidedly secondary role in this conventional history of Spanish conquest of the Southwest.

Twenty years ago Paul Horgan accomplished this same service for the general reader in *Great River*. *Great River* is still available in paperback at considerably less money.

LAWRENCE C. KELLY  
Denton, Texas

ALICE E. SMITH. *The History of Wisconsin*. Volume 1, *From Exploration to Statehood*. Madison: State Historical Society of Wisconsin. 1973. Pp. xiv, 753. \$15.00.

Written history is one of Wisconsin's most notable products; this work—the first in a projected six-volume history of the state—is in the best tradition of the state's productivity in the historical field. Drawing upon the scholarly output of the State Historical Society and of the history department of the state university, Alice Smith has written a book that bears testimony to the contribution made to the history of the state by such Wisconsin historians as Draper, Turner, Thwaites, Kellogg, Quaife, Schafer, Paxson, Hicks, Curti, and their colleagues and students. The book testifies, too, to Miss Smith's own research accomplishment, concerning especially the economy and politics of the territorial period, during a fruitful career as senior research associate at the State Historical Society, and to her broad understanding of national developments, in the context of which local history of this kind must be written. To judge from her footnotes and bibliography she has left unread virtually no book, article, doctoral dissertation, or master's thesis bearing on the Wisconsin experience to 1848, and she has digested this wealth of material with remarkable balance and in an engaging literary style.

Developments to 1848, in what is now Wis-

consin, provide a capsule history of the early contacts of Europeans and of the nature and problems of the original permanent settlements in the upper Middle West. Miss Smith provides lucid detail on most aspects of the story: French penetration and British involvement; Indian relations during the Revolution and the early national period; the removal of Indian claims and efforts to acculturate the natives; the physiographical basis of American occupation and exploitation; the appeals to settlement, including townsite promotion; the society, economy, civil government, and politics of the territorial period; and the movement that resulted in statehood.

An admirable chapter on cultural strivings benefits particularly from Miss Smith's use of the work of architectural historians, though she might have made more than she does of the element of chance in the cultural resources available in a frontier setting. Throughout, she is commendably sensitive to the existence of an urban dimension in the society of this early period and wisely recognizes the urban character and urban aspirations of villages and towns, still small in terms of population. She might well have attempted to assess the relative impact of such urban dwellers in the territorial legislature and the constitutional conventions. Her nevertheless substantial treatment of the achievement of territorial status and, later, statehood supplies significant evidence of the liberalization of government that accompanied the successive creation of new territories and of the conservative individualism that had become prevalent by the time statehood was accomplished.

BAYRD STILL  
New York University

IAN M. G. QUIMBY, editor. *American Painting to 1776: A Reappraisal*. (Winterthur Conference Report 1971.) Charlottesville: University Press of Virginia, for the Henry Francis du Pont Winterthur Museum. 1971. Pp. x, 384. \$4.50.

The Winterthur Spring Conference was started in 1954 as a reunion-study weekend for alumni of its graduate program in the decorative and related arts, which is administered by the University of Delaware. The decision to open the meeting to outsiders specializing in the yearly

subject was welcomed by all professionals for the opportunity it would present to share in at least some of these arts. Confining these conferences to a very restricted topic and time span it is possible to obtain papers of unusual excellence and scholarship. Now, thanks to Winterthur's expanded publication program, these yearly symposium proceedings are available to the general public to enjoy and study.

One has only to glance through the notes accompanying the seven major papers in this report to realize how long overdue was a re-study of colonial American painting. Excepting, of course, the original colonial documents cited, the largest portion of research material is pre-1960. Therefore, the papers present to scholars a great deal of information on recent discoveries and work in progress. Prospective students may benefit from ideas for possible research. To historians, the opening remarks of the meeting made by Neil Harris, history department of the University of Chicago, speak most eloquently of the contributions symposiums such as this can make to the new approaches to interdisciplinary studies and scholarship.

The essays are of such uniform excellence that it would seem unfair to cite any one of them individually. However, it is hoped that Roland Fleischer's work on Gustavus Hesselius and Peter Mooz's on Robert Feke, presented in this volume, will develop into full, much-needed biographies of these artists. And perhaps the lack of current scholarship concerning Southern colonial painters will be noted and undertaken by someone soon.

Certainly a most important contribution in these meetings was the section dealing with the technical aids to identification of colonial paintings. Conservators highly trained in the use of new scientific machinery are playing an increasingly large role in assisting curators in their work on attributions. Four leaders in the field took part in this meeting and explained some of the methodology now being used in this work. The illustrations, of good quality throughout, are particularly helpful in understanding these technical papers.

It is also interesting to have published the question and answer period that followed, as they further explain some of the problems these conservators now face.

Peter Mooz, chairman of the conference, is to be congratulated on the excellent variety of topics and speakers selected. The even more difficult task of seeing through to publication, lectures transformed into essays, has been accomplished with great skill by Ian Quimby and his staff.

JOAN D. DOLMETSCH

Colonial Williamsburg Foundation

WILSON CAREY MCWILLIAMS. *The Idea of Fraternity in America*. Berkeley and Los Angeles: University of California Press. 1973. Pp. xiv, 695. \$14.95.

Everything is on a grand scale in this ambitious, massive study of American thought: the range, the length, the successes, the deficiencies. McWilliams, a professor of political science at Rutgers, has attempted no less than a full-scale reinterpretation and critique of American liberalism. He invokes Robert Frost's phrase, "a lover's quarrel," to evoke his own stance toward the Left.

This book reminds one of Parrington's *Main Currents in American Thought* at many turns, except, very importantly, that McWilliams reverses Parrington's celebration of the liberal tradition; many of Parrington's heroes receive severe scolding from McWilliams, while many abused figures of Progressive history are resurrected. Like Parrington, McWilliams traces patterns of American thought through the written works of major individuals: Winthrop, Cotton Mather, Roger Williams, John Wise, Jonathan Edwards, James Wilson, Jefferson, Calhoun, Carnegie, William Demarest Lloyd, Henry George, John Dewey, Eldridge Cleaver. As with Parrington the emphasis lies with literary figures. McWilliams has chapters on Emerson, Thoreau, Hawthorne, Melville, Whitman, Belamy, and Twain and deals with such writers as Fenimore Cooper, Sinclair Lewis, Frost, James Baldwin, and Ralph Ellison. Similarly, this work is as much an impassioned affirmation of certain ideas as it is a detached study of the past. Hence, figures are judged, prose is often eloquent, new understandings are called for. In the mix of history and exhortation lie many of the triumphs and difficulties of the book.

McWilliams's thesis has a simple core. In certain ways he elaborates and documents in a

more thoughtful and knowledgeable manner some of the ideas in Philip Slater's *The Pursuit of Loneliness*. Americans today stand isolated and lonely as a result of the domination in their lives of the Enlightenment tradition. Liberal Lockeans, with their emphasis on individualism, liberty, property, and the taming of nature, made a major error that their Puritan precursors had not committed: they assumed that fraternity would emerge as a result of egalitarian society. The Puritans and others steeped in religious tradition insisted, according to the author, that social order must begin with brotherhood and then work outward from there. Americans can be saved from anomie, he believes, only when they realize that "the ancients were right in seeing fraternity as a means to the ends of freedom and equality; and correspondingly, that the modern theorists who reversed this relationship were guilty of a serious error."

McWilliams begins with a difficult effort to conceptualize and define fraternity in the first 100 pages of the book. He follows, for the remaining 500-plus pages, with his chronological analysis of writers and leading historical figures seen from the perspective of their understanding of fraternity. Those who succumbed to Enlightenment optimism and facileness, who saw man through the metaphor of the *tabula rasa*, fare badly, while those with a greater sense of the tragic nature of life and the darker shadows of the human soul, and who thus began with a call for brotherhood in order to overcome human weakness, are appreciated.

The author displays an extraordinary grasp of an unusually wide range of materials. His book is consistently intelligent, frequently wise, and sometimes quite moving. Arresting and original insights illuminate many pages, and some of his analyses of literary figures break new ground. At times his prose is luminous.

On the other hand his chapters occasionally move like the slow movement of a Mahler symphony, but without the intensity. The book should have been cut by one-third, thus avoiding much of the tedium and pretentiousness that McWilliams accurately acknowledges in the first paragraph of his introduction. There are times when he labors unnaturally to connect an analysis of a writer, often unfolding superbly in its own right, with his central thesis ("how

will Twain measure up on the matter of fraternity?"'), resulting sometimes in an unbalanced, distorted understanding of the person. This is a common shortcoming of long books that are sustained by a single theme.

The value of *The Idea of Fraternity in America* rests finally on the validity of its central thesis, that is, on the solidity of its criticism of the Enlightenment tradition in America and the persuasiveness of its appreciation for the ways in which certain religious and ethnic traditions understood fraternity and may be used to help Americans find the path to a new place of brotherhood. Readers may arrive at different verdicts on that central issue. But on the way they will, I think, find a study whose flaws, though not inconsequential, are offset by the book's unsettling perspectives, far-reaching scholarship, and informed urgency, qualities that are hallmarks of important books.

RICHARD L. RAPSON  
Stanford University

*Boston Prints and Printmakers, 1670-1775: A Conference Held by the Colonial Society of Massachusetts, 1 and 2 April 1971.* Boston: Colonial Society of Massachusetts; distrib. by the University Press of Virginia, Charlottesville. 1973. Pp. xxv, 294. \$15.00.

*Boston Prints and Printmakers, 1670-1775* represents a model for an increasingly important segment of historical publication. Unlike older annual publications, this volume groups essays and monographs widely divergent in style and objective but closely related in subject matter. Eight aspects of early printmaking bearing upon America are so superbly illustrated by the Meridan Gravure Company that the text itself becomes a monument in modern New England book illustration and printing.

The eight articles are uneven and distinctive in intent. Four are summaries of existing scholarship, and four suggest sharp new directions for research. All are very welcome in a single volume and indicate a revival of the interest in engraving that Harry Peters and I. N. Phelps Stokes represented a generation earlier.

John Reps, as might be expected, summarizes superbly the printed views and maps of Boston by Bostonians. Quite properly he concludes with the superb Henry Pelham plan of Boston (London, 1777) decorated by the painterly

trompe l'oeil of dividers and Pelham's military pass of August 1775.

Excellent summaries, together with checklists and rich illustrations, of the work of William Burgis, Thomas Johnston, and Peter Pelham follow. Perhaps most surprising is the wide range of engravings Sinclair Hitchings has attributed to Thomas Johnston, including trade cards, maps, views, music, clockfaces, bookplates, military commissions, and possibly compass cards.

Martha Gandey Fales opens intriguing insights through the gathering of heraldic and emblematic engravings on trade cards, treasury notes, seals, bookplates, and on silver plate. She compares, among others, the work of Thomas Johnston, James Turner, Jacob Hurd, John Coney, and Paul Revere with extremely suggestive new points of view. Certainly the free use of heraldry was not the least of colonial liberties.

Charles Wood's study of scientific illustration and Bradford F. Swan's exploration of American Indian prints are both welcome not only because they commence new concerns, but also because they present good copies of very rare engravings and mezzotints.

Since all the illustrations are black and white, they would seem an ideal subject for a 150-print microfilm roll, such as those done by the National Gallery of Art. Certainly this would immensely broaden the knowledge of these visual materials and respect for them in the teaching of American history.

ANTHONY GARVAN  
University of Pennsylvania

J. WILLIAM FROST. *The Quaker Family in Colonial America: A Portrait of the Society of Friends.* New York: St. Martin's Press. 1973. Pp. vi, 248. \$12.95.

The purpose of this volume is to explain through a combination of Church and social history the importance of the Quaker family and its structure in maintaining the central importance of religion in shaping Quaker lives from 1672 to 1786. This period, the author claims, is one in which the essential elements of Quakerism remained the same while Puritanism was changing. Frost explains this as due to an increasing realization that Friends were

declining in importance and could not hope to convert or change the world. Since the survival of Quakerism was seen not as resting on converting the world, Quakers made the religious instruction of children and marriages within the sect major preoccupations to combat what they considered the pervasive materialism of American life.

Frost believes that a crucial problem is to determine what were the unique Quaker attitudes that had to be protected from contamination as contrasted with general eighteenth-century Protestant views. Frost, for instance, believes that many Quaker beliefs about the position of women were those of their Anglo-American contemporaries. But the author devotes little time to explaining how acceptance of the subordination of women is compatible with the unique Quaker recognition of the equal religious worth of each individual and the right of women to preach. Frost contends that the Quaker wife, while having spiritual equality in the meeting and more freedom than the law allowed, acquiesced in her Puritan status as a member of the weaker sex.

Frost interestingly rejects quantification as helpful in the study of the colonial Quaker family stating: "While quantification has provided useful information on many facets of family life, the problems discussed here cannot be placed in numerical categories. Ideas about children, views of courtship, and the impact of religious values upon behaviour patterns can be best dealt with through literary sources."

Despite this viewpoint Frost uses his statistical evidence neatly and I found in his statistical data some of the book's most interesting illuminations.

Frost criticizes the other side of the "gentle Friends" who were so opposed to war, the plight of the Negro and Indian, the mistreatment of domestic animals, the torture of flies, and the agony of worms, yet were conservative and morally harsh to their own young members.

Thus, Frost sees the Society of Friends as ingrown with tribalistic meetings devoted to protecting children from worldly contacts and contamination in which their nuclear families had the primary role in the children's maintenance, especially in imparting occupational training.

Frost believes that Quaker influence on Amer-

ican life has been hampered because they refused to jettison their distinctive doctrines and practices such as pacifism and religious toleration. He rightly takes the position that Friends have endured in a hostile world because of the Quaker family's successful determination to inculcate their peculiar doctrines in their children.

ANNE PANNELL TAYLOR  
Easton, Maryland

RUSSEL NYE. *The Unembarrassed Muse: The Popular Arts in America*. (Two Centuries of American Life: A Bicentennial Series.) Reprint; New York: Dial Press. 1971. Pp. 497. \$3.95.

This is both an immensely impressive and extremely enjoyable book. Impressive because of its scope and depth: enjoyable because of the many nostalgic trips evoked by incursions into the popular arts of the past. But first Professor Nye must give his definition of popular art. This he does by contending that sometime during the eighteenth century a cultural revolution occurred parallel to and caused by the Industrial Revolution. As the middle and lower classes steadily gained more leisure time and disposable income they sought new ways to fill that leisure and spend that income. The result was the emergence of a new art—neither folk nor elite—but rather mid or mass culture, the popular art of the people. Although a clear line can be drawn between popular and elite art, the line is not so clear between folk and popular art. Thus popular art could be described as folk art "aimed at a wider audience."

This is the art whose history in America Professor Nye proposes to examine. He succeeds in that intention admirably, somehow managing to combine a huge amount of factual data with trenchant analyses and a flowing narrative. Beginning with fiction and poetry the author takes us through theaters and tent shows, minstrels and musicals, the dime novels and the comics, mysteries and detectives, science fiction and Westerns, blues, big bands, bluegrass and Beatles, and finally, radio and television.

In all this, but most especially in the chapters on fiction, Professor Nye successfully relates the art to the social upheavals and consequent needs of various periods. For example, the

shift in goals from Horatio Alger to the Merriwells at Yale reflect necessary changes in popular values. The Alger hero achieves wealth whereas the Merriwells seek only to win a game fairly played. The heady euphoria of the latter part of the nineteenth century was giving way to a more realistic assessment of possible goals. After the Merriwells came Tom Swift, who helped immeasurably to prepare America's youth for the technological age. But trying to give an example does not do justice to the scope of this book. In any given period the reader can find in the structure and form of popular entertainment a revealing view of the society itself.

The bibliography is also impressive. I am aware of only one serious omission—the author somehow got through a discussion of country music without referring to Bill C. Malone's *Country Music U.S.A.*, which I had thought was definitive, or at least indispensable. Maybe that is why the discussion of country music omitted any mention of Jimmie Rodgers, the "Singing Brakeman," who may well have been the most important of all country singers.

One other slight criticism: this book is too good for such fine print and stiff glue binding. It deserves a better production. But for the rest Professor Nye's book can only be praised. Any teacher of American social or cultural history will want this book for use in his courses and for his own edification and pleasure.

HENRY A. KMEN  
Tulane University

DAVID CURTIS SKAGGS. *Roots of Maryland Democracy, 1753-1776*. (Contributions in American History, number 30.) Westport, Conn.: Greenwood Press. 1973. Pp. xii, 253. \$12.00.

Professor Skaggs applies to Maryland a refurbished version of the old Carl Becker thesis on the American Revolution. In Skaggs's view Maryland had a class-structured deferential society that fought the Revolution "to see the enactment of what Carl Becker has called an 'internal revolution.'" And his first chapter is devoted to a condemnation of the opposing interpretation, the "so-called Brown Thesis," which maintains that a middle-class society fought the Revolution to preserve its stake-in-society democracy. He condemns the Browns

for improper use of social science techniques, for failure to define democracy and middle class, and for complete misunderstanding of the deferential nature of colonial society.

But, as with Carl Becker, so with Skaggs: his book fails to substantiate his thesis. He never cites contemporary evidence to prove deference, and he ignores abundant available contemporary statements from the Maryland records, defining democracy as "lodging the legislative power in the common people or persons chosen out of them," speaking of the "Lower House with their democratic spirit," or referring to the voters as "levellers in their principles" who chose "persons of their own stamp" to represent them. On the franchise, which according to neo-progressives was not really effective because of deference, Skaggs estimates that a half to two-thirds of the adult free white men could vote, but he does not do the demographic research to show how many men with less than fifty acres of land could meet the £40 property requirement, or whether tenants-for-life had the vote in Maryland as they did in Virginia. In a chapter, "The Rise of Radicalism, 1772-1776," Skaggs's "radicalism" turns out to be simply measures leading to independence, not internal class revolution, as one would expect. And finally, the Maryland constitution of 1776, which Skaggs considered as a step toward democracy, was actually the most conservative of all Revolutionary state constitutions, and it completely refutes the idea of internal social upheaval. True, voting requirements were changed from fifty acres or £40 sterling in property to fifty acres or £30 current money. But other provisions were even more conservative than those in effect before independence. Representatives needed £500, instead of fifty acres or £40; senators now were indirectly elected by electors worth £500, held office for five years, and needed £1,000; a council chosen by the legislature also needed £1,000; and the governor, chosen by both houses, had to have £5,000. As in colonial times, many local officials were appointed, not elected, voting was viva-voce, and the constitution, drawn up by a convention that functioned also as the legislature, was never submitted to the people for approval.

If what happened in Maryland was a Carl Becker "internal revolution," Skaggs has cre-

ated a need for a new definition of social revolution.

ROBERT E. BROWN

*Michigan State University*

JAMES KIRBY MARTIN. *Men in Rebellion: Higher Governmental Leaders and the Coming of the American Revolution*. New Brunswick: Rutgers University Press. 1973. Pp. xiii, 263. \$12.50.

Frankly viewing the Revolution from the top down, Martin examines the 487 men who comprised the "political elite" in America during the era of transition from colony to statehood. The elite are defined as high executive office-holders above the level of assemblymen. Using the technique of collective biography and cross-tabulating measurable common characteristics, Martin finds that the 256 members of the new Revolutionary elite were generally of less wealth, education, and family status than their 231 colonial counterparts. The former were also more inner-oriented than transatlantic in economic outlook (although drawn from the same mercantile-professional occupational groups as their predecessors), more often natives of their province than migrants from elsewhere, but not perceptively different in age or religious affiliation. Since the general turnover in these offices was over 77 per cent Martin concludes that the Revolution was caused by the desire of the lesser elite, largely lower executive office-holders and assemblymen, to secure entry into the ranks of the entrenched aristocracy. Blocked by an immobile political structure they became "men in rebellion," commencing in 1763, as the only viable alternative to remaining permanently frustrated politically. The Revolution was not a contest between upper and lower classes in the Becker sense but a struggle within the upper class. The commonality of citizens was used to satisfy the ambitions of the elite. The people's reward was broader participation in the political arena but not leadership: this continued to be reserved for the upper class.

If this is to be considered a major new interpretation of the causes of the Revolution it will require more than this brief book to support its contention and more than this short review to analyze it. The statistical data are interesting but inconclusive. The new elite seem remark-

ably like the old, still drawn from the wealthiest 10 per cent of the population. The 77 per cent turnover is deceptive, since those displaced were high royal appointees expected to remain loyal to the Crown. The data apply to office-holders within the states only and tell us nothing of the characteristics of the new leaders in the Continental Congress and Army. The chief weakness of the argument is the assumption that because the lesser elite filled the vacuum created by the departure of royal officialdom their driving ambition for such offices was not only a precondition, but also a precipitant of their revolutionary behavior. In fact many of the new Revolutionary leaders accepted such positions reluctantly, out of obligation not ambition. Martin's explanation reduces an event of epochal importance to the tawdry level of a competition for place among grasping politicians, ignoring those issues of principle, religion, morality, ideology, constitutionalism, and social philosophy that led not only the elite but large masses of Americans to become "men in rebellion."

MILTON M. KLEIN

*University of Tennessee,  
Knoxville*

RICHARD M. KETCHUM. *The Winter Soldiers*. (The Crossroads of World History Series.) Garden City, N.Y.: Doubleday and Company. 1973. Pp. 435. \$10.00.

Richard Ketchum's *The Winter Soldiers* is intended for the general reader. Based upon published source material and a careful reading of secondary works, it is so well written that it reads like a good historical novel. It has no footnotes with which to frighten away the general reader, and it is written in a fast-paced, exciting style that should hold every reader's interest.

In his pages Mr. Ketchum has told the story of the winter soldiers who marched and fought while many of their comrades drifted away to their firesides. The winter soldiers made up the brave band of officers and men who remained with General Washington after his army had suffered a series of defeats in New York and New Jersey during the fall of 1776. By the onset of winter the army had suffered such losses from combat, disease, and desertion as to be unfit for battle. Yet its general decided

that he must lead it into action in order to bolster its morale and that of America by winning some timely victories.

Appealing to his weary and poorly equipped soldiers to undergo hardships for the cause of independence Washington embarked upon a daring and strenuous winter campaign. He led his troops across the Delaware River to attack a regiment of Hessian infantry at Trenton. After surprising and defeating the Hessians during a sleet storm he withdrew his chilled and weary soldiers to Pennsylvania. Within a few days he renewed his winter offensive; on his second thrust into New Jersey he escaped from a British army led by Charles, Lord Cornwallis, at Trenton, and marched over icy roads to fall upon a small but elite British force at Princeton. At the college town he won a brief but bloody battle. And with his victories at Trenton and Princeton, according to Ketchum, he and his tiny army saved America from defeat.

It is fair to say that Ketchum has added little to our knowledge of the campaign of 1776 that was not known by Douglas Southall Freeman when he wrote his monumental biography of George Washington. But Freeman wrote a detailed, scholarly work heavily freighted with footnotes. And it is to be feared that few people, other than scholars, will read Freeman's heavily documented pages today. Given the circumstances, there should be a market for Ketchum's *Winter Soldiers*. It is written specifically for the general reader, and it tells an exciting story so well that it deserves a large audience.

GEORGE W. KYTE

*Northern Arizona University*

HARRY AMMON. *James Monroe: The Quest for National Identity*. New York: McGraw-Hill Book Company, 1971. Pp. xi, 706. \$12.95.

There are so few biographies of James Monroe it is no wonder that students sometimes think of Monroe as a "doctrine" rather than a president. The penultimate biography was published in 1946, fifteen years after the death of its author, W. P. Cresson. Now Harry Ammon has published *James Monroe: The Quest for National Identity* after fifteen years of research.

Obviously a Monroe biography presents special problems. Materials on the man, as distinct

from the statesman, are so sparse that a Monroe biographer who is unwilling to deal solely with Monroe's political career faces an extraordinary burden. For example, even after fifteen years of active research, Ammon can present his knowledge of Monroe, to age fifteen, and his ancestors in just the first two pages of the book. Four more pages, and Monroe is eighteen. The year is 1776 and Ammon can speak at length about military events with which Monroe was sometimes only peripherally associated.

Because of Monroe's penchant for privacy it is doubtful that a full picture of the whole man can ever be written. This may be as Monroe would have wanted it for, from what Ammon can learn, Monroe was modest and shy, lacking in brilliance, and slow in thought at eighteen (p. 8), and he was unchanged at seventy, still "modest, diffident, quiet and, as always, rather awkward in his movements" (p. 549). Fortunately Thomas Jefferson, and eventually the nation, also recognized his personal warmth and kindness, his unfailing readiness to sacrifice personal interests for public service, and his phenomenal capacity for work that more brilliant contemporaries found tedious.

Monroe was a significant part of so many crucial events in our formative years that we must learn what we can of him, even if it is less than we would like to know. Once Monroe plunges into politics Ammon provides a splendid three-dimensional portrait of the statesman in action.

Ammon argues persuasively that Monroe's role in the successes that we associate with him was larger than has generally been recognized and that his role in the Monroe failures has often been misinterpreted or magnified. Prime examples of the former are his presidency in general, his astonishing successes in diplomacy that have been too readily credited to Secretary of State John Quincy Adams, and the "good feelings" that resulted in part from Monroe's assiduous wooing of the opposition Federalists. Monroe's achievements as president were so little regarded by contemporaries that when he died eulogists spoke almost exclusively about his military career of a half-century before. Examples of Monroe's failures, with which Ammon must contend, are his aborted diplomatic missions, his silly and misguided presidential candidacy in 1808, and his opposition



to the Constitution in 1788. More often than not it was extreme sensitivity to criticism that prevented him from resolving minor differences before they became huge ones.

Ammon also emphasizes Monroe's largely overlooked record in the Confederation Congress, where he consistently espoused nationalism and a national army, was unique in his knowledge of, and concern for, the West, and single-handedly developed the governmental structure for territories. During Jefferson's first administration Monroe brought such executive talent and hard work to the governorship of Virginia that he far out-performed "bigger" men—Thomas Jefferson and Patrick Henry—who were distinctly poorer governors.

DONALD O. DEWEY  
California State University,  
Los Angeles

JOHN S. WHITEHEAD. *The Separation of College and State: Columbia, Dartmouth, Harvard, and Yale, 1776-1876*. (Yale Publications in History: Miscellany, 97.) New Haven: Yale University Press, 1973. Pp. x, 262. \$10.00.

Whitehead's immediate concern is to describe the governmental relations of four colonial colleges in the century after independence, but his larger endeavor is to argue that the distinction between "public" and "private" foundations did not exist in American higher education before the late nineteenth century. This proposition is not new for the seventeenth and eighteenth centuries, but it has never been pushed so far into the national period nor used to destroy so many textbook categorizations. As a consequence Whitehead's little book is not only a useful compilation of specific college-state relations, but a piece of "vigorous iconoclasm" (p. ix), not altogether convincing, but nonetheless ingenious and provocative.

Up to 1820 Whitehead finds a variety of "alliances" among his four colleges and their respective states. Between 1820 and 1850, he notes their "estrangement": state representatives still sat on most governing boards, but state aid was largely choked off by the competing and now organized claims of the common schools, by criticisms of the colleges for being small, aristocratic, and sectarian, and by the colleges' commitment to a seemingly impractical classical curriculum. But between 1850 and 1865, ac-

cording to Whitehead, the "bond of faith" between the colleges and states was unexpectedly renewed by a few philanthropists who not only paid to diversify the curriculum (as in the Lawrence, Chandler, and Sheffield scientific schools of the three New England colleges), but helped to channel state aid, especially from the Morrill Act, to their colleges. Finally, in the post-Civil War decade, the campus and the statehouse formally separated as the "alumni movement" provided a group to take the place of most state representatives on the college governing boards. Yet, even in 1876, the contrast between "public" and "private" institutions of higher learning was "only a budding idea," waiting to be developed by President Eliot of Harvard and others whose celebration of the independent college or university has misled historians ever since.

While the colleges sought public support throughout Whitehead's period, the states supposedly favored the alliance to assure the proper training for future legislators. Yet the democratic politics of Jacksonian America largely undermined any special deference to college graduates, and the additional concern of the state to uphold an established Church, and the collegiate source of its ministerial recruits, was checked by the denominational complexity of mid-eighteenth-century America, even before the opening date of this study. Indeed, it can be argued that the main transformation of the earliest colleges from quasi-public to private status occurred, not in Whitehead's period, but in the century between the Great Awakening and the common-school movement. Given this different perspective the independent position of other colonial foundations such as Brown and Princeton is understandable, the special meaning of incorporation in early America can be discerned, and the legal simplifications of the Dartmouth College decision are plausible.

If Whitehead is unduly critical of Harvard for being Federalist, of Yale for being Congregational, of Dartmouth for being a "small college," and of Columbia for having no identity at all, it is because he is convinced of the full viability of the college-state alliance throughout this period where others might only see years of unresolved confusion. Perhaps his assertions so often begin with phrases such as "strangely enough" and end with exclamation marks not merely

because his facts refute the anachronistic "public" and "private" schema of others, but because they run against his own attenuated hypothesis. Yet for all these and other difficulties, his book is a refreshing, insightful essay.

JOHN HOFFMANN  
*Michigan History*

RICHARD TREGASKIS. *The Warrior King: Hawaii's Kamehameha the Great*. New York: Macmillan Publishing Company. 1973. Pp. xxiii, 320. \$10.00.

Tregaskis, the renowned novelist and war correspondent, authored several books, including *Guadalcanal Diary*, *Seven Leagues to Paradise*, *Vietnam Diary*, *X-15 Diary*, and *John F. Kennedy and PT-109*. He dedicated his final effort "to the mana of Kamehameha, the Napoleon of the Pacific, that gave me the power to write this book," which appeared the year of the author's death.

In this biography, sketched on a broad canvas with the Hawaiian islands of the late eighteenth and early nineteenth century in the background, Tregaskis refers to the amorous proclivities of a giant resulting in twenty-two wives or concubines and fifty children and his superhuman physical prowess in tilting the mammoth Naha Stone, which traditionally could only be moved by a nobleman of the Haha caste, and his ability in triumphing over his powerful rivals. The author emphasizes the shrewdness of Kamehameha in impressing visiting white captains and fur traders that he was blameless in the killing of Captain James Cook and in cultivating their friendship, thus acquiring some of their weapons—particularly cannons—that proved decisive in battle. Thus we see the acumen of a man who cultivated the friendship and adopted some of the tools and methods of foreigners, yet clung to the mores of his native land.

*The Warrior King* contains a sixteen-page album, a four-page unannotated bibliography, a short commercial type index, no quotations, and no footnotes. Although this is the first full-length adult biography of Hawaii's greatest and most powerful ruler, it is not necessarily definitive. However, being partially analytical, it represents a worthy addition to the literature of the Napoleon of the Pacific.

MERZE TATE  
*Howard University*

H. SHELTON SMITH. *In His Image, but . . . : Racism in Southern Religion, 1780-1910*. Durham, N.C.: Duke University Press. 1972. Pp. x, 318. \$8.50.

This carefully researched, well-organized, and well-written study tells in fascinating detail the depressing story of racism among the leaders of Southern religion between 1780 and 1910. Beginning his account at a time when at least some of the churches—in addition to the Quakers—professed an antislavery position, Professor Smith chronicles the churches' swift turnabout (within twenty years), thus further demolishing the hoary myth that it was the villain Garrison and his abolitionist cohorts that had killed a flourishing antislavery movement. It is hardly surprising, then, that as pressures on the South increased, its church leaders were found in the forefront of the defenders of the peculiar institution, were able to enlist God on the side of secession and, after defeat, discovered Him as the author of segregation.

Although Professor Smith refrains from excessive moralizing and allows the dismal record to speak for itself, the title of his book is a constant reminder of what he clearly regards as a tragic paradox. I am not so sure I agree. Professor Smith's perspective appears to be shaped by the underlying assumptions of liberal Protestantism. In his view the Southern churchmen cannot be called truly Christian because they were in conflict with the Biblical anthropology of the *imago Dei*. If "God created man in his own image," then it follows "that all men are equal in the sight of God, and that they therefore owe one another equality of respect and goodwill." But this logic, I believe, has not always been self-evident. For more than a millennium and a half the Church (and I include Protestantism) used the authority of Saint Paul to argue that equality in the sight of God was beyond time, making social, economic, or even racial inequalities among men mere temporal ephemera. Salvation was after all for the soul and not the body. Religion could thus effectively serve the interests of the ruling class.

Why Southern churchmen, specifically, were so eager to serve the slaveholders is a question Professor Smith has left unanswered. I raise this issue with some diffidence because of my strong sympathies for the much-beleaguered discipline of intellectual history. Surely, I do not advocate

a return to a simplistic social or economic determinism that reduces ideas to a frothy foam floating on the stream of history. It seems to me, however, that what is now called for is a sophisticated approach that tries to come to terms with the complicated and seemingly intractable relationship between ideology and culture.

KLAUS J. HANSEN  
*Queen's University*

CHARLES COLEMAN SELLERS. *Dickinson College: A History*. Middletown, Conn.: Wesleyan University Press. 1973. Pp. xvii, 626. \$20.00.

Anyone who undertakes to write the history of a college faces fundamental problems of focus and emphasis: should the book be addressed to those who share a personal interest in the school's life or to scholars concerned with broader issues in the development of education? Mr. Sellers has attempted to resolve this dilemma by presenting something for everybody, but the final product of his efforts more closely answers the needs of alumni and friends of the college than of the wider scholarly community. The subject of his study is a small liberal arts school in Carlisle, Pennsylvania, that began as a colonial, old-light Presbyterian academy. Supported by such important figures in Pennsylvania as John Dickinson, for whom the college was named, and Dr. Benjamin Rush, the school initiated a collegiate department in 1783 when it was chartered as Dickinson College. Plagued with financial difficulties and internal sectarian squabbling the college survived until 1816, was reopened in 1821, and was finally forced to close in 1832. Two years later the Baltimore and Philadelphia Methodist conferences agreed to revive the school. Although they proved more successful at attracting both money and scholars than the Presbyterians had been, the college remained small and conservative into the 1960s. The classics requirement was not dropped until 1946; compulsory chapel lasted until 1965. Women were first admitted in the 1880s, but were long subject to an enrollment quota of 25 per cent of the total student body.

This lengthy narrative history of the college provides a detailed account of every stage of Dickinson's development down to 1970. Written from a remarkably well preserved collection of

early college records, diaries, and other manuscript materials the book's chapter divisions are based upon the administrations of its various presidents. The author's sensitivity to the personalities, aspirations, and administrative styles of the college's past leaders is, to a reader not otherwise familiar with Dickinson, the book's most intriguing quality.

The chief fault of the book is its lack of analysis. For example, after showing how in the eighteenth century Dickinson was the equivalent of Princeton and the University of Pennsylvania, Mr. Sellers fails to explain why in the course of the nineteenth century these colleges became great universities while Dickinson did not. Because there is too little attention paid to the comparison with developments in other American colleges, the book with all its massive detail has a distinctly parochial quality. Sons and daughters of Dickinson will, however, be well served by this carefully researched, interestingly written, and well-illustrated volume.

CAROL HOFFECKER  
*University of Delaware*

JEROME MUSHKAT. *Tammany: The Evolution of a Political Machine, 1789-1865*. [Syracuse:] Syracuse University Press. 1971. Pp. xii, 476. \$15.00.

With a sure knowledge of his subject Jerome Mushkat illustrates how the New York Tammany Society, a nonpolitical, patriotic, and fraternal order founded in 1787, evolved into the political organization that went on to control New York City's government for much of the nineteenth and twentieth centuries. He concludes his study at 1865, with "Boss" William Marcy Tweed firmly entrenched as undisputed leader of the political machine and therefore of the municipality. Mushkat describes in careful detail how Tammany's incipient neutrality, buffeted by the Gallomania and Jeffersonianism of its early leaders, soon moved the order toward a partisan stance. By the Jacksonian period the society became so popularly identified with the Democratic party that eventually the Tammany designation for public office was considered the only legitimate party choice. As again and again the organization proved its stability and vote-getting strength, intensive internecine struggles for control and influence surfaced; such internal bloodletting among the various sachems (lead-

ers) and braves (members) became particularly severe during the party upheavals and realignments of the Monroe and John Quincy Adams administrations and during the 1850s and Civil War ear.

It is to Mushkat's credit that he is able to keep track at all times of the several factions and personalities vying for power within Tammany, especially as the factions had the confusing tendency to divide into subfactions. For example, during the 1820s the "Wigwam" sheltered under its roof representatives of such political denominations as Crawford Regulars, anti-Van Buren Republicans, Burrites, Matthew Davis Burrites, Rufus King High-Minders, anti-Adams High-Minders, Clintonians, Swiss Federalists, Clayites, nationalists, and anti-Masons. There are good sketches of the better-known politicians involved with Tammany, from the nationally prominent Clintons (George and De-Witt), Aaron Burr, and Martin Van Buren to such powerful local figures as Matthew Davis, Mordecai Noah, Fernando Wood, Lorenzo Shepard, and Elijah Purdy. Along the way, Mushkat justifiably inters some myths about Tammany. Burr never controlled the organization; in fact, the society would have preferred George Clinton over him as Jefferson's 1800 running mate. Behind its egalitarian pretensions, a Tammany clique met privately and set party policy. Underneath its professed support of immigrants lay a strong xenophobia, especially toward Catholics; Mushkat does a masterful job in this area as he wades through the controversy involving Governor William Seward and Tammany over the city's public school system.

In a work of impeccable scholarship Mushkat has provided a rich and extremely useful starting point for students of urban political history in the United States.

IRVING KATZ  
Indiana University,  
Bloomington

RICHARD E. ELLIS. *The Jeffersonian Crisis: Courts and Politics in the Young Republic*. New York: Oxford University Press. 1971. Pp. xii, 377. \$9.75.

BERNARD SCHWARTZ. *From Confederation to Nation: The American Constitution, 1835-1877*. Baltimore: Johns Hopkins University Press. 1973. Pp. xi, 243. \$10.00.

In his rereading of the judicial battles of the Republican years, Richard Ellis is able, as few other historians have been, to meld national politics with state politics and to probe the judiciary issue for all it can reveal about the intensity of postrevolutionary politics and the development of the legal profession. In defining and redefining the structures of their courts, Americans were also defining the ways in which they thought about the rule of law, the responsiveness of courts to popular opinion, and whether an intellectual elite was possible in a popular republic. Ellis argues that in Kentucky, Pennsylvania, and Massachusetts, as well as in Congress, the intricacies of the debate over court reform reveal a pattern shaped by a fear of "power, and its potential for misuse" (p. 124). The radical programs, different though they were in specific detail, had in common the desire to bring judges under direct popular controls, a distrust of lawyers, and an insistence that the common law was an aristocratic import incompatible with plain republicanism. This skepticism of law, though often articulated in a sophisticated way, could easily drift into crude anti-intellectualism. Moderates of both parties were held together by a desire to protect the public against "the turbulent and the restless" and to create an efficient, dependable, and fair system of justice.

Since Henry Adams, nearly every observer of the Jeffersonian years has commented on the willingness of Republicans to maintain Federalist programs. Where Adams argued that Republicans were forced by circumstance to become, in effect, Federalists, Ellis argues that their decisions grew naturally out of the moderate Jeffersonian view of the world. If Ellis reactivates "consensus history" by emphasizing that Jefferson and Marshall were not as far apart as has usually been assumed, he does so only by way of an energetic and frankly neo-Progressive insistence that true ideological division did exist in the early Republic, located *within* the Republican party. For Ellis the strategic political division in the Jeffersonian years is less the Republican/Federalist split (whose continuing importance he does not deny) than the division between radical and moderate Republicans.

Only when he comes to identifying other rivalries that were part of the struggle over the ways in which the Revolution was to be fulfilled

does Ellis begin to grope. He relies on vague categories like "the democratic-minded" and the "commercial-minded" (p. 252); the end of the book is lamely phrased. Moreover, nearly everyone in the early years, from Shaysites to Hartford Convention Federalists, can offer a persuasive and real claim to being, by their lights, the true inheritors of the Revolution. Ellis offers an overly simplistic construction of the Federalist schism: ultracommercial, pro-British, elitist High Federalists versus moderate Adams Federalists. It is a scheme that places Oliver Wolcott, for example, squarely among the unyielding High Federalists for his opposition to repeal of the Judiciary Act of 1801 but ignores Wolcott's own view of himself as a bridge between the Adams Federalists and the Hamiltonians and ignores the fact that he would himself later join the Democratic-Republicans.

These reservations aside, I end with great admiration for this book and for the strength of Ellis's case that in the judiciary crisis was forged a strong middle and right-of-center consensus powerful enough to drown out the radicals, and so explains the fragility of American Jacobinism. In passing, he has added to our reasons for insisting that the years 1776-1815 be treated as a unit, a generation troubled by issues and definitions with a single theme.

At the beginning of *From Confederation to Nation: The American Constitution, 1835-1877*, Bernard Schwartz offers a similar promise: that he will treat "the four decades after Marshall's death . . . as a virtual continuing constitutional convention, during which a second Constitution developed and took its place side by side with the Constitution of 1787 and the Bill of Rights of 1791" (p. x). But after a lively introduction to the personnel of the Taney Court, the book drifts into a series of brief summaries of cases and issues. These summaries are straightforward and often of substantive interpretative interest (those on Taney's concept of due process, for example). Schwartz restates his objections to Stanley Kutler's reading of the significance of the Chase Court, and continues his own defense of Taney's judgment in *Ex parte Merryman*. The book does not, however, sustain the coherent thesis promised in the introduction.

The purpose of the book, which reads like a textbook but is offered as an interpretation, remains unclear. It is also disquieting that no

mention is made of the extent to which it depends on Professor Schwartz's previous books. From chapters 4 to 6 of *The Reins of Power* (1963) a number of close paraphrases and direct duplications are drawn, without footnote acknowledgment. From the volumes of *A Commentary on the Constitution of the United States* come a substantial number of unacknowledged duplications. Portions of part 1 (*The Powers of Government* [New York, 1963], 2: 213, 249-50) turn up in the new book at pages 138-40; from part 2 (*The Rights of Property* [New York, 1965], 280-82) come most of pages 9-11. Certainly an author has the option of quoting himself, and certainly there would be use for an epitome of Professor Schwartz's many volumes on constitutional development. But it does seem to me that a summary of previously published materials ought to be so identified.

LINDA K. KERBER

University of Iowa

EDWARD K. SPANN. *Ideals & Politics: New York Intellectuals and Liberal Democracy, 1820-1880*. Albany: State University of New York Press. 1972. Pp. xiii, 269. \$10.00.

In this "group biography," Edward K. Spann has written of the "interplay of human personalities," as represented by native New Yorkers Gulian C. Verplanck, William Leggett, and James F. Cooper, and transplanted Yankees William C. Bryant (the main protagonist), members of the Sedgwick family (especially Theodore III), and the New Jerseyite Parke Godwin (Bryant's son-in-law), as they participated in the development of an "already established liberal democratic tradition" of equality of opportunity and the dogma of rule by the people. Whether such a novel approach (the author also desired to emulate C. P. Snow) is more effective than the more traditional one man, one biography, is debatable, but that the author has given his reader an excellent review of the complexity of nineteenth-century New York society in a well-written, well-researched volume is without question.

At the start Bryant is introduced debating a career in law or poetry. His early poems aided by encouragement of the Sedgwicks were artistic successes, but financial failures. He received a

total of \$14.92 for his 1821 *Poems*. In court he received no justice, losing a case in the Massachusetts Supreme Court on a minor technicality. Bryant sought sanctuary in New York City and there found fame and fortune in the *Evening Post* and the friendship of kindred spirits speculating on an ideal free society in which the interests of the individual and community could be realized, a society based on equality, stability, harmony, and morality. The conservative Verplanck and Cooper shared this vision with the liberal Bryant, Leggett, and the Sedgwicks. At times cooperation was evident as in support of Jackson and in opposition to the tariff, while often differences arose over the bank, among other issues.

There are excellent chapters on the fears of Cooper and Verplanck over dangers to a free society of "selfish materialism" and the possible development of a class of privileged corruptors exemplified by a burgeoning "pressocracy." This is balanced by the optimism of Jacksonian *laissez faire*ism, as voiced by Godwin, Bryant, and the short-lived but interesting radical, William Leggett. The group as a whole, Mr. Spann feels, provided little that was new. They generally embellished the virtues of hard work, individualism, and free-trade capitalism.

The surviving members, Bryant and Godwin, involved in the reforms of the 1870s, which followed revelations of corruption, still insisted on the efficacy of the natural law and limited government. The author concludes that they may have served to hamper the efforts of modern liberalism in developing a role for government as a balance against corporate power. While providing a good overview of major themes in New York history, some important issues, for example, those concerning urban affairs, are not discussed.

Such seemingly mundane subjects as water supply, parks, ferries, and the host of such problems facing the emerging city did require solutions and were related to political and social ideas and ideals and received often close attention by members of the group. What did Bryant and Cooper feel about the temperance question, which became a center of bitter controversy reflecting changing social values? A look at the bricks and mortar of society with the same thoughtful scholarship as was devoted by the author to more lofty issues could have been

extremely interesting and rewarding. One of the few errors made: Philip Hone was appointed, not elected mayor in 1825. What was Bryant's thought on the desirability of having the office made an elective one as it was in 1834? It should be noted, however, that this kind of fault finding does not detract from a well-done effort, but it does make book reviewing all the more intriguing.

LEO HERSHKOWITZ  
Queens College,  
City University of New York

FRANK MCNITT. *Navajo Wars: Military Campaigns, Slave Raids, and Reprisals*. [Albuquerque:] University of New Mexico Press. 1972. Pp. xii, 477. \$15.00.

In the preface the author states: "In writing this book my principal concern has been to determine the underlying causes of the hostilities that led to almost continuous warfare between the Navajo Indians and white colonizers of the provinces and territory of New Mexico. Inevitably, a corollary concern was with the treaties that punctuated the temporary lulls between periods of fighting."

The author succeeds in identifying two major underlying causes: first, that in retaliation for Navajo raids, white men attacked the Navajos to capture prisoners to use as slaves; second, that after 1846, American settlers encroaching on Navajo lands constituted an issue that was "serious though not dominant." In identifying these causes the struggle between the Navajos and the white man has been documented thoroughly from its beginnings through the massacre at Fort Fauntleroy in September 1861; the dust jacket bears the welcome information that Mr. McNitt plans to add to this history of the Navajos.

Although it is uncertain when the Spanish authorities began to trade in Indian slaves, the practice was widespread by the last decade of the seventeenth century, and whites continued to capture Navajos to use as slaves until the Navajos were sent to the Bosque Redondo. But the Indians also took prisoners, both whites and Indians from other tribes, and made them slaves. Almost from their first contact warfare (including the taking of captives for slaves) characterized the relationship between Navajos and

whites so that there developed a rather regular pattern of Indian raid, white reprisal campaign, peace council, and treaty. Sooner or later the treaty was broken, usually by an Indian raid, and the cycle was repeated.

Even though it is true that slave raids by both Indians and whites and land encroachment by whites were partly responsible for more than a century of fighting, the real cause appears to have been the totality of the confrontation between two different cultures that had very different ethical traditions and value systems.

*Navajo Wars* is a well documented, detailed military history and contains, therefore, considerable information about military movements and campaigns. The only map in the book is the Miera y Pacheco map of 1778, which appears on the end papers. But because there is no other map in the book, unless the reader is familiar with the geography of a large part of New Mexico and a good slice of eastern Arizona he will be just as confused about where things are as were some of the troops that ventured into the Navajo lands.

CLYDE C. WALTON

*Northern Illinois University*

WILLIAM GRAVELY. *Gilbert Haven, Methodist Abolitionist: A Study in Race, Religion, and Reform, 1850-1880*. Edited by the Commission on Archives and History of the United Methodist Church. Nashville: Abingdon Press. 1973. Pp. 272. \$8.95.

In the first full-scale biography of Gilbert Haven since 1883, William B. Gravelly provides a detailed presentation of the life and ideas of a nineteenth-century religious and social leader whose acts command our respect. From his conversion to Methodism in 1839, while a student at Wesleyan Academy, until his death in 1881, Haven struggled to articulate the social consequences of his religious beliefs. In the pre-Civil War years, when he served several Massachusetts churches, this led him to a vigorous espousal of abolitionism. In the years after the Civil War, as editor of the influential Methodist weekly *Zion's Herald*, and finally as a bishop of his church stationed in Atlanta, Georgia, he attracted national attention by his firm expression of the belief that if all men were equal before the Lord, they could not be unequal in society or politics. Within his Church this led him

to bitterly oppose the tendency to separate churches and conferences on racial lines, a struggle he would ultimately lose. In politics it made him an outspoken opponent of Andrew Johnson and a strong supporter of Grant and Reconstruction. He totally opposed all forms of segregation—social, religious, or political—and called for full acceptance of the freedmen as equals. These extreme views, which included a willingness to tolerate interracial marriage, shocked his contemporaries. But Haven continued to propound them, not only from the safety of his Boston editorial pulpit, but also after his elevation in 1872 to a bishopric of the Methodist Episcopal Church with the duty of supervising the Southern conferences.

Mr. Gravelly's careful, detailed description of the activities of Haven, drawn from extensive research in primary sources, and fully annotated, gives us a useful description of what Haven said and did. He is less successful in bringing the man to life, which will make his book of greater interest to advanced students than to undergraduates or the general reader. However, the ideas and acts of Haven are of more importance than his personality, and Gravelly does give us them. His book well deserves the Jesse Lee Prize of the Commission on Archives and History of the United Methodist Church, which it won in 1970.

MILTON BERMAN

*University of Rochester*

JAMES F. PEDERSEN and KENNETH D. WALD. *Shall the People Rule? A History of the Democratic Party in Nebraska Politics, 1854-1972*. Lincoln, Nebr.: Jacob North. 1972. Pp. xix, 449.

At first glance this book's somewhat ostentatious title suggests that it is democratic party propaganda issued under the guise of a serious effort to depict and explain the Nebraska Democracy's none-too-successful history. This suspicion remains with the reader throughout the book, but is allayed by the authors' efforts to explain and analyze rather than apologize. Although financed by the state's Democratic Central Committee, it is a surprisingly reliable addition to Nebraska historiography. The question-title *Shall the People Rule?* is not intended to identify the chief motivation of Nebraska's Democratic party. "Shall the people rule?" was the battle cry of

William Jennings Bryan's third presidential bid in 1908 and best describes a Populist-reform and largely minority element in the state's dissension-ridden Democratic party.

The rifts in the state party are apparent among the men that it raised to national prominence, such as J. Sterling Morton, William Jennings Bryan, Gilbert Hitchcock, Arthur Mullen, and Charles Bryan. Through the years these men tore the party apart with personal disputes and conflicting support and opposition to laissez-faire government, fusion, gold and silver standards, prohibition, the war, and the New Deal. The authors identify two early wings of Nebraska's Democratic party in the struggle between the goldbug, Bourbon Democrat J. Sterling Morton and the silverite-reform-fusionist Democrat W. J. Bryan in the 1890s. A division of similar proportions continued into the twentieth century when the Bryan wing confronted the Gilbert Hitchcock and Arthur Mullen faction of the party from Omaha over the issues of prohibition and machine control of the Nebraska Democracy. Under "Brother Charley" in the 1920s and early 1930s the Bryan wing of the party ironically abandoned activist government for a reactionary anti-New Deal stance at a time when the national party moved in the opposite direction.

This points up the modern dilemma of state Democrats who wish to identify with the reform impulses of the national party. To capture state offices Nebraska Democrats often stand to the right of their Republican opponents to the extent that the Republican party from Sam McKelvie in the 1920s to Norbert Tiemann in the late 1960s has been the state's innovative and activist governmental party. When Charles Bryan said in the 1931 depression and drought that Nebraska would take care of its own, he struck a keynote of the state party in its opposition to national government relief programs and centralized federal government power.

At best this book resembles a long and well-written, graduate-level research project, but it also has many of the shortcomings of a graduate seminar paper completed in a limited time period. On many questions of national concern the work fails to consult all available sources and interpretations. While other studies discount Bryan's decisive role in nominating Woodrow Wilson in the 1912 convention, these authors

contend that "Bryan had demonstrated phenomenal survival ability in almost single-handedly picking the 1912 nominee" (p. 184). On the other hand there is thorough utilization of local state sources for this history, especially theses and dissertations at the University of Nebraska. This book makes a welcome contribution to the overall understanding of Nebraska's sometimes incomprehensible Democratic party. It will be a work that students of Nebraska history should consult, but consult critically with the knowledge that the authors are at times less than professional in their approach.

WILLIAM D. ROWLEY  
University of Nevada,  
Reno

WILLIAM KAUFFMAN SCARBOROUGH, edited, with an introduction and notes, by. *The Diary of Edmund Ruffin. Volume 1, Toward Independence: October, 1856-April, 1861.* (The Library of Southern Civilization.) Baton Rouge: Louisiana State University Press. 1972. Pp. xlviii, 664. \$20.00.

This is an outstanding volume and, as Avery Craven notes in his foreword, "Its publication has long been overdue." Edmund Ruffin, Tidewater Virginia planter, agricultural scientist, writer, editor, and militant secessionist, began this voluminous diary in October 1856 for two purposes, to maintain the sharpness of his pen and to occupy the leisure hours that followed his retirement from active plantation management. He wrote almost every day—this volume takes the story through the firing on Fort Sumter—for the rest of his life, which he took with his own gun in the spring of 1865 when his beloved Confederacy lay in ruins. The result is a massive, articulate, perceptive, and revealing documentary on Southern civilization on the eve of and during the Civil War, which might well surpass anything comparable yet in print.

Distrustful of democratic government, a devoted defender of Negro slavery, Ruffin became convinced following the "civil war" in Kansas in the mid-1850s that there was little hope for the North and South to live peacefully together much longer. Consequently, he devoted all of his energies to educating the South that separation was the only answer. He wrote numerous pamphlets and letters-to-the-editor, he traveled far and wide, and he either spoke to or corre-



sponded with countless persons in high places, seeking to convince the South of the folly of cooperation. But few people seemed to be listening, and by 1859 Ruffin was sunk in gloom. Then providentially, John Brown struck at Harper's Ferry, and the adrenalin again began to flow. Now he had an audience, now he was sought out, now he was honored wherever he went.

Thirsting for secession, and war if necessary, Ruffin was at the center of nearly every major event in the year before the conflict commenced. He wore the uniform of a Virginia Military Institute cadet at the hanging of John Brown, he attended the Democratic party conventions of 1860, he was an invited guest on the floor of the secessionist conventions of South Carolina, Florida, and Virginia, and he wore the uniform of the Palmetto Guard at Cummings' Point, where he fired the first shot at Fort Sumter. Ruffin played a conspicuous role in preparing, conditioning, and influencing the South for separation.

Professor William K. Scarborough has done a solid editing job, although the frequent use of brackets—for manuscript page number and missing first names—is distracting, and perhaps too much space is devoted to agricultural science. Then, too, a map showing the various Ruffin plantations would have been helpful. But these are very minor blemishes on a major contribution to the literature of the South and the Civil War. Volume 2 of the Ruffin diary is eagerly awaited.

EUGENE C. MURDOCK  
Marietta College

LOUIS R. HARLAN. *Booker T. Washington: The Making of a Black Leader, 1856-1901*. New York: Oxford University Press. 1972. Pp. xi, 379. \$10.95.

*The Booker T. Washington Papers*. Volume 1, *The Autobiographical Writings*, edited by LOUIS R. HARLAN and JOHN W. BLASSINGAME; volume 2, 1860-89, edited by LOUIS R. HARLAN et al. Urbana: University of Illinois Press. 1972. Pp. xl, 469; xl, 557. \$15.00 each.

I want desperately, I dissemble shamelessly, I preach the white man's gospel eloquently, and therefore I am. This is the Booker T. Washington who emerges from the initial two volumes of a projected fifteen-volume edition of his

private papers, and from Harlan's biography, the only three-dimensional study of a black leader in all of our vast historical literature.

In searching for the dominant ethos of the secretive, sometimes contradictory Alabama educator who became a black power broker for the nation, one discerns pre-eminently a business man—down to the last henhouse joke he told at the fundraising dinner. Good segregated race relations was good business. Civil rights bills were less important than “throw[ing] our force to making a business man of the negro.” While other black leaders have idealistically cried, “Let My People Go,” “Up Ye Mighty Race,” and “I Have a Dream,” Washington pragmatically counseled, “Cast down your bucket where you are.” Rather than denounce the outrages inflicted on his people, Washington characteristically took the dollars and cents approach. If Negroes were lynched in the winter, he said, people could not expect to have reliable labor in the summer. Washington was nothing if not a realist.

Most of his correspondence carries the emotional power of a bill of lading, and, similarly, money is usually the item of interest. He was forever going on a “money trip,” always in “urgent need of a good big sum,” to run his Tuskegee Institute empire. Utter exhaustion was seemingly the only way to slow him down, momentarily. On such rare occasions he was terribly bored. Through much of his European trip, paid for by white benefactors, Washington slept fifteen hours a day.

In the little private life he allowed himself, he appears to have valued his first wife largely for her “extreme neatness in her housekeeping and general work,” his talented, loving second wife for her abilities as a fundraiser. His final marriage Harlan pronounces a “practical bourgeois contract.” Notwithstanding his grand public recompense from his Atlanta Compromise Address, from his dinner at the White House, both of which Harlan definitively examines, underneath he seemed to wear a hair shirt. What a life sentence: to be white-controlled, Victorianly-tortured, missionary-motivated, power-propelled, and educated and black in Alabama.

His autobiography, *Up From Slavery*, is public relations at its most manipulative. Indeed, it was partially composed by a white journalist hired by Washington to heighten his image. The

book espouses a philosophy of "there's always room at the top," and has Washington, for once with candor, saying he had "high regard for the man who could tell me how to succeed." Those who told him how were white entrepreneurs. General Lewis Ruffner, who owned furnaces, mines, and farmland, let the youthful Booker be his houseboy and permitted his efficient wife to teach the youngster the joys of hard work, order, and cleanliness. For the rest of his life Washington equated civilization with a toothbrush. Most influential was General Samuel Chapman Armstrong ("the noblest, rarest human being I ever met") who as Christian padrone of Hampton Institute sought to mass produce conciliatory Negroes, who would eschew politics and make themselves indispensable to Southern whites in "hand, head, and heart." Washington shaped his career, school, social outlook, and the "very cut of his clothes" after Armstrong's example. And there was William H. Baldwin, vice president and general manager of the Southern Railroad, who became Washington's most intimate white adviser. "I almost worship the man," Baldwin once said of him. Well he should. White men like Baldwin, Rockefeller, Carnegie, Roosevelt—the list is long—created Washington, made him the sovereign lie they needed to help square their vulnerable consciences with their enterprising appetites. Washington was decidedly in on the creation. For as Harlan demonstrates, "He was willing to get what he could any way he could." Thus, he became the master of the mask, whom Stanley Elkins could have studied with profit.

This is a biography of industry, intelligence, and equanimity. Harlan's Washington is a "man of action" whose purpose was power and whose strength was stratagem. The interpretation is more cautious than bold, occasionally more tell than show (where, for instance, is the evidence to conclude that Washington was left in "shambles" by his second wife's death or that he went through a "drifting" period in the early nineties). Some of the author's sentences stand out like roadblocks. Good editors, God bless 'em, have gone the way of footnotes at the bottom of the page.

The balance sheet on Washington? For Tuskegee whites the institute proved an economic lifeline. For Alabama, which gave it \$3,000 annually, it trained teachers and crafts-

men who could return to their respective communities and develop their vested interests in segregation. For Northern whites Washington's program was not only an endorsement of their retreat from Reconstruction but also was persuasive in holding Negroes in their "sunny South," where Washington knew they would "contribute to its business and industrial prosperity." For the black stockholders in the Tuskegee corporation there was, first, a suspension of civil liberties and, later, a great abject dependence on the man his secretary called the "Wizard." For millions of black people, Washington's skilled acquiescence failed to stanch the wounds of disfranchisement and segregation, humiliation and lynching. Washington, what dividends did he receive? Harlan concludes that, at his best, his "vision to make Tuskegee a model community pointing the way to the black man's salvation in America" triumphed over his corruptive methods and "gave nobility to his efforts to end the slovenliness that was a heritage from slavery and poverty." At his worst, Washington was "paternalistic" and "dictatorial" after the style of the planters and industrialists whom he incessantly flattered and from whom, especially in the case of the latter, he received generous consideration.

Let Ralph Ellison have the last word. In *Invisible Man* the school principal, a composite of Washington and his successor Robert Moton, advises the nameless student: "you let the white folk worry about pride and dignity—you learn where you are and get yourself power, influence; contacts with powerful and influential people—then stay in the dark and use it."

WILLIAM CHEEK  
*Université de Provence*

JEAN H. BAKER. *The Politics of Continuity: Maryland Political Parties from 1858 to 1870.* (The Goucher College Series.) Baltimore: Johns Hopkins University Press. 1973. Pp. xv, 239. \$11.00.

Until quite recently the political historiography of the Civil War period has portrayed a picture of party turmoil, voter confusion, and general disruption following this breakdown of democracy in the mid-nineteenth century. Now, in a carefully structured study of the Border State (Maryland), Professor Baker of Goucher

College has attempted to show through *The Politics of Continuity: Maryland Political Parties from 1858 to 1870* that this was not the case. Despite four years of battle, inflationary living, warmly contested political issues, the disruption of the state's labor force, and the passage of the Thirteenth, Fourteenth, and Fifteenth Amendments to the Constitution, the political parties of Maryland retained their basic structure, leaders, and consistency throughout the period 1858-70.

Originally a doctoral dissertation at Johns Hopkins University the book is grounded upon a variety of sources and demonstrates exhaustive research. Relevant manuscript collections are supported by statistical sources, newspapers, local histories, and biographies.

Baker's theme is that the Democratic party of Maryland was reorganized in 1859 on three basic principles: fear of the free Negro, Know-Nothing voter interferences, and preservation of the "Constitution and Union." She states that the Democrats lost power during the Civil War not because of "Unionism," but because their leaders had left the state, and, she says, once the conflict had ended the Democratic party regained its "greatest strength in areas which had supported [it] before the war," by using, in 1866, the familiar tactic of voter interference by military forces and stressing the need to control the Negroes and preserve the "Constitution as it is and Union as it was." The same continuity is evident for the development from Know-Nothing to Union to Republican party in Maryland.

Although she argues with moderation and skill Baker is not always convincing. She never clearly resolves the ethnocultural and religious antagonisms that frequently influence voting behavior, the effects of the 1850 immigration on the 1860s political picture, and the temperance and public school issues that resulted from voter awareness. And finally, little attention is given to economic interests that might have shed additional light on the various party coalitions in Maryland between 1858 and 1870. However, by focusing on this Border State's political parties during the Civil War era Professor Baker has helped us to understand a complex political picture. Obviously more research will have to be done on Maryland's politics for this period, and I hope the author will

carry on. It is nicely written, it is useful, and it should be read.

HUGH G. EARNHART

Youngstown State University

JOHN S. HALLER, JR. *Outcasts from Evolution: Scientific Attitudes of Racial Inferiority, 1859-1900*. Urbana: University of Illinois Press. 1971. Pp. xv, 228. \$7.50.

Reading this book is painful, and yet one cannot easily put it down. It is a history of the prostitution of biology and anthropology on behalf of race bigotry. The saddest aspect is that the culprits were not laymen who borrowed some bogus science to confer an appearance of respectability on their prejudices. They were professional scientists, several of whom have also made some authentic and even outstanding contributions to human knowledge.

The author restricts his attention almost entirely to the American scene, and to the time period 1859-1900 (the years of the publication of Darwin's *On the Origin of Species* and of the rediscovery of Mendel's laws of heredity respectively). Of course, racist misrepresentations of science did not end in 1900; they are practiced now. Fortunately, in recent decades many scientists raised their voices in opposition to racist anthropology, psychology, and biology. Irresponsible attitudes of the sort "I describe things as I see them; what use other people make of my findings is none of my concern" are not extinct but they are no longer prevalent. By contrast, there was little scientific opposition to racism during the period described in the book under review and, in fact, before Hitler made racist doctrines justification of his atrocities.

Negro slavery, the Civil War, and its long aftermath made American racists solicitous for scientific "evidence" that the blacks are "outcasts from evolution," inferior human beings, if they are human at all. Any evidence was good if it could be interpreted to show what was desired. The author surveys all this intellectual rubbish with admirable patience and objectivity, and with at least ostensible detachment. So-called polygenists asserted that there are several living species of the genus homo and that the whites and the blacks belong to different species (this view was still defended in the 1940s in a book published by Harvard Univer-

sity Press!). Polygenism was compatible with either evolutionism or antievolutionism. The Bible allegedly relates the story of the "Adamite family," but there were other species "with whom the sacred writer had no concern." Or else, the blacks and the whites are descendants of different species of apes. Mulattoes are inter-specific hybrids who are semisterile or sterile. Monogenists granted that all men belong to the same species, but the divergence of races made them basically and irreversibly different. Considerable ingenuity was expended on showing that the blacks are intermediate between, or more closely related to apes than to white men. For this purpose some characters were emphasized and others, in which the distance between whites and apes appears to be less than between blacks and apes, conveniently overlooked or interpreted away. One of the trump cards was "the facial angle" (invented already in the eighteenth century), which is close to 90 per cent in whites and lower in blacks. Alas, the hairiness of the body is often greater in whites than in blacks.

Another temptingly convenient line of argument was that the process of hominization was somehow delayed in the colored compared to the less pigmented races. The blacks are, consequently, underdeveloped, evolutionarily backward, and incapable of being educated (this view was expounded again by an anthropologist in a book published in 1962, and was hotly debated for a few years). A backward race may be expected to succumb in competition with a better endowed one, and so, not illogically, a view was expressed in post-Civil War years that eventual extinction of the blacks in the United States is an "unerring certainty." The confidently expected extinction failed, however, to materialize, and this raised some thorny questions. The blacks were alleged to be endowed with excessive "sexual passion"; not only did they multiply unduly, but there appeared more and more hybrids. The hybridization was virtually unanimously deplored; some contended that the hybrids were inferior to both parental races, while others conceded that they may be a biological improvement of the blacks, but surely a debasement of the white race. During the last quarter of the nineteenth century an amazing scheme was advanced by LeConte and some others. After all, there are different subraces

within the white race, and some of them (the Teutons, later more often named Nordics) are superior beings, while others, such as the Irish and the Eastern Europeans, are "marginal varieties," although still superior to the blacks. So, one should encourage "judicious crossing" of the white marginal varieties with the blacks, tolerate crosses of Teutons with the marginals, but protect the Teutons from direct unions with Negroes. LeConte did not realize that his system would inevitably result in gene exchange eventually from the "lowest" to the "highest" levels.

The white "marginal varieties" caused increasing concern toward the close of the nineteenth century when numerous specimens of them began to arrive as immigrants. An interesting theory was advanced by the paleontologist and scientific generalist N. S. Shaler to account for the evident, to him, inferiority of the "marginals." Many of the latter happen to be Roman Catholics; for centuries the Church drained the lower classes of talented individuals, condemned them to celibacy, and thus caused deterioration of the race. Shaler must have had some different explanation for the Eastern "marginals," whose priests were married, and often raised large families. One finishes reading the book with a heavy heart, but with admiration for the thoroughness and objectivity of the author.

THEODOSIUS DOBZHANSKY  
University of California,  
Davis

GEORGE SINKLER. *The Racial Attitudes of American Presidents: From Abraham Lincoln to Theodore Roosevelt.* (Anchor Books.) Reprint; Garden City, N.Y.: Doubleday and Company. 1972. Pp. xvii, 500. \$2.50.

Professor Sinkler states that the main objective of his book "is to ascertain the racial views of American Presidents during a selected period of history." Other objectives are to examine the extent to which presidential rhetoric gives support to Gunnar Myrdal's view that the Caucasian fear of amalgamation is at the core of the black-white problem in the United States, and "to obtain additional insight into the dynamics of race adjustment, and to determine the extent to which ideas of race influenced the

general thinking and political behavior of these Presidents" (p. xv).

After a brief introductory chapter the book is organized around the individual presidents in chronological order. It is based on their manuscript papers, as well as their printed works, and is liberally sprinkled with quotations, including material from the incoming correspondence. The main emphasis is on black Americans, although within some of the chapters there are brief sections devoted to native Americans, Orientals, Jews, and general "international" aspects of race. These latter sections are too brief and fragmented to be of value. Most of the material discussed demonstrates that whatever the nature of presidential rhetoric at a particular moment, the presidents generally displayed "a marked lack of vigor and courage" (p. 303) in protecting the civil rights of black Americans.

The author has scoured the presidential papers for the presidents' statements on race and has pointed to the main problems of their administrations in racial matters, but for the most part he has not placed the presidents within the racial climate of thought in the late nineteenth century. Except for brief statements in the introductory chapter there is little discussion of the massive arguments on race that, consciously or not, shaped the beliefs, rhetoric, and actions of the individual presidents. The emphasis is on discovering what each president said about race (particularly in regard to blacks) and to a lesser extent on what they did, rather than on the root causes of their rhetoric and actions.

Those wishing to gather material on the racial attitudes of particular presidents will find much of interest in this book. It will be of less value for those interested in the origin of these racial attitudes.

REGINALD HORSMAN  
University of Wisconsin—  
Milwaukee

ROBERT H. JONES. *Disrupted Decades: The Civil War and Reconstruction Years*. New York: Charles Scribner's Sons. 1973. Pp. xiv, 543. \$15.00.

KEITH IAN POLAKOFF. *The Politics of Inertia: The Election of 1876 and the End of Reconstruction*. Baton Rouge: Louisiana State University Press. 1973. Pp. xiv, 343. \$10.95.

Jones's book is a concise general survey of the Civil War and Reconstruction period, while Polakoff's work is a specialized treatment of the politics of the 1870s culminating in the settlement of the presidential election of 1876.

The former is intended for the general reader and for use as a textbook for undergraduates and is certainly one of the best of a number of such books to be recently published. There are several errors that seem to be the fate of many first editions, but the book is well organized, incorporates the most recent interpretations, and is extremely readable. It is comprehensive and thorough in regard to the major issues and events of the disrupted decades. The narrative is enlivened with numerous little known details and anecdotal material.

The emphasis is on political and military history, with more than half of the pages being devoted to the war itself. The course of the war is clearly traced and there is more than usual coverage of the war at sea. One chapter is devoted to the diplomacy, especially Northern, involved in the conflict, while a chapter on the war in the trans-Mississippi West reflects the author's previous studies of Indians and the Northwest during the Civil War.

The impact of today's civil rights movement is seen not only in the recognition given the part played by blacks in the antebellum period, during the war and in the aftermath of the conflict, but also in the generous treatment of the Radical Republicans.

Sectional interests and differences in the years before the war are amply developed, including the racism that was found in the North as well as the South. The constitutional crisis preceding the outbreak of war is thoroughly described and there is a good analysis of the divisions to be found in the Northern states throughout the entire period.

*Disrupted Decades* is generalized but it is solid and never superficial. There is a thorough explanation of such topics as states' rights, secession, and impeachment. Jones has succeeded in handling complex material in a manner that not only brings together a summary of the best historical scholarship, but does it with a clarity that holds the reader's interest.

Polakoff's study is probably as useful for its description and analysis of the organization and techniques employed by the two major parties in the seventies as it is for its conclusions con-

cerning the compromises of 1877. Much research has gone into the description of the political situation in the various states—North as well as South. Party convention practices in the states and the political situations in the states and at the national level leading to the nominations of 1876 are detailed. The two nominating conventions and the events preceding them are described, including the differences between the parties and between the factions within each of them. The point is made that while the decentralization of the parties was an advantage in the nominating process it proved to be a handicap in winning elections.

In fact, Polakoff's thesis is that the factions within both parties and the lack of national unity and leadership made the results of the election of 1876 inevitable and were more of a factor than the compromises that have been given so much attention by Woodward and others. The Democratic party leadership was indecisive and failed to unite on a course of action. Hayes often would not commit himself, and he really did not have to, but his friends who spoke on his behalf added to the confusion. At the same time Tilden was both unable and unwilling to hold the Democrats together. Perhaps there was little he could have done, in any event, because the white South wanted home-rule most of all, and Northern public opinion was demanding an end to "tinkering with the South."

Some historians have sought economic explanations for the actions of Southern political leaders, while others have returned to the more traditional accounts of the disputed election. There is little doubt that there were complex political and economic maneuvers going on, but as Polakoff notes, the same results would have been obtained without them. Since the bargains were largely broken the "politics of inertia" may be as good an explanation as any for what happened. While Polakoff disclaims any attempt to refute Woodward he does fault the latter's use of the sources and disagrees with his interpretation on key issues.

The book, which concludes with a useful essay on the sources related to political leaders of the period, is a significant contribution to the political history of the Reconstruction era.

HARRIS L. DANTE  
Kent State University

THOMAS M. PITKIN. *The Captain Departs: Ulysses S. Grant's Last Campaign*. With a foreword by JOHN Y. SIMON. Carbondale: Southern Illinois University Press. 1973. Pp. xix, 164. \$6.95.

JOHN A. CARPENTER. *Ulysses S. Grant*. (Twayne's Rulers and Statesmen of the World Series, 14.) New York: Twayne Publishers. 1970. Pp. 217. \$4.95.

JOHN Y. SIMON, editor. *The Papers of Ulysses S. Grant*. Volume 3, *October 1, 1861–January 7, 1862*. Carbondale: Southern Illinois University Press. 1970. Pp. xxv, 479. \$15.00.

"I thought if we could get him to come to Mount McGregor, and if he should die there, it might make the place a national shrine—and incidently a success." The real estate promoter was talking about the cottage on the grounds of his New York State resort hotel in which Ulysses S. Grant died. Shoddy exploitation accompanied greatness right to the grave. Grant, the simplest of men (simple minded, Henry Adams said), was both the enshrined hero of a noble cause and the wayward antihero of sham values.

Somehow Grant invited the confusion. He had come to Mount McGregor to be as comfortable as possible and to finish writing, before he died of throat cancer, his *Personal Memoirs*. It was a gallant (and successful) attempt to overcome a Wall Street failure. Surely this once the old soldier would be allowed to be alone with his task. Not so; in fact, Grant did not much like being alone. He went out in the June sun and, surrounded by charmingly overdressed relatives, was photographed for the newspapers with a silk hat on his head and a scarf, the only sensible garment on display, at his throat. On another day, at his own request, he was jostled along a path in a Bath chair to take the view at an overlook. All this was good for business at the Hotel Balmoral. When he died and his discoloring body was put on display, it attracted a trainload of visitors from nearby Saratoga. Before the next day's curious arrived, embarrassed embalmers "applied a bleaching lotion to the general's face." In contrast to this ghoulish nonsense, he had worked hard on the good plain prose of his book until seven days before he died. Ulysses S. Grant was representative of that which is most counterfeit as well as that which is most genuine in America.

Thomas Pitkin's book about Grant's death, *The Captain Departs*, was originally a work focussed on the historical site at Mount Mc-

Gregor. In its expanded form it deals not only with the place where Grant died but with the accompanying circumstances. It is not a work of history of the first importance, but it avoids excessive piety and, curiously, comes closer to placing Grant in his time and place than either of the other fine pieces of scholarship under review.

In *Ulysses S. Grant* John Carpenter has written the best short modern account of Grant's career that we have. He has seen one man by resisting the inclination to emphasize either Grant's military or his political career to the neglect of the other. Carpenter's study is reliable and useful, but it does not live up to the promise of its first pages, where he introduces the fascinating problem of explaining how a war and a people's perception of that war made a president and a folkhero of a "thirty-eight [year old] . . . ex-army officer fit for nothing better than a clerkship in a leather store." Carpenter speaks often of that which is enigmatic in Grant and in his summary states that "Grant the enigma remains and it will always be that way, simply because Grant defies reasoned analysis." In one sense that is true of the subject of any biography, in another it is simply throwing up one's hands. Enigma is where one begins with a man like Grant. It is a false notion that a writer must share the values of his biographical subject to analyze and know the man or woman in question, but in this study there is a progressive sense of disengagement from Grant. An acknowledged Christian commitment held Carpenter to his subject in *Sword and Olive Branch*, his biography of O. O. Howard, but there is no such tie to Grant.

There is precision and detail in the book but little description of the rich effect of the man on his times and of them on him. Events are ticked off and Grant appropriately credited or excused. Carpenter correctly spots the importance of the true if unlikely friendship of Grant and Hamilton Fish but errs in dismissing Amos Akerman as a worthless nonentity. The former Confederate who, as attorney general, thought that railroads should be controlled and that civil rights laws should be enforced, was an important example of men who did not think it inevitable that Reconstruction be abandoned. Grant dismissed Akerman and thereby lost the chance to be excused as simply a follower in retrogressive

racial politics. Grant chose the route he took. Carpenter does a better job with Orville Babcock. Grant's proposal that he go to St. Louis and testify to keep his attractive but crooked secretary out of jail has a nice note of loyalty of one soldier for another. It also raised the question, which Carpenter does not resolve, whether the president could possibly have been ignorant of swindles that reached so deep into his official and personal families.

If Carpenter could have read, in sequence, all documents and letters signed by Grant, would he have found the general still an enigma? One guesses the answer might be yes. Such is not the hidden premise of the third book under review, volume 3 of *The Papers of Ulysses S. Grant*. A Rankian trust in detail and a puritan faith in the word have led in recent years to an outpouring of superbly edited and annotated publications of the papers of great men of our history. There is a will to believe that with the man's own words in print the truth will out.

John Y. Simon's edition of the Grant Papers is one of the finest of these projects, but the volumes to date do not provide sufficient evidence with which to predict that when the mammoth job is done Grant, unaided, will walk off the pages no longer a mystery. Indeed the excellence of the notes in the completed volumes predict a problem. After 1861 the Civil War and Grant's involvement in it increased in complexity, and the editor may find himself with a task almost equal to annotating the official records of the rebellion. With completeness the goal, it is hard for an editor to consign anything to an appendix or microfilm, but it is disconcerting to find the better part of a page of a handsomely printed, bound, and illustrated book given up to a letter that reads, in its entirety: "I find a deficiency in muster & pay Rolls Send a Quantity at once."

With another short letter, editor Simon turns author and skillfully juxtaposes Grant with a rival. Grant's terse three-line letter saying "no" is contrasted with a three-page footnote in which John McClelland's request for a "yes" becomes more and more convoluted. The editor is setting Grant up for the besting of other greater rivals. McClelland was the first of Grant's enemies, all of whom were on his own side, of course. There is nothing but gentlemanliness in his

correspondence with Confederate general Leonidas Polk.

From October 1861 to January 1862, the dates of volume 3, Grant was a brigadier general and not a clerk, but he still is closer to the Mexican War quartermaster than we expect he will be when we encounter the conqueror of Vicksburg or the manslayer of Cold Harbor. We learn much about how an army was supplied; there was exasperation with people who would profit from the war in the river town of Cairo, in which the war in the West was rather quietly beginning. The only battle is Belmont, important more because Grant took the initiative and attacked than for any strategic results.

In 1861 Grant was already an able military politician. Testifying before a congressional committee, on which Elihu Washburne chanced to sit, he did not whine and fulminate, or blame civilian politicians for a supply of foreign guns into which American ammunition would not fit. But he did make the gun purchase look silly—and himself smart. With few such exceptions, however, there is little here that gets us below the surface. Another letter that does is Grant's testy rebuke of his outrageous father. Jesse Root Grant even then, and long afterwards, knew how to make his son feel like an incompetent boy. It rankled.

The letters convey the same qualities of blandness and authority that characterize the *Personal Memoirs*, but even in that great book the man is hard to see. One senses that when the people of Saratoga bought the elegant two-volume set they talked more of the money it earned for the widow than of the man it masked. And yet, as these three newer books testify, that man, Grant, still intrigues us as he did Americans of his own time, who found the stuff of fantasies in his military prowess and companionship in their own messy struggles with success and failure.

WILLIAM S. MCFEELY  
Mount Holyoke College

RUSSELL F. WEIGLEY. *The American Way of War: A History of United States Military Strategy and Policy*. (The Wars of the United States.) New York: Macmillan Publishing Company. 1973. Pp. xxiii, 584. \$12.95.

According to one prominent German soldier in

the late nineteenth century, "he who writes on strategy and tactics should force himself to teach an exclusive national strategy and tactics," otherwise his own army will never benefit from his work. Half a century later Captain Liddell Hart, the preeminent exponent of mechanization and of a strategy of indirect approach, wrote *The British Way in Warfare* to show "that there has been a distinctively British practice of war, based on experience and proved by three centuries"—and also to demonstrate that this historic British practice was in accord with his own theories.

Is there a comparable or even a discernible American way in warfare? Before World War I most soldiers probably would have answered that there was not: "von Moltke teaches us our strategy, Griepenkerl writes our orders, while von der Goltz tells us how they should be executed," and until recently most American works on strategy closely resembled a tossed salad, with bits and pieces from every fashionable European theorist. Now Professor Weigley asserts that there is indeed a characteristic American approach to war, although admittedly the evolution of American strategical doctrine before the 1950s is less evident in the realm of theory than in the application of strategic thought in war. According to Professor Weigley the "American way" surfaced initially in Grant's 1864 campaign, which aimed at the destruction of Lee's army instead of the capture of Richmond. This "strategy of annihilation" guided our military expeditions in the subsequent Indian wars, it was applied by Admiral Mahan to naval warfare ("In war," Mahan had written, "the proper objective of the Navy is the enemy's navy"), and during World War I it so thoroughly dominated the generalship on the western front that Billy Mitchell could snort: "The art of war had departed. Attrition, or the gradual killing off of the enemy, was all the ground armies were capable of."

By this time the concept of a strategy of annihilation had come to dominate our military thinking as a nation. The campaign in the Pacific during World War II impresses Professor Weigley as "a Mahanian triumph of sea power . . . rendered immensely more formidable through its acquisition of aerial and amphibious dimensions," while the mighty endeavor in Europe was planned in the best tradition of



U. S. Grant—a strategy of direct confrontation to destroy the German armies, with the combined bomber offensive in effect tilting the battlefield to a vertical position. This strategy of annihilation and its corollary, Unconditional Surrender (even the initials suggest “made in America”), failed us in the Korean conflict, and any alternative “proved so frustratingly at variance with the American conception of war that it upset the balance of judgment of American officers in the field and threatened the psychological balance of the nation itself.” The atomic revolution produced a rich outpouring of strategic literature in the 1950s, leading first to a strategy of deterrence followed by one of limited war and flexible response. As the Vietnam war clearly demonstrated, the latter must also go back to the drawing boards, and Professor Weigley ends with some provocative observations on the future of the American—or any other—way in war. Elsewhere in the book he quotes General George C. Marshall to the effect that “a democracy cannot fight a Seven Years War.”

This is an important book. Professor Weigley writes with authority, he has done impressive research in the old and mostly forgotten military journals, and he seems equally at home in the highly complex and technical treatises produced by the modern think tanks. Moreover, his unrivaled knowledge of the history of the American army and its institutions enables him to view the evolution of American strategy and policy in a clear, indeed an artistic perspective. One need not agree with all of his generalizations, his arbitrary categories (Grantian, Uptonian, Jominian, Mahanian and the like), or even with his mute assumption that the American way of war is unique, to appreciate his solid achievement.

I, for one, am impressed not so much with the American way in war as with the fact that from our beginnings as a nation, American strategic thought has often closely paralleled developments in Europe. Washington waged a strategy of attrition for the same reason that Frederick the Great assumed the strategic defensive after 1758: circumstances offered no choice. Winfield Scott may have been an old-fashioned, eighteenth-century general, but his strategy—like his manual of tactics—reflected the latest French theories. It could be argued

that Halleck and Dennis H. Mahan, by placing disproportionate emphasis upon the strategic role of fortifications, were the first true representatives of an American way in war: if many of their ideas came from Jomini, they applied them to problems that were distinctly American. It is irrelevant to suggest that they should have viewed strategy as a “Clausewitzian struggle for the overthrow of enemy armies.” Nor does Grant seem much different in approach from the Prussian general in 1866 who, searching his memory in vain for an instance or a doctrine that would suggest an appropriate solution to his particular situation, finally sputtered: “Let history and principles go to the devil! After all, what is the problem?” In his memorandum of 1868 von Moltke, who elsewhere defined strategy as “common sense applied to the art of war,” stated the doctrine with “Grantian” simplicity: “The operations plan for the offensive . . . consists solely in seeking out the main enemy force and attacking it wherever it is found.” Foch would have agreed. Mahan belongs in the mainstream of British naval historians, and our own official principles—like our national anthem—came from the British, who lifted them from the writings of J. F. C. Fuller, who in turn had deduced them from the *Correspondance* of Napoleon. Even the American way in war, it appears, owes something to the Corsican.

JAY LUYAAS  
*Allegheny College*

JAMES HASKINS. *Pinckney Benton Stewart Pinchback*. New York: Macmillan Publishing Company. 1973. Pp. xiv, 292. \$8.95.

The difficulties of undertaking a full-scale biography of Pinckney Benton Stewart Pinchback, a Negro leader prominent in Louisiana during Reconstruction, are indeed overwhelming. Only a few personal papers were available. Thus Pinchback's early life as the acknowledged son of a white planter and mulatto slave, his brief education in Cincinnati, his career as cabin boy and protégé of riverboat gamblers, plus his brief military service, are delineated with a literary license more characteristic of fiction than of historical biography.

Pinchback's rise in Louisiana politics during

Reconstruction began with his election to the state constitutional convention, where he proposed a strong civil rights article. Elected state senator, subsequently serving as Senate president, and finally as acting governor, he failed to be confirmed in his dual effort to be seated either as a United States senator or as a member of the House. In 1877, after a three-year attempt to secure a seat in the Senate, he retired to Washington, D. C., where he lived until his death in 1921.

In Haskins's analysis Pinchback emerges as a self-serving politician, who at the same time was solicitous of newly won Negro rights. Unfortunately Haskins, a professor of education, conveys little awareness of historical scholarship that has laid to rest many of the stereotypes that previously dominated interpretations of this era. For example, Louis Harlan's careful account of the successful integration of New Orleans public schools during Reconstruction does not comport with Haskins's comments about public education there. Neither do the perceptions about Negro life in New Orleans, gleaned from William S. McFeely's study of General Howard and the Freedmen's Bureau, accord with Haskins's generalizations.

While Professor Haskins presents an interesting, lively, and popular account of Pinchback's life, a scholarly evaluation of his role in Reconstruction history has yet to be written.

SUZANNE C. LOWITT  
*Transylvania University*

PAUL T. RINGENBACH. *Tramps and Reformers, 1873-1916: The Discovery of Unemployment in New York*. (Contributions in American History, number 27.) Westport, Conn.: Greenwood Press. 1973. Pp. xv, 224. \$10.50.

"There are tramps because so many people give without investigation and without cooperation," wrote sociologist Charles R. Henderson in 1906. But in the depression years of the 1870s and 1890s there were tramps because so many people had no jobs. *Tramps and Reformers* chronicles the gradual process by which leaders in New York City's Charity Organization Society and Association for the Improvement of the Condition of the Poor accepted the idea that involuntary unemployment actually existed in bountiful America. Because tramps

were the most visible poor people and hence the worst examples of intemperance, immorality, improvidence, and deficiency, Ringenbach inevitably interchanges discussions about the causes of tramps with those on the causes of poverty. The homeless unemployed of New York City remain historically faceless, although we find that they were mainly single men, native born, white, residents of the city, and likely to have some physical defect. The COS and AICP reformers appear as the familiar gaggle of paternalistic pseudo-scientists more concerned about the possibility of wasting kindness and money than about the reality of wasted lives. Thus they rail against missions, bread lines, clothing drives, handouts, the Salvation Army, and any other form of "indiscriminate aid."

Ringenbach credits the panic of 1893 with demonstrating the inadequacy of charity and voluntarism and with forcing reformers both to acknowledge that unemployment resulted chiefly from economic dislocation and to accept the need for public relief. The first claim is beyond dispute, but the author himself shows that most reformers still blamed the tramp's plight on personal deficiencies, while the significant studies and measures designed to mitigate the effects of unemployment followed the recessions of 1907 and 1913. He contends ably that fear of social disruption was the crucial motivation throughout the period 1873-1916: at first New York City's charitable leaders sought to prevent upheaval by repression; later they and their successors worked to forestall revolution by liberal reform.

Ringenbach's conclusions should startle no one and will not challenge existing interpretations about social thought and action in the period he covers. Rather, he has done a generally solid piece of work that takes us through the shifting opinions and efforts of one type of reformer. Others have covered much of this ground, but Ringenbach has filled in a number of details and has arranged them in a single study of New York City's attempts to deal with the problem of industrial unemployment.

J. PAUL MITCHELL  
*Ball State University*

WILLIAM WARREN ROGERS and ROBERT DAVID WARD. *August Reckoning: Jack Turner and*

*Racism in Post-Civil War Alabama.* Baton Rouge: Louisiana State University Press. 1973. Pp. xii, 195. \$7.50.

Born into slavery, Jack Turner spent his early life in Choctaw County, Alabama, and there he remained after the Civil War to settle into a life of farming. He also became active in the Republican party. Following the overthrow of the Reconstruction regime in 1874 Choctaw became one of the few Black Belt counties in which the Republicans consistently made a strong showing. That resulted from the fact that Turner and a few other black men assumed leadership of the local party and by their example and hard work encouraged blacks to continue voting. White Democrats consistently harassed Turner in the hopes of discouraging his political activities. They did not succeed until 1882, when Turner became the victim of a conspiracy in which it was alleged that he had plotted to incite blacks to massacre whites. Within a span of four days, he was arrested, declared guilty at a mass meeting, and hanged.

William Warren Rogers and Robert David Ward have produced a valuable study that offers a microcosmic view of political and racial developments in the post-Reconstruction South. Their findings support the views developed by C. Vann Woodward: the Redeemer era did not represent a period of good race relations, but neither was it a time in which blacks were totally disfranchised. Not only did many blacks continue to vote, but men such as Jack Turner refused to accept white domination and worked courageously to make Choctaw County a more livable place for blacks.

The authors maintain that Turner left a lasting legacy and that his example encouraged blacks to continue voting. Unfortunately they do not support this assertion. Had they offered a summary of election returns for Choctaw County from 1882 until the adoption of the disfranchisement constitution in 1901, they would have strengthened their argument. Turner's lynching suggests that throughout the Redeemer era whites worked steadily to suppress blacks. Instead of accepting the view that the disfranchisement constitutions signified the sudden shift to a harsher period of racism it appears that there was a consistent movement in that direction throughout the post-

Reconstruction era. This suggestion, however, should not detract from the fact that *August Reckoning* illustrates how valuable local history can be when it is well done and used to deepen knowledge of broader developments.

WILLIAM F. HOLMES  
University of Georgia

GEOFFREY J. MARTIN. *Ellsworth Huntington: His Life and Thought.* [Hamden, Conn.:] Archon Books. 1973. Pp. xx, 315. \$20.00.

"A rational understanding of history," geographer Ellsworth Huntington once observed, "requires a good knowledge of the changing physical background upon which the historical events occur." To interrelate that background with other forces in history Huntington (1876-1947) devoted his career, communicating his ideas in no fewer than 28 books, parts of 29 others, more than 240 articles, 700 public addresses, and 50,000 pieces of correspondence, making him by any measure "the most prolific United States geographer" to date. In this biography—the first complete and balanced treatment of the man—geographer Geoffrey Martin has done a masterful job of collating and synthesizing his subject's extensive papers for a sympathetic view of an extraordinarily energetic mind (though he has no bibliography of other secondary sources).

Attacked (with some truth) by critics as rash, unscientific, and deterministic, Huntington sought to "comprehend the course of civilization throughout history" by studying the influence of the environment—heredity, culture, and especially climate—upon individuals and thus entire civilizations. He was philosophically close to such historian-generalists as Turner, Durant, Barnes, and Toynbee; the latter's knowing foreword to this book acknowledges Huntington as a pioneer in interdisciplinary analysis, but one who "suffered for being ahead of his contemporaries." His precomputer quantitative research, one example of his far-sightedness, led him into tree-ring counts and solar energy analyses for a tentative theory of climatic pulsations, a subject only now being given adequate consideration in many disciplines. (See, for example, Colin Renfrew, *Before Civilization: The Radiocarbon Revolution and Prehistoric Europe* [1973]).

Huntington's intellectual verve in positing his one-sided theses and leaving the "con" sides to the critics in order to establish a meaningful dialogue often confounded and embarrassed his peers, especially his employers at Yale and the struggling profession of geography at large. And his forays into elitist-oriented eugenics became political when he promoted restrictive American immigration policies during the 1920s and echoed (no doubt unconsciously) the Nazi selective breeding of the 1930s. Such associations, including the one linking astrology with cosmic periodicities, the author deliberately minimizes, which is unfortunate. Nevertheless, to Huntington, the question was the thing, and Martin recognizes such questions as Huntington raised as too sweeping and premature for the mainstream of American geography as it began to overspecialize.

This book in fact often reads as an intellectual history of that profession during Huntington's lifetime, revealing its conservative attitudes, the trials of teaching versus research, problems of funding for expeditions (for Huntington traveled widely, especially in Central Asia), and indeed the attempts to get historians to acknowledge the importance of geography. (See his "Changes of Climate and History," *AHR*, 17 [1913] 2: 213-32.) Like the historians, the geographers served their country well in World War I, in which the Coal Administration had Captain Huntington produce a tract urging people to conserve fuel by keeping house temperatures under sixty-five degrees Fahrenheit (!), a temperature he considered ideal for physical activity—which led him to admire the Germans, disparage the tropics, and regard Newport, R.I., as the ideal locale.

Huntington synthesized geography, environment, and history only incompletely in his last work, *Mainsprings of Civilization* (1945), although the outline for its never-completed sequel "The Pace of History," reproduced in the biography, offers opportunities for further inquiry. But was he in fact an environmental determinist? Not in the sense of the Ratzel-Semple school, says author Martin. Rather, in his attempt to build "a scheme revealing the place of man twixt the cosmos and microcosmos," Huntington was developing "a new form of determinism"—never named—"more

moderate, supple, subtle, and versatile than anything" before or since. This book is required reading for all students of universal history.

CLARK G. REYNOLDS  
University of Maine,  
Orono

*The Joyous Journey of LeRoy R. and Ann W. Hafen: An Autobiography.* Glendale, Calif.: Arthur H. Clark Company; Denver: Fred A. Rosenstock: The Old West Publishing Company. 1973. Pp. 334. \$11.50.

*Joyous Journey* is the life story of LeRoy R. Hafen, Western historian. Much of the content is devoted to those adult years with his wife, Ann Woodbury Hafen, chronicling their joint ventures as a research-literary team. Their shared experiences confirm the book's theme: "Two are better than one . . . for if either fall, the one will lift the other up." Hafen recounts his years of youth in a remote Mormon community in Nevada, his early local education and higher education at Brigham Young University, and graduate study at the University of California, Berkeley, under Professor Herbert E. Bolton, eventually leading to the Ph.D. degree. Hafen's lifetime professional career centered on the Colorado Historical Society where he served as state historian. He details experiences in expanding the Society's research and museum resources, his work as editor of *Colorado Magazine*, part-time teaching at the University of Colorado and Denver University, and his activities in various professional organizations.

In many respects Hafen's life story is the common autobiography of hundreds of professional historians active in the period 1920-50. Even the devoted husband-wife team performance, touching and romantic as it is, is not unique for his generation of scholars. But two aspects of this work imbue *Joyous Journey* with a dimension of uniqueness that sets this life story apart. Hafen was reared at Bunkerville, nestled near the Virgin River on the Nevada frontier, and his life provides intimate glimpses into the Mormon life-style during the latter part of the nineteenth century—the toil, hardship and commitment to personal and group success. Hafen details the link of this remote

saints' settlement with the Utah Mormon establishment. He reports his family origins, largely Swiss immigrant converts to Mormonism, depicting his father as a stern patriarch and practicing polygamist. Interestingly, his father's polygamy occurred after the Edmunds Act of 1882. *Joyous Journey* openly comments on the role of the mother in this sort of family system, and frankly reports the attitudes of plural wives, one to the other. Beyond this, the book's value is in casting light on the personal and historiographic genesis of a large body of literature on the American West. Hafen's publication list contains thirty-eight works, and *Joyous Journey* yields intimate glimpses and insights into the generation of this rich list.

It would seem that, approached in this light, *Joyous Journey* has value for the historian, and thereby its faults become minor. Only one flaw in construction need receive comment. The text is laced with quotations from journals and letters that lack connective tissue and interrupt and divert the reader. This and other faults are offset by the author's disarming candor, his open sharing, frankness in language, and direct reporting that enrich *Joyous Journey* with a special earthy essence.

ARRELL M. GIBSON  
University of Oklahoma

EDWARD P. CRAPOL. *America for Americans: Economic Nationalism and Anglophobia in the Late Nineteenth Century*. (Contributions in American History, number 28.) Westport, Conn.: Greenwood Press, 1973. Pp. vii, 248. \$13.00.

Although it may not be so self-evident, as Mr. Crapol seems to think, that Anglophobia was more widespread and more consciously exploited by politicians in the United States during the last quarter of the nineteenth century than hitherto, the author nonetheless makes a case for suggesting that it was more markedly expressed in terms of economic nationalism. This change he relates very effectively to the depression of the 1870s. That experience highlighted the financial and commercial predominance of Great Britain at the same time that the export boom of 1879-81 emphasized the opportunities of economic expansion. And even as the balance of trade moved decisively in favor of the United States, resentment against

the British was sustained by the persistent imbalance of payments, by Britain's maintenance of the gold standard, and by the evident role of British investors in the exploitation of Western lands. So, after the end of the Civil War, American economic nationalism widened from the narrow issue of the domestic tariff to the questions of international transportation, reciprocity treaties, and the remonetization of silver. These were questions that were forced gradually upon the attentions of all major parties whether their purpose was to preserve or to reform the essence of American society. And, whatever the considerable differences of opinion over them, these were also issues that forced all administrations into some sort of confrontation with Great Britain. All this seems clear enough in Mr. Crapol's rather labored account. The author seems to think that this sustains the case for economic determinism in the history of the Anglo-American rapprochement. It would appear, however, that sentimental nationalists used economic chauvinism to serve their purposes as much as economic interests used Anglophobia. Nor is the dilemma any clearer on the British side. This Mr. Crapol touches rather too superficially and arbitrarily by assertions about the priority and precedence of American over German economic rivalry in the British government's calculations. By a foray into the Chamberlain papers he underlines what has long been known, that Salisbury was isolated and defeated on the Venezuelan question by the rest of the Cabinet. But so was Palmerston on the Crompton and Central American questions nearly half a century earlier, before the economic considerations Mr. Crapol describes had become immediate or urgent. The essential difference in the significance of the two episodes would seem to be the intrusion of the Civil War; the essential similarity political as much as economic.

KENNETH BOURNE  
London School of Economics

VICTOR WESTPHALL. *Thomas Benton Catron and His Era*. Tucson: University of Arizona Press, 1973. Pp. x, 462. Cloth \$12.50, paper \$6.95.

Thomas Benton Catron was the leader of the Republican party in New Mexico during the last four decades of the territorial period. As

a determined empire builder he managed to accumulate almost as much land as the states of Rhode Island and Delaware combined. His prominence continued into the statehood period, Catron being elected one of New Mexico's two United States senators in 1912.

Westphall has written an informative and carefully documented biography. From Catron's early years in Missouri to his tenure as the only rabidly Republican ex-Confederate to serve in the United States Senate, Westphall has conscientiously chronicled the events of Catron's life. The exhaustive examination of Catron's business affairs is particularly valuable. Catron had worked diligently to acquire partial or complete control of at least thirty-four Spanish or Mexican land grants. Although perpetually in need of cash he had a "psychotic aversion" to losing any part of this vast domain. But poor business judgment and mounting debts compelled him to sell most of his holdings after 1890. At the time of his death in 1921 only seven land grants remained in his possession.

Readers may have trouble recognizing Westphall's Catron. Because of his controversial involvement in some of New Mexico's most celebrated disputes Catron has appeared as an arch villain to many students of the state's history. Westphall, however, stoutly defends him against practically all charges, insisting that most emanate from an unfair "oral tradition" that has grown up around him. He denies Catron's complicity in the American Valley murders and refutes the charges leveled against him in the disbarment proceedings that stemmed from the explosive Borrego affair. He insists that Catron's involvement in the Lincoln County War was only to protect his property, despite a preponderance of evidence elsewhere that political meddling was almost a passion.

In handling this admittedly acquisitive individual as a man of his times Westphall has given a better-balanced picture of Catron than existed before. But Westphall's Catron standing alone will not suffice as an accurate image of the controversial New Mexican.

ROBERT W. LARSON

*University of Northern Colorado*

HOWARD P. CHUDACOFF. *Mobile Americans: Residential and Social Mobility in Omaha, 1880-*

*1920.* (The Urban Life in America Series.) New York: Oxford University Press, 1972. Pp. x, 195. \$8.95.

Although one of the most neatly hewn among the "new urban history" productions, this volume has problems with its format. The data are significant and meticulously presented, but it does not quite come out a "book," in the classic sense. To my mind, too, *Mobile Americans*, like others of the genre, is with respect to form too organized around a theoretical straw man, which it all too neatly destroys. In a sense what we have here is a fine piece of empirical research, an important building block, which is dressed out sometimes as local history, sometimes as a statement in a tired academic controversy about the costs and benefits of urban mobility.

*Mobile Americans* is a longitudinal quantitative examination of residential movement in Omaha, Nebraska, in two stages of the city's growth, the thrust of much of which is to prove that "the neighborhoods which men sought and the kinds of housing they acquired expressed real improvement in the quality of life for themselves and their families" (pp. 109-10). To this end Howard Chudacoff explores possible differentials in mobility by area, ethnicity, and occupation, and he relates these patterns to outmigration, occupational mobility, and political preference. "Findings" aplenty—in the sense of important-looking relationships—grace Chudacoff's pages (the tables number twenty-eight).

Although the author rightly emphasizes the generality of mobility, ought it distract him from exploring the differentials he finds? Should it lead him to overlook the relationships among age, residential mobility, and property accumulation, which he can treat with his first but not with his second sample? Could we not, further, hope for more of a sense of what widespread mobility meant to Omaha's economic and physical growth, rather than simply to the "openness" of life there? Chudacoff's chapters on the housing market and on voting are suggestive, but not sufficiently tied to the morphology of Omaha, though the areal sampling design would permit this.

*Mobile Americans* will be greatly valued by specialists, and no doubt widely "cannibalized" for its findings. One hopes its critics will take

proper note of series editor Richard Wade's grandiose claims for the book and give serious thought to appropriate packaging for such vital yet ungainly research contributions.

JOHN MODELL  
University of Minnesota,  
Twin Cities

KENNETH S. DAVIS. *FDR: The Beckoning of Destiny, 1882-1928: A History*. New York: G. P. Putnam's Sons. 1972. Pp. 936. \$15.00.

The author has done a masterful job in creating a psycho-biographical study not only of the snobbish patrician Franklin Delano Roosevelt, but of each of the strong individuals who left a significant imprint upon this evolving political activist. The reader is stimulated anew by imaginative portraits of vital personalities: FDR's imperious mother; his cousin and exemplar, Theodore Roosevelt; a deeply devoted, unattractive kingmaker, Louis Howe; a Southern Populist-Progressive, almost saintlike in his patience and understanding, Josephus Daniels; a Woodrow Wilson whose great vision was stained with an intolerant heart; and an inferiority-plagued Eleanor who evolved as a vibrant late-bloomer.

Kenneth Davis's unique command of the language insures fascinating reading, as does his keen perception of major developments abroad and profound socioeconomic changes at home. All this insures a challenging and suitable substitute for an average textbook that encompasses United States history between Reconstruction and the eve of the great Depression.

The author is deeply indebted to the pioneering work of Frank Freidel, along with Alfred Rollins and others who have spent considerable time with the massive archives at Hyde Park's Roosevelt Library. But the spotlighting, as well as the meaning of individuals, movements, and events, is the contribution of Davis alone. He makes clear that young Roosevelt's life at an upper-class Harvard is light years away from its present egalitarian counterparts, that the fusion of Wilson's New Freedom and TR's New Nationalism had tremendous significance for our times, and he highlights the failure of Roosevelt, and other world leaders, to understand and cope with the im-

pact of technology upon the world's population.

Far less knowledgeable of the social welfare struggle than such Tammany stalwarts as Robert F. Wagner, Sr., Al Smith, and Big Tim Sullivan, aristocratic FDR was an "awfully mean cuss" when he first entered state politics. As an efficient, assistant secretary of the navy, he was an outspoken supporter of imperialist Mahan and his big navy colleagues. Nor was he disturbed by the massacre of thousands of Haitians, or by the kidnapping of additional thousands by the U. S. Marines to be used as slave labor to build roads inexpensively. And the recent events in Vietnam were recalled with all of their heart-rending tragedies upon reading that these same U. S. Marines habitually called Haitians "gooks," regarded them as sub-human, and often spoke of killing them as a sport. Has history taught us nothing?

It was not his courageous struggle with infantile paralysis that insured a "spiritual transformation" in the 1920s, but FDR's ability to perceive and respond to changes, sooner than most others, which enabled him to shelve his earlier imperialist and more conservative tendencies and devote himself to a somewhat enlightened, and potentially more progressive apprenticeship for the presidency, through the governorship of New York.

BERNARD BELLUSH  
City College,  
City University of New York

BRUCE L. LARSON. *Lindbergh of Minnesota: A Political Biography*. Foreword by CHARLES A. LINDBERGH, JR. New York: Harcourt Brace Jovanovich. 1973. Pp. xix, 363. \$14.50.

This is the story of an honorable man's concern for his America. Charles A. Lindbergh of Minnesota inherited a liberal turn of mind from his father, a liberal Swedish politician who migrated to the United States when the son was but a year old. He grew to manhood in Minnesota, and after his graduation from the law school of the University of Michigan in 1883 he practiced law for a year in St. Cloud, Minnesota, before settling permanently in the nearby community of Little Falls.

In 1887 he married the daughter of the family with whom he boarded when he first came to

Little Falls, and he entered into local politics with election as county attorney in 1890. His first wife died in 1898, and in 1901 he married Evangeline Lodge Land, who became the mother of Charles A. Lindbergh, Jr., the pioneer in aviation.

Politically, Lindbergh was Republican in those years, and in 1906 he became the Republican congressman from the Sixth Congressional District of Minnesota, a position he held until the end of the Sixty-fourth Congress in March 1917. He soon demonstrated qualities of independence by joining the Progressive insurgent group fighting against the power of the speaker of the house, Joseph G. Cannon. He accepted the liberalism of Theodore Roosevelt, and he rejected the conservatism of William Howard Taft. He became a vocal critic of moneyed interests and big business, and he thought the Federal Reserve System created in 1913 would lead the nation into disaster. He was ardently antiwar, and he was one of the minority that opposed American involvement in World War I.

He wrote diligently for publication. He was neither a polished writer nor speaker, but he impressed most people with his earnestness and with the impressive documentation with which he bolstered his arguments. When he could find no established publication willing to accept his work he became his own publisher. In 1918 he wrote a little book, entitled *Why Is Your Country at War?*, intended to clarify his views on economics, politics, and the war. It became an indictment of American business and the inner circle of business leaders. In Minnesota it was interpreted as an act of disloyalty to a nation at war and contributed to his defeat as the gubernatorial candidate of a newly organized independent political group, the Nonpartisan League.

The account of his campaign for the governorship of Minnesota in that election year is a tale of interference with his appearances at political gatherings, of weathering storms of rotten eggs and spoiled tomatoes, and of having the homes of his supporters painted yellow by ardent opponents. After the election was past, the Nonpartisan League, primarily an organization of farmers, joined a labor group to form a new Farmer-Labor party. Lindbergh was active in its evolution, and at the time of his death in

early 1924 he was the leading candidate for endorsement as its candidate for governor.

Lindbergh was something of an anachronism in American politics. As the author of this volume explains it, he "could be nothing but what he was." He was ruggedly an individualist at a time when party regularity was called for, and time after time he favored changes in current practices without regard for their effect upon his own political life. He was utterly incapable of subterfuge, and that, in a way, explains both his successes and his failures.

This is a thoughtful little volume. Sometimes it seems pedantic and some of the stylistic mannerisms of the author may distract, but the message he seeks to convey comes out clear and strong. It is a valuable addition to the literature of the Progressive age in American politics.

ARTHUR J. LARSEN  
*University of Minnesota,  
Duluth*

JACOB J. WEINSTEIN. *Solomon Goldman: A Rabbi's Rabbi*. New York: Ktav Publishing House. 1973. Pp. xiii, 295. \$10.00.

Weinstein writes of Solomon Goldman (1893-1953) as "a rabbi's rabbi" not because colleagues came to the older man of renown for counsel, but because Goldman crowded so many types of rabbinical excellence into one career as to be a paragon of the modern rabbi, a man to be emulated: preacher of enormous eloquence, Judaic scholar deeply absorbed in the sources, builder in Chicago of the largest conservative congregation in the land, leader of American and world Zionist forces at a fateful time in Jewish history, patron of Hebrew and Yiddish poets and authors, creator of a pioneering Jewish day school. Goldman was one of the dozen great rabbis of the swirling, desperate times before and after World War II. Weinstein has written an admiring life sketch of his older friend, studded with quotations from letters and sermons, and rounded off with extensive excerpts from works long out of print. Weinstein has not attempted to evaluate Goldman's life in relation to the American rabbinate as a whole, nor to contrast Goldman with such rabbinic giants as Stephen S. Wise, Abba Hillel Silver, James G. Heller, Joshua Loth Liebman, Simon Greenberg, Milton Steinberg, and the incom-



parable Mordecai M. Kaplan. Although this biography includes some of the sense of stress and change and danger under which Goldman lived, too little of the background development is offered to permit a reader unacquainted with American Jewish personalities and institutions to comprehend Goldman's true importance. In a way, then, Weinstein has written *A Rabbi's Rabbi* primarily for rabbis and others personally involved in American Jewish professional and intellectual activity. But his well-written book will be extremely useful when the time comes to evaluate the achievements of that period (now ended, for good or ill) when America's rabbis were the leaders of and spokesmen for its Jews.

BERTRAM W. KORN

Hebrew Union College—  
Jewish Institute of Religion,  
New York

DAVID M. TUCKER. *Lieutenant Lee of Beale Street*. Nashville: Vanderbilt University Press. 1971. Pp. xi, 217. \$7.95.

Born black and in poverty in 1894, George Washington Lee should have lived his years unknown except to his immediate neighbors in Sunflower County, Mississippi, in the heart of the Delta. It was a time of negligible opportunity for blacks and of racist extremism—a time when unparalleled numbers of lynchings despoiled the lush landscape and when political demagogues manipulated white anxieties and hatreds by condemning black voting and “nigger education” for having inflamed black bestiality and fulminated rapes and other assaults against white people. Yet Lee, the subject of this well-written biography, escaped the Delta for relative wealth, power, and position in Memphis.

As described by David M. Tucker, Lee was not only a person of both impressive accomplishments and humiliating defeats, but also a person who, for racial as well as personal reasons, had a deep need of recognized success and an equally deep fear of failure. An alumnus of Alcorn College (the black OCS at Fort Des Moines, Iowa) and the French battlefields of World War I, Lieutenant Lee settled permanently in Memphis after the war. In the 1920s and 1930s he became a community leader as a black capitalist and an enterprising life

insurance executive, a Republican politician who could work in harmony with the Democratic boss of Memphis, E. H. Crump, and as a writer of a novel, short stories, and the acclaimed *Beale Street: Where the Blues Began*. By the late 1940s, however, Lee's public stature had declined. Even with the evident black restlessness with “the snail-like pace of change” in Memphis, Lee continued to believe that “blacks could make no better contribution toward desegregation than to continue the development of their own business, their own talents, and thereby earn community respect and fellowship.” Further diminishing his authority were the successes of civil rights activists, the emergence of Southern black Democracy and the Goldwaterite conquest of Southern Republicanism, and, finally, “black power.” Still alive today, Lieutenant Lee's voice is an echo from the past rather than a signpost to the future.

Although minor defects (for example, no bibliography and the discordant use of the archaic “colored”) mar this good book, it performs the valuable function of suggesting fruitful areas of inquiry and, in some cases, of re-examination for historians of black America; among these are the relationship between racism and reform in twentieth-century Southern politics, the social origins of the South's black bourgeoisie, and the social bases of black ideologies.

WILLIAM M. TUTTLE, JR.  
University of Kansas

RICHARD B. SHERMAN. *The Republican Party and Black America: From McKinley to Hoover, 1896-1933*. Charlottesville: University Press of Virginia. 1973. Pp. viii, 274. \$9.50.

Richard Sherman centers attention upon the Republican presidents, their racial attitudes and policies, and the manner in which they responded to the repeated attempts of black leaders to have the party live up to its abolitionist origins. In every presidential election between 1860 and 1932 the ballots of those black Americans able to vote substantiated Frederick Douglass's contention that “The Republican Party is the deck, all else is the sea.” The author expertly assesses this odd political love affair.

Observers of the Republican party during the

1960s and 1970s have frequently noted its inclination to arrive at a suitable "Southern strategy." Sherman's study makes clear the extent to which the party has struggled all along with the problem of trying to gain, maintain or regain majority status by breaking the Democratic solid South. Regrettably Republican leadership has tended to protect the prejudices of majority whites rather than the rights of minority blacks. As a consequence blacks, although long distrustful of the schizophrenic Democrats, eventually shifted their political allegiance.

The author nowhere does a better job of illustrating the Republican record than in his treatment of the party's ambivalent stand on lynching. Protection of the lives of its citizens is the most fundamental responsibility of government, but nearly all Democrats and significant numbers of Republicans shrank from applying the amount of federal force needed to control the streets and fields where the lynchings took place. Arguments in favor of state rights and local initiative won out over appeals by NAACP leaders and other blacks to end this national disgrace.

Sherman's treatment also deals effectively with the actions of some of the second-level white officers of government who have so often held positions of importance and who have frustrated black attempts to bring the power structure to account.

The book has some minor weaknesses such as the author's frequent tendency to use the word Negro instead of the now generally preferred black and his occasional failure to make clear that American racism has been a white and not a black problem (e.g., p. 53). His supporting footnotes are generally good, but his bibliographical comment is much too brief.

In addition two larger limitations impinge on the book's usefulness. First, the author omits much of the human interest detail that serves to heighten emotional response and draw a larger reading public. Second, the book includes little by way of behavioral data. Studies of political parties particularly lend themselves to powerful blends of traditional and quantitative materials, but the author has overlooked this possibility.

However, these reservations should not obscure the conclusion that Richard Sherman has

supplied us with a scholarly, solid, balanced, valuable book on an important subject not previously studied. It is a pity that such a worthwhile contribution will be read by a mere handful of professional historians and political scientists.

SIG SYNNESTVEDT

State University of New York,  
College at Brockport

GERALD D. NASH. *The American West in the Twentieth Century: A Short History of an Urban Crisis*. Englewood Cliffs, N.J.: Prentice-Hall. 1973. Pp. viii, 312. Cloth \$9.95, paper \$4.95.

Although primarily a work of synthesis intended for the general reader, Professor Nash has demonstrated that in the hands of a skilled craftsman the history of a region may also reveal a great deal about the entire American experience. Focusing upon the trans-Mississippi West (more precisely the subregions of California, the Pacific Northwest, the Rocky Mountain and Plains states, and the Southwest), he provides an urban framework for interpreting and understanding the forces that made the West what it is today. Thus he contends that between the Dry West of Walter Prescott Webb and the Wet West of the Pacific slope there came into being in the twentieth century a new oasis of urban civilization born of technological control over the environment. Using the urban oases as bench marks Nash traces the maturation of the region as a whole, arguing that it passed through two distinct stages: between 1898 and 1941 the relationship of the West to the older East was like a colony to the mother country, whereas since World War II the region has set the pace for the nation in economics, politics, social mores, and culture (of both the serious and popular variety). The thesis is not new, but the sophistication with which the author accounts for the differential rates of maturation within the subregions will interest the specialist as well as the novice.

In elaborating upon his central theme Nash steps outside the Turnerian framework. He does not ignore the mediating effect of environment, but rightly asserts that cultural influences (with a big assist from the federal government) may well have been more significant in shaping the modern West. For, he notes, the migrants

of the post-1890 decades had brought to the region the major traits of the civilization east of the Mississippi: the dominant Anglo-Saxon conceptions of social status and ethnic minorities, the political institutions of the East, a desire for wealth, an uncritical faith in science and progress, and a strong drive to re-create the cultural life of the communities they had left behind. By mid-century, if the West had come to mirror the attainment of the affluent society in America, Nash concludes that it also had become a prime example of some of its most elemental flaws: the anomie of individuals, the failure to acculturate racial minorities, and the inability to prevent environmental imbalances brought by wanton and unplanned applications of science and technology.

To treat simultaneously so many facets of so divergent a region as the trans-Mississippi West and the urban oases therein is a herculean task, but one the author accomplishes successfully. His writing combines the perspective of the insider—having lived the greater part of his professional life in the region he knows both the terrain and the people—with the critical intelligence of a mature scholar. Thus there are shrewd observations about the pattern of economic growth and the crucial roles of private initiative and federal monies in freeing the West from the yoke of colonialism. But surprisingly sharp, too, are Nash's discussions of the people—whether transplanted Midwesterners, Chicanos, blacks, or American Indians—and the concept of regionalism as manifested in literature and the arts. While the organization of the book is largely synthetic, the author succeeds in telling his story because he never lets the material get away from him. Recurrent themes and appropriate pauses summarize and pull together the myriad of facts. It is a book well-worth reading.

PHILIP J. FUNIGIELLO  
College of William and Mary

JOHN BRAEMAN *et al.*, editors. *Twentieth-Century American Foreign Policy*. (Modern America, number 3.) [Columbus:] Ohio State University Press. 1971. Pp. ix, 567. \$10.00.

WILLIAM APPLEMAN WILLIAMS, editor. *From Colony to Empire: Essays in the History of American Foreign Relations*. New York: John Wiley and Sons. 1972. Pp. viii, 506. \$10.95.

Together these volumes under review comprise a series of essays by eighteen historians of American foreign relations. *Twentieth-Century American Foreign Policy* is by design a collection of largely disconnected essays that share only their common relationship to the country's external relations. In *From Colony to Empire* editor William Appleman Williams has covered interpretatively the chronological development of the United States from its ambitious beginnings to its present-day global leadership. Most of his contributors are identified with the New Left, yet Richard W. Van Alstyne, who contributes two essays, is not. And the essays, as a group, reflect a fundamental realism as much as they do the latter-day revisionism. Thus, even these writings should be examined for their individual contributions rather than as a unified presentation of the American experience. This is not to suggest that the volume lacks coherence.

So varied in nature and content are the long essays in the Braeman, Bremner, and Brody volume that they appear to lack both theme and scheme. This matters little, for the collection includes much wisdom on both the historians and the history of the century. Charles E. Neu's opening essay on "The Changing Interpretive Structure of American Foreign Policy" is a prodigious, successful effort to identify and characterize the immense volume of American writing on United States foreign relations since the beginning of the century. David F. Trask has rendered a similar contribution, focusing only on the writings since 1957. Together these essays catalog the conceptual and interpretive achievements of a large and distinct body of American historians. Waldo H. Heinrichs has added an exceptionally erudite history of the rise of American career diplomacy. Essays by Paul A. Varg, Manfred Jonas, and Lawrence S. Kaplan analyze three central phases of American foreign relations since 1900: the nature of American world power, 1900-17; the failure of collective security before 1939; and the contributions of NATO. Robert Craig Brown, Lyle C. Brown, James W. Wilkie, Allan R. Millett, A. E. Campbell, and Warren I. Cohen trace twentieth-century United States relations with Canada, Mexico, Cuba, Great Britain, and China respectively. These essays vary in style and levels of analysis, but they all reflect an

essential expertise. Warren Cohen, for example, establishes an unusually high level of historical analysis in his study of the United States and China, an analysis similar in concept to that of Manfred Jonas on the failure of collective security. The brightest and the best personalities, as these studies reveal, have served the country badly on more than one occasion.

With its simple chronological organization, *From Colony to Empire* posed choices of authorship rather than subject matter. That the United States would attain some commanding position in world affairs seemed decreed from the beginning. What the essayists of this volume question are the methods, motivations, and costs of the successive stages in the country's expansion. Walter LaFeber accents the largely intellectual contributions of Franklin and Madison to the country's imperial character in the eighteenth century. Richard Van Alstyne's analysis of American continental expansion, 1803-67, is, as usual, highly perceptive and critical more of the means than the ends of American expansionist policy. Edward P. Crapol and Howard Schonberger present a balanced account of the economic factors in the rise of American global expansionism after the Civil War, focusing especially on agricultural and industrial surpluses, internal improvements, the quest for an effective navy, merchant marine, and reciprocal trade agreements to reduce the competition for foreign markets. In their concern for economic pressures Crapol and Schonberger generally ignored the countering arguments that raised questions of ends and means and stressed the ultimate price of the nation's expansionist policies. On the other hand Robert Freeman Smith's study of the 1920s is almost identical in analysis to those of Cohen and Jonas. Henry Berger's study of the 1930s concentrates more narrowly on the American labor movement and foreign policy. Finally, Lloyd Gardner's thoughtful essays on American foreign relations, from 1945 to 1970, raise a full spectrum of questions based far more on realism than on economic determinism.

These two volumes differ, then, in organization, character, perhaps even in purpose, but they have many positive qualities in common. Both contain essays of high quality, some remarkably insightful, which can add immeasurably to what historians and others might come

to understand about the American past. They demonstrate, moreover, a perceptive blending of viewpoints that challenges the notion that diplomatic historians who claim membership in identifiable schools of interpretation always write history that is rendered distinguishable by previous ideological or nonideological commitments. Is it possible that diplomatic historians might one day reach a consensus on some central aspect of the American experience?

NORMAN A. GRAEBNER  
*University of Virginia*

THOMAS C. COCHRAN. *Social Change in America: The Twentieth Century*. New York: Harper and Row. 1972. Pp. 178. Cloth \$12.50, paper \$2.45.

Thomas Cochran has long been an advocate of "useful history," and he has often pointed out the advantages and limitations of applying social science procedures to the study of history. A decade ago he observed that "history, if it is to hold its high place in the field of learning, must suggest policies for meeting current problems." The dilemmas facing the historical profession today add urgency to Cochran's contention in this volume that history must prove it has some analytical utility. "It must move beyond the traditional and undefined justification of 'increased understanding' toward forming a basis for, at least, limited forecasting of the range of social probabilities" (p. 12).

*Social Change in America* is based on a course of lectures sponsored by St. Antony's College, Oxford University in 1970. It is a companion volume to an earlier collection of essays, *The Inner Revolution* (1964, 1970). Beginning with the assertion that the basic purpose of useful history is explanation of social change Cochran invites some historians to turn from the discovery of new facts in order to seek the larger meaning or utility of the vast amounts of data available in dissertations, monographs, and articles. Specifically he argues for acceptance by historians of a behavioral science approach to social change. In the first chapter he explains the use of role theory as a method of organizing historical data. In subsequent chapters he applies the behavioral approach to intellectual, religious, educational, technological, demographic, and other aspects of change in

twentieth-century American history. The basic information contained in these brief interpretive chapters is familiar, but the analysis is often stimulating and arresting. "Demographic Forces" and "Proprietary and Managerial Enterprise" are particularly rewarding. Most of the chapters emphasize the importance in social change (as well as social conformity) of the "roles, role-sets, and accepted institutions" of business elites.

Thorstein Veblen's name appears only once in the text but many passages have a Veblen-esque ring: "The actual owners and proprietors of enterprise have not been an organized or homogeneous group, and although they have given the United States a business-like culture, it has not been through the deliberate use of power, politically or socially, but rather through emulation of their roles in the adoption by others of their everyday habits and customs" (p. 97). Not derivatively, but in orientation, interdisciplinary expertise, playful humor, and underlying seriousness of purpose, Cochran's book continues and renews the approach Veblen, Beard, Dewey, and Commons introduced into American scholarship in the early years of the twentieth century.

ROBERT H. BREMNER  
Ohio State University

DAVID H. BURTON. *Theodore Roosevelt*. (Twayne's Rulers and Statesmen of the World Series, 17.) New York: Twayne Publishers, 1972. Pp. 236. \$5.95.

DAVID H. BURTON. *Theodore Roosevelt and His English Correspondents: A Special Relationship of Friends*. (Transactions of the American Philosophical Society, New Series, volume 63, part 2.) Philadelphia: the Society, 1973. Pp. 70. \$3.00.

A biography of Theodore Roosevelt suggests a simplification of the reviewer's customary descriptive task, for in a basic sense the contents are already well known. And in David Burton's *Theodore Roosevelt* the traditional chronological frame contains the expected topics: youth, the frontier experience, politics, the Spanish War, the presidency, 1912, and final years. In his analysis Burton generally follows such post-Pringle revisionists as William Harbaugh and John Blum in viewing TR not as a failed progressive but as a successful conservative re-

former—his record of accomplishments, both foreign and domestic, is deemed worthy; his skill as a political professional is extolled; his belief in character and morality is stressed. Although Mr. Burton acknowledges Gabriel Kolko's recent challenge to this positive conceptual framework, in the main he rejects it. As a volume in Twayne's Rulers and Statesmen of the World Series, a series which aims to reach the general reader with concise biographies based on recent scholarship, the book is entirely satisfactory.

One is less certain of both the publisher's and author's more ambitious hopes. The book jacket speaks of "placing Roosevelt in the American intellectual tradition . . . of his times," while Mr. Burton in the preface says that his "own approach . . . is to discover in Roosevelt's mind, in his thought, and in his values, not so much the 'true' TR, but truths about him which either have been ignored or, more commonly, misunderstood." In order to place Roosevelt, it would seem necessary to establish in some detail the components of the American intellectual tradition, yet neither the text itself, specific footnotes, nor the bibliographical essay utilize such seemingly relevant works as, for example, *Age of Reform*, *Rendezvous with Destiny*, *Social Thought in America: The Revolt Against Formalism*, or *The End of American Innocence*. The one brief reference to *The Course of American Democratic Thought* and *The American Mind* was not to establish the essentials of our intellectual tradition, but rather to indicate how TR, frequently, has not been taken seriously as a political thinker. As to the "truths about him," promised by Mr. Burton, it is hard to see where he has really given us anything new or cleared up old misunderstandings. Certainly it is no surprise to learn of the personal, as well as social, premium that TR placed on "the fighting edge," to learn that he believed in self-interest as the only true basis for foreign policy, or that he subscribed to the early twentieth-century belief in the positive relationship between imperialism and progress.

Mr. Burton expands on TR's foreign policy views in *Theodore Roosevelt and His English Correspondents: A Special Relationship of Friends*. He uses letters between TR and five of his most consistent English correspondents

(Cecil Spring Rice, Arthur Hamilton Lee, James Bryce, St. Loe Strachey, and George Otto Trevelyan) to point up principles held in common, principles that these writers believed provided a sound basis for friendship between their two countries. Some of the letters are published here for the first time and Mr. Burton artfully weaves together the letters and his own commentary. Once again one is impressed with familiar notes: that the friendship was cemented by "the moral excellence which Roosevelt and his friends took to be the unique possession of their race"; that a common heritage, common language, and shared institutions played their part; and, most important, that the elimination of past conflicts of interest made it increasingly possible to think in terms of a mutual, rather than solely national, self-interest.

In short, Mr. Burton's two books appear more useful for the general reader rather than the professional historian.

ROBERT A. HUFF

*Hobart and William Smith Colleges*

OTIS L. GRAHAM, JR. *The Great Campaigns: Reform and War in America, 1900-1928*. (Prentice-Hall History of the American People Series.) Englewood Cliffs, N.J.: Prentice-Hall, 1971. Pp. xiii, 386. Cloth \$8.95, paper \$3.95.

Here Otis Graham puts together his data and many ideas on progressivism from early to late, the field he explored from a special angle of vision in *From Roosevelt to Roosevelt*. Doubtless by the predetermination of editor and publisher, the book introduces with flourish the new six-volume and six-author series for students, the History of the American People. Probably setting a format for the other volumes, this one is divided into two main sections, the text and a collection of illustrative documents, and two short ones, a bibliographic essay and a section of illustrations. This last falls far short of the editor's announcement in the foreword of a photographic essay. But the text and the bibliography are substantial, lively, and neorevisionist, as promised.

The title word, "campaigns," voices the multiplicity and continuity that carry the burden of the book. It begins with the "thousand campaigns" of Progressivism before 1916, moves on to participation in World War I, and,

finally, covers reform and reaction during the 1920s. Graham imposes manageability on all this by dividing text and documents into corresponding, parallel sections: "Reform: 1900-1916," "War: 1914-1918," and "Reform: 1917-1928." His main contribution to professional readers is his effective sorting out of the campaigns—separating stated purposes from real ones, democratic and liberating efforts from organizational efforts, social-justice reforms from self-serving arrangements.

The author portrays no heroes. The nearest is Woodrow Wilson, from whose papers—while making no more than one selection from any other Progressive—Graham chooses seven of his twenty-eight illustrative documents. But his admiration is limited. While saying that Progressivism in general had no affinity for war and that America's participation in World War I was unavoidable, he says also that Wilson's getting the country to act in 1917 was done for "the wrong reasons at the wrong time." Not much honor for the war president, here. In the same area he opposes his opinions to those of Leuchtenberg, Bernstein, and others, but I doubt that he has pondered equally the antimilitarist Progressives, including Jane Addams whom he admires.

Altogether Graham honors Hofstadter, but avoids status interpretation; he acknowledges debt to Kolko and other radical historians, but thinks them extreme about Progressivism. He identifies himself with the community versus society dichotomy of Hays and Wiebe. Extending that line of thought he points out many shortfalls in Progressivism. Agreeing about this, I find the whole suggestion of the book uncommonly pessimistic. As one follows Graham back to the first quarter of the century, one can hardly dismiss remembrance that only yesterday more demanding campaigns than ever were being fought for social equality and that American bombers were exceeding all previous campaigns of destruction in behalf of national purposes. When will histories of the American people start to examine fully events of stress and accommodation between those two types of great campaigns—which are here expertly delineated, but without much recognition that the two are always in tension?

CHARLES A. BARKER

*Johns Hopkins University*

CARL W. CONDIT. *Chicago, 1910-29: Building, Planning, and Urban Technology*. Chicago: University of Chicago Press. 1973. Pp. xiv, 354. \$12.50.

In this pathbreaking "technical biography" of Chicago, Carl Condit has moved beyond his previous work on the structural and esthetic qualities of building to include discussions of the development of the street and transportation network of the city, its parks and boulevards, the impact of the Burnham Plan, and the construction of Chicago's cultural institutions and waterways (the latter doing double duty as sanitary conduits and transportation arteries). He has combined esthetic, technological, and transportation elements to provide a solid history of the physical development of a major city. The result comes closer than any other book in fulfilling Roy Lubove's suggestion that urban historians study the city building process, the values, economic functions, and technology that have made a city a particular kind of artifact.

From the acceptance of the Burnham Plan in 1910 to the crash of 1929, Chicagoans built office complexes, theaters, museums, hotels and apartments, and railroad terminals on a grand scale. Condit finds many of those structures superior to anything of recent years. Architecture may have been eclectic and derivative in contrast to the major innovations of the Chicago school, which flourished from the 1880s through the early years of the century, and the later Prairie school; but the best buildings were admirably suited to their functions and represented great structural achievements. A case in point is the Tribune Building, whose winning design evoked Louis Sullivan's bitter denunciation.

For the period before 1929 the author reverses John Kenneth Galbraith's argument of the 1950s about the prevalence of private affluence and public squalor in the United States. Large individual firms, commercial developers, railroads, philanthropists, and the government all built magnificently; while the immediate physical environment for poorer Chicagoans was cramped and mean. Condit deplores the contrast but does not ask to what extent the concentration of power and wealth that made possible the great structures he ad-

mired was also responsible for the squalor he condemns.

He closes, with 1929 because depression, war, postwar militarism, and the economic decline of the city later weakened Chicago's civic vitality. He has some deservedly harsh comments about the impact of recent neglect, indifference, and inflation upon the city's parks and museums.

The book has fifty-nine illustrations, seven useful statistical appendixes, and a superior index. A disappointing feature is that information essential to following his argument is sometimes relegated to the notes, which are grouped at the end of each chapter. This placement is particularly unfortunate in that none are references—the reader has only chapter by chapter sections in the bibliography as a guide to sources—but all are explanatory or provide additional data and therefore belong at the bottom of the page. Ideally some of this material should be in the text to enhance the reader's understanding. Condit's story is too important and complex to bury treasures in relatively inaccessible places.

JAMES F. RICHARDSON  
*University of Akron*

RICHARD K. SMITH. *First Across! The U.S. Navy's Transatlantic Flight of 1919*. Annapolis: Naval Institute Press. 1973. Pp. 279. \$10.00.

Richard K. Smith chronicles an important but barely remembered airplane flight, the 1919 crossing of the Atlantic, in stages, by the U.S. Navy's NC-4. His book's focus is wide enough to comprehend the minutest details of the preparation and execution of the flight. The flight log, transcripts of telephone conversations, drawings, and photographs are among the items reproduced in testimony to Smith's scouring of the archives. The reader, after threshing the detail, is likely to retain this general impression of the flight: it was a team effort that drew upon engineering excellence (civilian and naval), upon the "can-do" spirit of navy aviators, and upon the generous, indeed lavish, sustenance of the Navy Department. If the reader is a specialist he will also remember the practical lessons taught by the flight: the requirement, in oceanic aviation, for

accurate weather forecasting and reliable radio communication and direction-finding.

Nineteen nineteen was the year of several transatlantic flights, and of more attempts. Perhaps most of these were prompted by the London *Daily Mail's* offer of a prize of £10,000 to the first man across. But the U.S. Navy team, headed by Commander John H. Towers, was impelled to be first for the sake of being first—and for the consequent publicity that could be expected to advance their goal of building a naval air force. This expectation was not soon fulfilled. Still, success was better than failure. At least the navy was spared condemnation for having assigned almost one hundred ships to support the hop-scotching NC-4 (and its sister seaplanes, NC-1 and NC-3, which did not complete the crossing).

The flight of NC-4 (commanded by Lt. Commander Albert C. Read) was workman-like and not productive of heroism or drama. Something similar may be said of this book. But unfortunately the author, unlike his aviators, does strive for drama and profundity, conspicuously so in discussing the Paris Peace Conference (pp. 107–08) and American civilization (pp. 207–08). The results are embarrassing.

WILTON B. FOWLER  
Naval War College

INGA FLOTO. *Colonel House in Paris: A Study of American Policy at the Paris Peace Conference, 1919*. Aarhus: Universitetsforlaget. 1973. Pp. 374. 75 D. kr.

This volume, by a young Danish scholar, is difficult to review. On the one hand it strikes a judicious tone and contains a thoughtful historiographical essay on the Wilson-House relationship; on the other it suffers from an at times poor translation, is overly burdened with scholarly apparatus, follows an eccentric footnote style, and utilizes italics for authors' names as well as book titles, while omitting the italics for titles in the footnotes and bibliographical material. In short it has all the strengths and many of the weaknesses of Germanic-style scholarship.

Regardless of psychological insights and possible motives the author concludes that a break between Wilson and House was virtually inevitable. She depicts House as already standing

in opposition to Wilson's role and policy at the time he left for Europe and the pre-Armistice negotiations in October 1918. Frustrated because his influence with the president was already waning the colonel also felt himself to hold positions more to the left than Wilson. Moreover, House's role as the chief American negotiator not only gave him a vision of how to run a conference properly, but also a "taste for being the man to make the decisions" (p. 239). Finally, he became heavily influenced by Georges Clemenceau, the French premier, and developed a pro-French stance.

Yet the pre-Armistice negotiations, the author concludes, clearly revealed House's obvious weaknesses as a negotiator. He simply could no longer be certain of Wilson's wishes and thoughts, in contrast to his past role via the president. Moreover, House felt unsure of his role at the Paris Peace Conference. He became so absorbed in consolidating his own position within the American delegation that he failed to re-establish or maintain his old function with the president. During Wilson's absence from the peace conference, House forgot that his main duty was to act as spokesman for American policy as he essayed the role of manipulator of the conference. The author concludes that the colonel indeed acted clearly contrary to Wilson's intentions (acting "illoyally," according to the translation) on such important matters as the League Covenant and a preliminary peace treaty and French designs upon the Rhineland. Consequently, the break evidently occurred as soon as the president returned to France, at the very first talks between the two men. House thereafter was left without real influence upon American policy.

In summary, this study is likely to remain the definitive, to use the well-worn adjective, account of the House-Wilson rupture at Paris, and it throws new light on the American position at that conference.

DANIEL M. SMITH  
University of Colorado,  
Boulder

HEINRICH AUGUST WINKLER, editor. *Die grosse Krise in Amerika: Vergleichende Studien zur politischen Sozialgeschichte, 1929–1939*. (Kritische Studien zur Geschichtswissenschaft, 6.)



Göttingen: Vandenhoeck & Ruprecht, 1973. Pp. 243. DM 34.

Historians in the United States need hardly be reminded of the fruitfulness of comparative approaches to the study of American history. Yet agreement on theory often outruns actual practice. Hence this book of essays about the United States during the Great Depression of the 1930s should receive an unusually warm welcome since it provides interesting perspectives on understanding the American experience in a broader worldwide historical context. Its appearance is timely since scholars in the United States like John Garraty have recently begun to examine the American crisis of the 1930s in relation to the varied faces of European totalitarianism.

A central question that concerns these German historians is why similar economic crises in Germany and the United States resulted in very different political responses. In an excellent essay Ellis W. Hawley, the only American in this volume, suggests that this was so because the New Deal represented only a special phase of the broader development of an "organizational society," an outstanding trend in twentieth-century American life. His German collaborators discuss particular aspects of this interpretation whose theoretical postulates were laid out in Germany by Rudolf Hilferding. In separate papers Jürgen Kocka and Peter Lösche seek to explain why American labor in the New Deal turned neither to fascism nor to communism. They find a possible explanation in the New Deal's integration of labor into the organizational society, a society that exhibited some of the characteristics of syndicalism. Hans-Jürgen Puhle comes to similar conclusions after his examination of farm protest movements, while Hellmut Wollman, after exploring New Deal housing policies, concurs as he finds Franklin D. Roosevelt's social vision very limited, indeed. Roosevelt's caution was partly due to judicial obstructionism, Willi Paul Adams argues in his discussion of the United States Supreme Court in the 1930s. The concluding essay by Heinrich August Winkler attempts to crystallize the conclusions of the various contributors. The organizational society in the United States was molded by forces such as social mobility, the frontier experience, the

absence of a feudal tradition, the political dominance of the middle classes, and a tradition of democratic political theory. These influences prevented the rise of totalitarianism in the United States during the 1930s, in contrast to Germany. Despite superficial similarities between fascism and nazism as contrasted with the New Deal, Winkler argues that these movements were poles apart. He is particularly critical of the provincialism of New Left and Radical Right historians during the last two decades who have overlooked fundamental differences between German, Italian, and American political systems and who ignored, distorted, or oversimplified deeply rooted cultural and national values.

A major contribution of this work is to broaden the historical understanding of American as well as German historians about the United States during the Great Depression. It would behoove specialists in modern American history to consult this excellent book, primarily to ponder its many insights. To the German scholars who participated in this venture their American colleagues can only say: "*Famos. Macht so weiter!*"

GERALD D. NASH  
University of New Mexico

JAMES A. CLARK and MICHEL T. HALBOUTY. *The Last Boom*. New York: Random House, 1972. Pp. x, 305. \$8.95.

The ingredients of drama could hardly have been better mixed: a nation racked with depression, a piney woods section of east Texas, historically poor and undereducated, a lonely, seventy-two-year-old puritanical wildcatter who accepted no logic that doubted his faith that oil lay beneath the red soil, congregations that prayed each Sunday for his success as a means to their own temporal salvation, daily crowds of five to eighteen thousand watching the well being sunk because they had nothing better to do, and also because the success of the well held the only likelihood that their lives would ever be better, and then, finally, one of the world's great testaments of faith, a gusher that blew beyond all belief.

Columbus Marion Joiner, the discoverer of the greatest domestic oil pool in American history, watched the Daisy Bradford 3 blow and

then asked Dry Hole (formally known as D. Harold) Byrd to gauge the flow. "Whisper it to me," he said. When Byrd read his gauges, he whispered in Joiner's ear, "She's flowing at the rate of sixty-eight hundred barrels a day!" With disbelief Joiner shouted, "SIXTY-EIGHT HUNDRED BARRELS!" and the last of the great booms was on. Appropriately Daisy Bradford 3 came through on a Sunday, October 5, 1930. Although five thousand people had passed up church that day because they wanted to be at the well, many thousands more wished that they had been less religiously devoted so that they could have been present at this creation that is still spoken of in wonder throughout the petroleum industry and throughout the oil-minded Southwest.

The timing of this retelling is particularly significant, what with the United States apparently being sucked dry of its chief underground energy resource, edicts and fiat and pleas issuing almost daily on the topic, and the Arab world sitting on top of an oil heap, content in the knowledge that it holds the missing link in the world's diplomatic chain. The new Alaskan field will probably eclipse the importance of the east Texas field, but it will occur at least a thousand miles from where huge masses of people congregate. And it will be a controlled development, administered by corporation executives, government investigators, and ecological adherents. Other unsuspected stratigraphic traps may be uncovered in the midst of settlement, and they may be mined by wildcatters, but the nation has rules now that will prevent the excesses of 1930. When the authors of *The Last Boom* maintain that the scene around Kilgore cannot be repeated, they are on solid ground, even though that ground, like the minds of the people who live on it, is basted with great gobs of petroleum.

The significance of the east Texas field lies in more than the spectacular treasure hunt that ensued, the rise of swashbucklers like H. L. Hunt and others, and even the amount of oil made available to the United States for nearly half a century. The deeper meaning of east Texas lies in the fact that men like W. S. Farish, president of Humble and later of Standard of New Jersey, and J. Edgar Pew of Sun watched the field develop—"explode" might be a nearly more precise word—and whereas

they had matured with the thought that the words "government regulations" meant industrial blasphemy, they now began to consider government help. As the east Texas world became more riotous, martial law followed to stop the flow of so-called hot oil from the field, while the demand for conservation increased. In Washington, Secretary of the Interior Harold Ickes tried to bring regulation under his control, but he was beaten off by federal agents who felt that the Texas Railroad Commission could handle its own problem. The result was that the Railroad Commission became probably the strongest state regulatory board in the United States, and its stand on proration became the national standard. Ignoring the fact that the Railroad Commission has come under justifiable suspicion for setting oil allowable levels that keep the price of petroleum high—the fact remains that it has also shown other states the road to extracting the greatest percentage of crude oil from reservoirs. Before east Texas, oil men paid scant heed to conservation, and permitted, if not encouraged, "too-rapid withdrawal by men and companies bent on sudden fortunes." Whereas before east Texas a recovery of 40 per cent of the crude was considered adequate, now the percentage runs regularly to 80 and some times even 90 per cent. Again, when the east Texas field began to produce almost as many millions of barrels of salt water from its ancient sea as it was producing barrels of oil, the state of Texas, the operators (from corporate giants to small independents), and the technicians combined to put together a salt water disposal program that has become an industry model.

And finally, when the German armies were swinging toward Moscow and Joe Stalin was calling for a second front that could not be delivered because of, among other reasons, the inability to get petroleum to where the troops were, Ickes buried a hatchet with J. R. Parten, one of the most civilized Texans of this century, and persuaded him to be director of transportation for the Petroleum Administration for War. The result was the Big Inch and Little Inch pipelines, twenty-four inches and twenty inches in diameter, that began to move crude to Illinois on February 13, 1943, and on the following July 19—eleven months and sixteen days after the first joint was laid—all the way to the

East Coast. Without the Big Inch and Little Inch, most experts agree, the Allied invasion of Europe would not have been possible. Along the way Parton fought everyone: the War Production Board that refused to allocate steel, producers of alternative fuel in the North, and his own senator, W. Lee O'Daniel, who feared that Texas would be dried up for the benefit of the East. A good field should deliver 100 million barrels of oil during its lifetime—the two pipelines supplied more than 316 million barrels during the war period.

Critics will charge that the book is propetroleum. The charge is indisputable. But the fact remains that two men, one a professional oil writer and the other one of the most distinguished geologists in the business, have produced a book that only men steeped in the profession could have written. Their work represents a real contribution.

JOE B. FRANTZ  
University of Texas,  
Austin

DONALD GARNEL. *The Rise of Teamster Power in the West*. (Sponsored by the Institute of Industrial Relations, University of California, Berkeley.) Berkeley and Los Angeles: University of California Press. 1972. Pp. xi, 363. \$12.50.

FARRELL DOBBS. *Teamster Power*. New York: Monad Press; distrib. by Pathfinder Press, New York. 1973. Pp. 255. Cloth \$8.95, paper \$2.95.

The largest union in the United States is the International Brotherhood of Teamsters. But prior to the 1930s the Teamsters was a relatively small union with membership limited to local drivers who delivered milk or ice in the local community. Today, however, it is close to being a general workers' union with jurisdiction over "all teamsters, chauffeurs, warehousemen and helpers; all who are employed on or around horses, harness, carriages, automobiles, trucks, trailers and all other vehicles hauling, carrying, or conveying freight, merchandise, or materials; automobile sales, service and maintenance employees, garage workers and service station employees, warehousemen and all kinds employed in warehouse work, stockmen, shipping room employees, and loaders, that is persons engaged in loading or unloading freight, merchandise,

or other materials on to or from any type of vehicle; all classes of dairy employees, inside and outside, including salesmen; brewery and soft drink workers, workers employed in ice cream plants, and all other workers employed in the manufacture, processing, sale and distribution of food, milk, dairy and other products; all truck terminal employees; cannery workers; and other workers where the security of the bargaining positions of the above classifications requires the organization of such other workers." It is no wonder that someone has said that the Teamsters feel justified in organizing anyone since "if it moves, it is a truck; if it has four walls, it is a warehouse."

How this basic change in the structure of the Teamsters took place is the story related in two books: Donald Garnel's *The Rise of Teamster Power in the West* and Farrell Dobbs's *Teamster Power*. Garnel, a San Jose State University professor, describes Dave Beck's rise to influence along with the Teamsters he represented on the West Coast, and Dobbs continues his story of the Teamsters in the Midwest. (An earlier book of his, published by Monad Press in 1972, was *Teamster Rebellion*.)

Two different types led the breakthrough in Teamster power: Dave Beck was a conservative, nonideological trade unionist; Farrell Dobbs and his associates were Trotskyists. What did they both have in common? They both realized that the key to Teamster growth was to organize the over-the-road drivers, and that the key to increased power was developing a regional base in between the local union and the cautious president of the Teamsters, David Tobin. Both succeeded in achieving these goals, and with their success the Teamsters grew to be so powerful that there are those who claim that they practice collective bludgeoning with the weaker employers rather than collective bargaining.

Garnel's book, which shows the signs of being an earlier doctoral dissertation, is a scholarly and somewhat dull work that covers the development of commercial trucking, the history of the Teamsters, the growing influence of Beck, the developing of the Highway Drivers Council of California and the Western Conference of Teamsters, the organizing drives, and the patterns of collective bargaining that emerged. The facts are there: the achievement of pay by the mile (a partial explanation of

the recent strikes by truckers as a result of the proposed fifty-five-miles-an-hour limit), pattern bargaining, multiemployer and multilocal bargaining, and many other details. Indeed the facts are there, but one would have welcomed more analysis. For example, Beck's public reputation is not based on his organizing Teamsters; it is on his supposed organized corruption. Little attention is paid to the issue.

Dobbs's book is vastly different from Garnel's volume. Dobbs has written a memoir. He describes, from his point of view, what took place. He portrays the idealism and commitment of the people who sought to organize the Teamsters with him. But he also reports how, as Trotskyists, he and his associates would frequently consult with Jim Cannon, the leader of the Trotskyists movement, before acting.

Both Beck and Dobbs launched the Teamsters into power. Both no longer have any real influence within the organization. But Hoffa and now Fitzsimmons are cut from the Beck mold. The Teamsters, the American general workers' union, is a conservative labor organization—unlike many general workers' unions abroad, which are frequently politically to the left. How both achieve power is described in these books. The eventual use to which the power was put, except in collective bargaining, will have to await future studies.

ALBERT A. BLUM  
University of Texas,  
Austin

BRUCE KUKLICK. *American Policy and the Division of Germany: The Clash with Russia over Reparations*. Ithaca: Cornell University Press. 1972. Pp. viii, 286. \$9.50.

With this monograph on United States policy toward Germany during the years 1939–46, Bruce Kuklick joins a growing number of young historians who have argued that U.S. policy makers were ultimately responsible for the origins of the cold war and who deny that U.S. policy was based on the desire to curb Soviet expansionism as by a multilateral conception of a desirable world order. Kuklick concedes that U.S. policy toward Germany was often inconsistent, that key advisers diverged in their policy recommendations, and that

their influence varied over time. He concludes, however, that in the final analysis neither disorder nor bureaucratic infighting fundamentally determined policy but rather that "multilateralism decisively shaped American diplomacy toward Germany and was at the center of the serious American disputes with the Russians. State Department foreign policy is the single most important thread for leading one through the tangled maze of great-power strategies" (pp. 226–27).

By focusing on U.S. policy concerning reparations and the political divisions of postwar Germany, Kuklick has provided an in-depth study of the broader interpretations advanced by other radical historians—notably Barton Bernstein, Lloyd Gardner, Gabriel Kolko, and William Williams. Although focusing primarily on the State Department, Kuklick has not neglected the conflicting policy advanced by the War and Treasury Departments during the war and postwar years or the differing priorities that shaped the responses of Presidents Roosevelt and Truman. His monograph is a sophisticated and thorough analysis of the pressures contributing to the fitful development of our policy toward Germany. In a further sense, Kuklick (in contrast to John Gimbel) minimizes the obstructionist postwar role of the French; he argues that at best the French simply delayed matters, that U.S. policy—based on the desire to integrate Germany into a multilateral economy—principally attempted to force Soviet compliance with this objective.

A thoroughly researched examination into the available archival records, this is also a thoughtful consideration of the conclusions of earlier diplomatic and political historians. The result is an important historical contribution. Unevenly written, at times highly interpretive and unduly complicated and tendentious, Kuklick's insightful and persuasive account succeeds in underpinning the conclusions of Williams, Kolko, and N. Gordon Levin about United States policy in the twentieth century. His monograph should challenge both the orthodox historians and the liberal revisionists (like myself) to reassess their basic assumptions about the formulation of U.S. foreign policy and the origins of the cold war.

ATHAN THEOHARIS  
Marquette University

ROBERT L. UNDERBRINK. *Destination Corregidor*. Annapolis: United States Naval Institute. 1971. Pp. xiv, 240. \$9.50.

Without question one of the most poignantly dramatic events in recent American history was the defeat and surrender of a poorly equipped, surrounded and starving American-Filipino army in the Philippines at the hands of a vigorous and overwhelmingly superior Japanese attack. Although the Philippine debacle has been written about in detail since World War II, practically nothing is known about the attempts made to supply the beleaguered garrison on Bataan and Corregidor. Indeed, most Americans probably believe that since the Philippines were cut off early in the great southward Japanese drive no serious consideration was even given to supplying MacArthur's forces. In *Destination Corregidor*, Robert Underbrink tells of the decision by President Roosevelt and the military commands in Washington to attempt to keep the Philippines supplied, the somewhat informal organizational and command network that was established to mount the operation, and finally the belated, inadequate, and heroic efforts of those who carried out the operations themselves.

*Destination Corregidor* is primarily the story of the operations told in personal detail. The network that was established involved the use of some military resources, mainly aircraft and submarines, but its main component was an American-financed system of chartering privately owned merchant vessels in Australia and the Dutch East Indies. These larger ships carried food, ammunition, and medical supplies to Mindanao and Cebu in the southern and central Philippines, and from there small inter-island Philippine vessels attempted the most hazardous part of the undertaking carrying the supplies through to Bataan and Corregidor.

The book, while a good account of a fascinating and nearly unknown event of World War II, is not complete enough to satisfy the scholar. The planning aspect of the operations is not thoroughly discussed. The reasons why aircraft could not be allocated in larger numbers for the supply effort is not made clear. The navy's decision not to allow more than a handful of submarines to be employed is stated, but not the discussion and the reasoning behind the decision.

As for the rest, the book is written around a chronological framework that makes for considerable shifting from one operation in progress to another. A topical approach might have produced less confusion. There are no footnotes, although a bibliographical essay indicates that primary and secondary sources were used, with much emphasis upon interviews with participants in the operations themselves. Two maps are adequate. In sum, it is a fascinating little book, but unfortunately it raises as many questions as it answers.

ERNEST ANDRADE, JR.  
University of Colorado,  
Denver

*Foreign Relations of the United States, 1947. Volume 1, General; The United Nations.* (Department of State Publication 8674.) Washington: Government Printing Office. 1973. Pp. xxiii, 1096. \$6.80 postpaid.

This volume makes an important addition to the documentation on U.S. foreign policy and the origins of the cold war. The materials on the United Nations indicate a rapidly deteriorating political climate, epitomized by the Soviet introduction in the General Assembly of a resolution attacking the United States for "criminal propaganda for a new war." Some observers wondered whether the "warmongering" resolution was an offensive or a defensive move, while U.S. Ambassador Walter B. Smith and George F. Kennan thought the Kremlin might be planning to withdraw from the United Nations.

Related documentation shows that the Joint Chiefs of Staff were indeed planning for a possible war with America's "ideological enemies," that is, the USSR. They argued that abandoning aid to Nationalist China would permit concentration of "our forces for a crushing offensive from the West against our primary ideological opponents" (p. 745). They believed also that "Japan is the one nation which could contain large armed forces of our ideological opponents in the Far East while the United States and her allies in the West launched a major offensive in that area" (*ibid.*). In 1947 State Department counselor Charles E. Bohlen expected the East-West confrontation to mature in months to a point where war

might erupt. If this crisis were "to be solved short of war, it must result in a radical and basic change in Soviet policies" (p. 765). George F. Kennan and Secretary of State Marshall, however, contended that the danger of an imminent war was exaggerated. They nonetheless joined other segments of the foreign policy establishment in plans to assimilate West Germany and Japan on the side of the free world. Many Americans considered that Moscow would soon extinguish Czechoslovak independence, but wrote this off as something Washington could not avert.

Documentation on arms controls reveals a paradox: Washington was adamant on focusing negotiations on atomic energy, that is, on the very domain where America enjoyed a monopoly. Washington opposed Soviet proposals for a Commission on Conventional Armaments (in which Moscow was believed to be stronger than the United States) as mere propaganda. As in later years, Washington could see little way of testing Soviet intentions or calling a possible bluff, even though most other UN members resented Washington's dogmatism. The Joint Chiefs of Staff concurred and added their objection to any requirement to supply information on U.S. forces abroad, even if it were already in the public domain.

This book also contains important documentation on trusteeship agreements for the former Japanese-mandated islands and nonself-governing territories outside the UN trusteeship system; on U.S. policies relating to world shortages in resources; on polar regions; and on the origins of NATO, the CIA, and the National Security Council, much of which gives contemporary headlines an overtone of *déjà vu*.

WALTER C. CLEMENS, JR.  
Boston University

*Foreign Relations of the United States, 1947. Volume 2, Council of Foreign Ministers; Germany and Austria, 1948, volume 2, Germany and Austria.* (Department of State Publications 8530 and 8660.) Washington: Government Printing Office. 1972; 1973. Pp. xxx, 1251; xxx, 1575. \$6.00; \$8.75 postpaid.

These massive volumes document the evolution of U.S. policy toward Germany and Austria from the Council of Foreign Ministers' meeting in Moscow in April 1947, the last serious at-

tempt to negotiate peace treaties for Germany and Austria, through the first six months of the Berlin blockade. It was a critical period in American foreign policy, and the decision to abandon four-power diplomacy and to integrate Germany into a Western European community marks a watershed in the history of the cold war.

Included here are a rich variety of documents. There are extensive records of the fruitless proceedings of the Council of Foreign Ministers; detailed reports on developments in Germany from political adviser Robert Murphy and Military Governor Lucius D. Clay; exchanges of notes between the United States and the other nations involved in Germany and Austria; and records of policy formulation within the State Department and, to a lesser extent, the Truman administration as a whole. The papers highlight a number of themes: the breakdown of Soviet-American discussions; differences among the United States and Britain and France, particularly on the economic rehabilitation of Germany, and debates on tactics among American officials themselves.

The volumes make clear that the failure of the Moscow meeting of the Council of Foreign Ministers ended any hopes for a four-power treaty for Germany. Shortly afterward, Secretary of State George C. Marshall proposed a "spiritual federation of the west" to indicate to the Russians that "having gone so far they could not advance any further." By the beginning of 1948 the United States was firmly committed to the establishment of a German government in the Western zone, and at the London conference in the spring it placed heavy pressure on the reluctant French to go along.

The most interesting portions of the 1948 volume deal with the Berlin crisis. It is evident that the United States anticipated and was prepared for the blockade. American officials never seriously considered abandoning Berlin, and though they did not feel there was a great risk of war they were prepared to fight if necessary. Once the airlift had been demonstrated workable the State Department was reluctant to open negotiations for fear of restoring the prestige that Russia had lost by the blockade. When serious discussions began the United States insisted that they should be limited to

Berlin and should not concern the broader questions of Germany's future, on which American policy was already firmly set. Despite the possibility of war Americans saw advantages to be gained from the crisis. It made the Russians look bad and helped sway the recalcitrant French and Germans toward accepting the proposals developed in London.

The documents provide some fascinating glimpses into the personalities of the period: a pathetic Archduke Otto pleading with "dear Mr. Acheson" to prevent confiscation of Hapsburg property; Secretary Marshall bluntly informing President Auriol of France, "I am not a diplomat: I mean exactly what I say and there is no use reading between the lines"; Winston Churchill advising Ambassador Lewis Douglas in April 1948 that the Soviets should now be told that if they did not "retire from Berlin" and "abandon Eastern Germany," the United States and Britain would "raze their cities"; Ambassador Walter Bedell Smith commenting in September 1948 that the "present hysterical outburst of humanitarian feelings" about the Berliners "keeps reminding me that just 3½ years ago I would have been considered a hero if I had succeeded in exterminating these same Germans with bombs."

These two volumes meet the high standards that the historical office of the Department of State has set for the *Foreign Relations* series. The editors faced an unenviable challenge in selecting the most important and revealing items from the mountains of available archival materials, and they have performed their task capably. The editorial notes and indexes are well done.

The collections are not without shortcomings. The inaccessibility of White House records means that the historian must still rely on Truman's inadequate *Memoirs* for such important top-level decisions as the institution of the airlift and the rejection of Clay's proposal to send an armed convoy through the blockade. Scholars seeking to examine occupation policies must supplement these materials with army records. In addition the introduction to the 1948 volume righteously declares that the failure to achieve a four-power agreement on Germany was the exclusive result of "Soviet intransigence," a loaded statement more appropriate for a white paper than for a scholarly volume.

Nevertheless, these two volumes will be an indispensable source for those seeking to understand the development of East-West conflict over Germany, perhaps the key issue in the early cold war.

GEORGE C. HERRING, JR.  
University of Kentucky

THOMAS G. PATERSON, edited with an introduction by. *Cold War Critics: Alternatives to American Foreign Policy in the Truman Years*. Chicago: Quadrangle Books. 1971. Pp. 313. Cloth \$10.00, paper \$2.95.

RALPH STAVINS *et al.* *Washington Plans an Aggressive War*. New York: Vintage Books. 1971. Pp. x, 374. \$1.95.

What Senator J. W. Fulbright has called "the arrogance of power" is what these books are all about. One of them deals with the architects of the Vietnam War while the other discusses the dissidents who had the temerity to oppose Harry Truman's cold-war policies upheld in Congress by Democrats and Republicans alike. The thread uniting the two studies is the use of executive power, for the spirit of bipartisanship in foreign policy created during the late 1940s may well have enabled the presidents of the 1960s to plan their war without fear of the resistance faced by Woodrow Wilson, Franklin Roosevelt, or Truman himself before he discovered the magic word that opened the doors of cooperation with a previously hostile Congress: anticommunism. From 1948 until the past few years, opposition to the administration's foreign policies was considered disloyal. And it was just this lack of a respected opposition that permitted Kennedy and Johnson, as Stavins makes clear, to initiate plans for war without having to consider the attitudes of Congress or the people in any but a manipulative sense.

Paterson's book examines the position of those brave enough to question Truman's policies despite the charges that were certain to be hurled against them for lack of faith. All they had in common was a desire to seek accommodation with the Soviet Union based upon the assumption that America's erstwhile ally did not pose a military threat to Western Europe or to the United States. Even in evaluating the USSR they ranged from the somewhat naive trust of Senators Claude Pepper or Glen Taylor

to the out-right hostility of Senator Robert Taft, although most were critical of the undemocratic nature of the Soviet state and its behavior in Eastern Europe. Some critics, notably Henry Wallace, shared the administration's desire to expand the Open Door into Eastern Europe but disagreed on effective methods. Wallace's ideas, paradoxically, have metamorphosed into establishment policy under the unlikely sponsorship of Richard Nixon. Other critics like Walter Lippmann never quite understood the relationship between politics and economics and thus questioned the wisdom of seeking to penetrate Russia's satellites rather than trying to secure better relations with the Soviet Union.

Few were entirely consistent in their thought. Barton Bernstein describes how Lippmann oscillated between the desire to expand the area of democracy and free trade and a recognition of Stalin's valid security needs, which precluded these goals. Thus he could oppose the Truman Doctrine as threatening the Soviet Union and then support the Marshall Plan without realizing that it would be viewed as equally menacing. Similarly, William Bermann shows how James P. Warburg conceived of the possibilities of a strengthened, unified Germany enlarged by territory east of the Oder-Neisse, with a friendly USSR acquiescing. In his sketch of I. F. Stone, Norman Kaner explains how the journalist's early disenchantment with the administration led him to suspect that Truman and Syngman Rhee deliberately provoked the North Korean attack in 1950. But surely the flexible, non-ideological approach to foreign policy offered by such critics as Wallace, Lippmann, Stone, or Warburg offered greater possibilities for peaceful coexistence with the Soviet Union than the stubbornly consistent anticommunist stance adopted by the Truman administration.

Henry Berger's critique of Senator Taft seems out of place among cold war critics who hoped to achieve a rapprochement with Russia. Taft may have opposed an unlimited commitment of American troops to NATO, but his initial hesitation about the Truman Doctrine did not affect his vote for it. His objections to the Marshall Plan were based mainly upon expense, and his bellicosity toward the Communists in Asia indicate that he may have believed simply that Truman had chosen the wrong battleground for the struggle between communism and democracy.

Taft, however, does seem relevant for his concern over the effects of the erosion of congressional authority and expansion of executive initiative in foreign relations—a concern not shared by liberal critics of the cold war. Stavins, Barnet, and Raskin prove Taft's fears to have been well founded. In *Washington Plans an Aggressive War* they demonstrate how the abdication of Congress placed Kennedy, Johnson, and their advisers in a position where they could ignore the legislature or at best seek its support to ratify in advance actions secretly decided upon, as with the Gulf of Tonkin Resolution. In the most cogent section of the book, Stavins depicts in detail the decisions that led step by step to open warfare. Barnet analyzes the cold-war-type thinking that made those steps seem logical, and Raskin explains the reasons for the atrophy of congressional power and suggests a program for demilitarizing the national security establishment and creating standards of individual responsibility for policy makers.

Although the prior publication of the Pentagon Papers has stolen much of their thunder, Stavins and Barnet are particularly adept at describing how Kennedy, Johnson, and their advisers considered only those policy alternatives consistent with the cold war myths Truman had utilized. And if communism is viewed as inevitably monolithic and expansionist, preventive war can seem rational and even necessary. Civilian and military advisers alike, as well as the presidents themselves, accepted these suppositions. The only difference between them seems to have been less caution among the civilians at first—when Dean Rusk and Robert McNamara wanted to commit American troops before the military men were willing—and more political acumen later—as Johnson and his civilian advisers decided to hold off the bombardment of North Vietnam until after the 1964 elections.

With these assumptions both presidents were able to justify to themselves the deceptions enabling them to make war in Vietnam. They lied to Congress and the people about the reasons for our initial involvement and expansion of the war and never doubted their right to do so. Congress abandoned its constitutional responsibility and accepted those policies without question. That is the message of this book, and a frightening and convincing message



it is, despite the numbness fostered by the numerous studies about Vietnam in the past few years.

The level of scholarship in both books is high. In *Cold War Critics* Bernstein's essay on Lippmann and Radosh and Liggio's analysis of Wallace's thought are particularly valuable, although one questions the pertinence of "Black Critics of Colonialism and the Cold War." James Warburg's role as a critic of foreign policy, incidentally, had begun during rather than after the war as Bermann indicates, when he was forced from his policy-making position in the Office of War Information for insisting on unequivocally prodemocratic positions towards liberated nations, although Roosevelt was moving away from such policies. In *Washington Plans an Aggressive War* some citations would have been helpful for the documents and conversations Stavins cites. Raskin's use of the Nuremberg trials to construct a basis for demilitarization of the foreign policy hierarchy and establish individual responsibility seems praiseworthy but rather quixotic. These are minor points in two welcome additions to the growing literature that takes as theme the higher patriotism of Carl Schurz: "My country, right or wrong: if right, to be kept right; if wrong, to be set right."

SYDNEY WEINBERG  
Ramapo College

DANIEL C. THOMPSON. *Private Black Colleges at the Crossroads*. (Contributions in Afro-American and African Studies, number 13.) Westport, Conn.: Greenwood Press. 1973. Pp. xi, 308. \$11.00.

This book is an effort "to understand and interpret" the past activities and contributions of private black colleges and to discuss current problems, opportunities, and prospects. The author is a black sociologist whose interest in this subject dates at least from his 1956 doctoral dissertation on "Teachers in Negro Colleges." Although the time-focus is mainly the present there are special references to the origins of these colleges and the decade of the 1960s.

The book is based on research in contemporary scholarly literature on higher education and the study of ten unidentified private black colleges by questionnaires and interviews.

Information on the students derives almost exclusively from use of questionnaire and interview. Student newspapers and newsheets that have been produced in recent years apparently are not used. Despite the existence of other relevant sources almost exclusive use of the questionnaire and interview method also applies to the information presented on the faculty. It is regrettable that the bibliography and other references reflect little or no use of the material on black education that has appeared in recent years in such excellent publications as *Black Scholar*, *Negro Digest*, and *Black World*. Also conspicuous by their relative absence are such sources as histories of individual black colleges; memoirs, autobiographies, or biographies of some of their administrators, teachers, and graduates; and the *Journal of Negro Education*, *Journal of Negro History*, *Phylon*, and *Quarterly Review of Higher Education Among Negroes*.

There is considerable variation in the length of chapters. Three are 13 pages in length; the longest are two chapters on "The Students" and two on "The Faculty," which average 40 pages each. The last three chapters, only 9, 13, and 18 pages in length respectively, are on "Economic Status," "Social Dynamics," and "The Future."

There are fifty-three private black colleges, all but three of them (Hampton Institute, Tuskegee Institute, and Bishop College) classified as small (p. 37). Half of these institutions "average less than 700 students." Although today 48 per cent of the nation's blacks live outside the South, this region is still the source of from 90 to 95 per cent of the students who attend private black colleges. Eighty-six per cent of these colleges were started by religious bodies, including all but two of the forty colleges that are now in the United Negro College Fund (pp. 275-76). The denominations "make relatively small financial contributions to their Black colleges" (p. 276).

Due to such factors as greater difficulties with the old problem of finding adequate financial resources, once all-white colleges and universities now recruiting black students and faculty, and rising expectations by black students, private black colleges are now at a critical crossroads. A crisis exists because for their problems "there is no reliable solution avail-

able" (p. 261). Although where financial support is concerned, "churches, foundations, individual philanthropists, business corporations and government on all levels have blatantly discriminated against Black colleges," these groups criticize the colleges for not meeting certain high standards that only better funding could make possible (p. 267). This study of struggle confirms that, despite affluence, the nation has had only a token commitment to the uplift of the black minority.

Although Thompson, too, is consistently critical he is far from agreeing with such thinkers as Christopher Jencks and David Riesman that private black colleges are "disaster areas." Thompson points to many reasons why the entire nation should give adequate financial support to these institutions. Numerous other changes are needed in several areas. For example, due to such factors as population shifts and technological changes some of these colleges "need to effectuate systematic mergers, relocation and reorganization" (p. 275). Also, in course offerings very often these colleges have been "simply imitators" (p. 273). Despite intense competition they must recruit and hold more of the best qualified faculty and better high school graduates and do a better job of projecting their strong points. Much of their old image has passed with the world that gave it birth. Before the 1960s all blacks at these colleges were regarded by local whites as "good niggers"; they were "neutral" in the communities—but the sit-ins and other protests ended this neutrality (pp. 15–16).

The author's definition of a private black college makes it appear that the institution about whose existence he has written at length is yet to be created. He writes: "The concept 'Black college' is not intended to describe the racial composition of the student body or faculty, as is implied in the much-used concept 'predominantly Black college.' It means instead that whatever the racial composition of the student bodies or faculties . . . control will remain with Black trustees and administrators and with selected non-Blacks acting in behalf of Black people and their unique interests" (p. 277). Where these institutions are concerned, when did any blacks ever really have such "control"?

The final chapters on "Social Dynamics" and "The Future" fail to make it clear that the fate

of both private and public predominantly black colleges probably will be determined to a large extent in the future, as they have been in the past, by the level of sophistication of organization and political struggle of blacks. Too much of this book comes through as the old idealistic plea to white liberalism. Until blacks can move from pleas to demands based on organized political and economic power private black colleges—and other black institutions—may well do what many too often have done, that is, move from one crossroads to another. Still, for historians in several specialties and the general reader, there is much valuable information in this book, information presented in a manner that reflects the author's own good training, experience, and insights.

EARL E. THORPE

North Carolina Central University

HARLAND B. MOULTON. *From Superiority to Parity: The United States and the Strategic Arms Race, 1961–1971*. Westport, Conn.: Greenwood Press. 1973. Pp. xii, 333. \$12.00.

This type of book frets the traditional historian. It deals with vital topics—weapons and strategies for nuclear defense of the United States, 1945–71, but the information and its presentation defy the usual rigorous criticism demanded by historians. We like to examine evidence from which statements of fact or conclusions are drawn and then see if we agree. But for post-1945 national defense history the sources are largely closed to non-Department of Defense (DOD) historians. Officials of the DOD regularly testify before Congress concerning the numbers of manned bombers, missiles, warhead megatonnage, or Polaris submarines on station, but the historian's ability to verify such information stops there. The number of aircraft, submarines, and missiles existing in the USSR are often stated by the same spokesmen with the source being given as "the intelligence community" of the United States or possibly the annual published estimates of the Institute for Strategic Studies. In brief, though Dr. Moulton has written a history of the United States and the strategic arms race, a large portion of the basic primary sources are closed to him and to us.

If we accept this limited ability to review the

sources, and with it the possibility that DOD statistics and estimates of "the threat" (USSR military strength) might have been in error occasionally, then there is much of value in this book. The author is a staff member at the National War College and thus has the opportunity to study national military planning and its attendant classified and public literature. Much of this book began as a doctoral dissertation in history, but it has been reorganized, rewritten, and extended. The first and eighth chapters will be useful to the classroom historian because of their competent presentation of arms race history from 1945 to 1960 (prologue) and the story from 1965 to 1971 (epilogue). The heart of the book analyzes in detail the Kennedy-Johnson years (1960-65) in six full chapters. Here the reader must grapple with the DOD's peculiar argot, but patience will lead to a reasonable comprehension of such terms as "massive retaliation," "controlled response," "mutual deterrence," "damage limitation," "assured destruction," and "counterforce strategy." Unlike Herman Kahn, the author presents no awful scenarios, but the reader is left with few doubts that the DOD does plan for nuclear war. His book arrives at the rather ironic conclusion, at least for those who opposed American participation in the Vietnam War, that the conflict was so expensive that the United States was forced to allow the USSR to move to a position of nuclear armaments parity with it. Having achieved such parity the USSR then agreed to engage in Strategic Arms Limitation Talks. As frightful as the subject is, the nuclear arms race is history and requires careful study. Dr. Moulton has helped us all with his work.

GERALD E. WHEELER  
*San José State University*

#### CANADA

DAVID S. MACMILLAN, [editor]. *Canadian Business History: Selected Studies, 1497-1971*. [Toronto:] McClelland and Stewart. 1972. Pp. 346. \$12.50.

The sixteen papers in this volume range from lengthy chronicles, whose detail obfuscates rather than clarifies, to useful interpretations, whose specialized research is linked to questions of central importance in business history. Although their findings could have been pre-

sented in more succinct form, the latter category includes Gerald Tulchinsky's profile of the Montreal business community, T. W. Acheson's analysis of the social characteristics of Canadian businessmen, Patricia Roy's study of lobbying, and Douglas McCalla's investigation of financing Canadian railways in the London money market. But the most noteworthy contribution is "'Dyspepsia of the Mind': The Canadian Businessman and His Enemies," by Michael Bliss.

Bliss argues that at the turn of the century Canadian businessmen, in contrast to Gabriel Kolko's picture of their contemporaries in the United States, seriously doubted their ability to dominate the political process. Although the author's method is fairly described as proof by selective quotation, his essay has value for two reasons. First, it provides a corrective to recent interpretations that see businessmen as the key force in shaping public policy, and therefore has significance for current historical controversies. Second (and more important given the underdeveloped state of Canadian business history), it concludes with an assessment of the implications of the author's findings and enumerates the specific questions that follow from his work.

Hardly any of the other authors are so explicitly concerned with framing questions for future research. Even Frederick Armstrong's historiographical essay is weak in this respect; it is more descriptive than critical. It is true that Alan Wilson suggests a number of important problems, but the bulk of his essay is a survey (valuable as such) of the evolution of business history as a distinct field of study in Europe, the United States, and Canada.

Many of the other essays illustrate the consequences of poorly conceived problems. For example, J. Lorne MacDougall's five-page paper on the railroad entrepreneur George Stephen concludes, "What a pity it is that all the interesting questions are nearly insoluble" (p. 196). The problem with this essay is that it does not really raise any questions, much less answer them. Similarly, the contribution by the editor, David Macmillan, on Scottish mercantile interests in Canada, lacks a strong interpretive framework and compounds the error by the inclusion of excruciating detail. In the same way, Richard Rice's study of "The Wrights of

Saint John" inundates the reader with incompletely digested information. More carefully framed questions would have provided the key to distinguishing relevant from irrelevant data. Unfortunately too many of the papers in this collection prove inadequate when measured by such an elementary standard.

A recent special issue of *Business History Review* (47 [Summer, 1973]), edited by Glenn Porter in consultation with Robert Cuff, provides a better sampling of Canadian business history than this volume.

THOMAS E. VADNEY  
University of Manitoba

GUY-MARIE OURY. *Marie de l'Incarnation (1599-1672)*. In two volumes. Quebec: Presses de l'Université Laval. 1973. Pp. ix, 311; 320-607. \$16.00.

Marie Guyart (1599-1672) is one of several Canadian missionaries whose vigor of mind and body and complete dedication to martyrdom in life or death made them fit colleagues for the better known Jesuit fathers in the first French missions in North America. Her spiritual development followed a familiar pattern: notably pious in early years and conscious of a vocation that her parents did not recognize, she experienced a sudden "conversion" as a very young woman. This was followed by a sense of direct divine guidance that led to her entering the newly founded convent of the Ursulines at Tours and later to Canada to found a community of Ursulines dedicated to the conversion of the Indians and officially for the instruction of Indian girls.

She was able to obey the call to Canada, which, she believed, came directly from the Virgin Mary, only at the age of forty after extraordinary hardships and difficulties: the opposition of her family (she had been married and left a widow with a young son and a bankrupt business before she was twenty and for years supported herself by acting as a very useful drudge in her brother-in-law's household); the austerities she inflicted on herself and the apparent harshness of a series of spiritual directors; doubt on the part of ecclesiastical authorities about her mission; and a complete lack of funds to carry it out.

In 1639, however, the difficulties were ap-

parently surmounted. With some financial support provided by the pious but unpredictable Mme. de la Peltrie she set sail for New France with two companions. The hardships of the voyage were a prelude to many years of bare survival, enduring cold, hunger, and—a special trial for the cloistered nuns—a complete absence of privacy in the tiny house in Lower Town Quebec where they were at first accommodated. Physical trials were aggravated by the complexity of the relations of the new community with Ursulines in France and with the local Jesuit superior, to whom Marie seems to have made her vow of obedience with certain mental reservations.

In spite of hardships, anxieties, overwork, and serious illness, Marie de l'Incarnation lived thirty years in Quebec, supervised the building of two successive houses in the Upper Town, established a school that is still famous today, acquired sufficient knowledge to spend her declining years preparing dictionaries and spiritual works in four different Indian languages, and, cloistered as she was, came to know every leading man in Quebec, cleric or lay, including the great Laval who was also occasionally hampered by her reserved obedience.

Dom Oury has been at much pains to criticize and exploit his limited material. As he says, "La discretion de l'Ursuline dans ses lettres . . . est desesperante pour l'historien." Unfortunately, he has attempted to follow simultaneously three themes or threads: the inner spiritual growth of Marie, his primary interest; the story of her life and work in France and in Canada, for which his material is limited; and current developments in Canada during the critical thirty-three years (1639-72) on which his information is barely adequate. He does not quite succeed in pulling together these three threads, and too often in his secular narrative he resorts to a rather awkward topical arrangement.

The result is a long work, not entirely successful as a biography. Yet it has clearly been a labor of love and it does offer a credible picture of a heroic indomitable, and charming woman.

HILDA NEATBY  
University of Saskatchewan

GEORGE F. G. STANLEY. *Canada Invaded, 1775-1776*. (Canadian War Museum, Historical Publications number 8.) Toronto: Hakkert. 1973. Pp. xiv, 186. \$8.95.

In this unpretentious little volume Professor Stanley, the noted Canadian military historian, describes the attempt by American rebels to conquer Canada and thus give the new nation complete control of the eastern continent. In his account of the two-pronged invasion the author, with a masterly grasp of detail, follows the fragmented push of Philip Schuyler and Richard Montgomery up the Champlain-Richelieu route; emphasizes again the extreme difficulties faced by Benedict Arnold on the Kennebec and Chaudière rivers, resulting in a fatally weakened force that finally emerged before the walls of Quebec; and reminds us of the remarkable absence of coordination between the two American armies. On the Canadian side the author points to the depleted numbers of British regulars, the general unpreparedness in the initial months, partly due to faulty information on the disposition of the habitants, and argues that the formation of the Royal Highland Emigrants was an important factor in stiffening Canadian defenses before the arrival of British reinforcements in the spring of 1776. (The frontispiece, a colorful pen drawing of a tall, proud, and kilted officer of this regiment, complete with un-Scottish tan, forewarns the reader of this emphasis in the text.) While critical of Guy Carleton for overzealous caution Stanley offers the sound assessment that given difficult circumstances be performed sagely and adequately.

The treatment of the political context of the invasions is thin, particularly in the cavalier summary of the origins of the Revolution during which the author laments that American opposition to the Quebec Act was mere demagoguery (p. 16). Such remarks may please some of Professor Stanley's Canadian followers but they are unscholarly and obtrusive. On the military events, however, the author has a sure touch and tells a complex story with accuracy and grace all the while managing to include such delectably irrelevant details as the fact that sporrans in Canada were made of racoon skins.

Yet when all these qualities are mentioned the stubborn truth remains that precious little

that is new emerges from Stanley's volume. The book's main distinctions compared with existing accounts are that it is short, readable, and copiously illustrated.

GORDON STEWART

Michigan State University

*Documents on Canadian External Affairs*. Volume 1, 1909-1918 (1967, pp. ix, 906, \$10.00); volume 2, *The Paris Peace Conference of 1919*, edited by R. A. MACKAY (1969, pp. xxx, 237, \$5.00); volume 3, 1919-1925, edited by LOVELL C. CLARK (1970, pp. cxviii, 1007, \$12.00); volume 4, 1926-1930, edited by ALEX. I. INGLIS (1971, pp. cxix, 1038, \$12.75); volume 5, 1931-1935, edited by ALEX. I. INGLIS (1973, pp. cxv, 818, \$10.00); volume 6, 1936-1939, edited by JOHN A. MUNRO (1972, pp. cxlv, 1334, \$12.75). Ottawa: Department of External Affairs.

Prepared by the Historical Division of the Department of External Affairs for the use of scholars and for the simultaneous enlightenment of the general public, these six volumes present official papers on Canadian foreign policy from the time of the establishment of the department in 1909 until Canada became involved in World War II in 1939. At the beginning of that period Canada was a colonial dependency with little say, or even interest, in world affairs. As a consequence of Canada's part in the First World War and the peace conferences, and partly through representation in the League of Nations, Canada's concern with international problems proliferated. A small, but highly competent, Department of External Affairs with a diplomatic service was developed. Between the wars Canadian policy, much influenced by American example and the need to conform to it, while protecting newly acquired full autonomy, sought to avoid a repetition of the experiences of 1914-18 by evading commitments. The documents in these six volumes illustrate and substantiate the main themes in the formulation and implementation of that policy and of other foreign relationships up to the time of Canada's deliberate demonstration of independence in the act of entering the Second World War.

Separate editors were recruited from outside the department for each of these volumes (except that volumes four and five were done by a resident historian, Alex Inglis) and the editors were given full access to all External Af-

fairs documents and full control over selection for publication. The editors state that no documents were withheld or omitted for reasons of state or to avoid embarrassment to any individual or group, an exception being in volume six where the papers about the abdication are omitted in conformity with a Commonwealth agreement, and a paper about smuggling was omitted to avoid embarrassment to a person still alive.

The primary aim of the project was to present material that would show what happened and why it happened. In the earlier volumes this was taken to mean the selection of papers that represented the senior level of government policy and decision making and the omission of the contributions by lower-level officials unless they happened to be the most revealing expression of a problem, position, or policy that is extant. Documents that had been published elsewhere were omitted unless they were considered necessary to explain an aspect of policy or policy making. On the other hand material from other collections, for instance from prime ministers' or governor generals' papers, were included if the editor thought he needed them to round out his story. For volume four and later volumes, dating from 1926, this somewhat restrictive publication policy was changed. In volumes four, five, and six there is a greater emphasis on policy formulation, some personal letters are included, and a little more information about the way policies were evolved has been made available.

Each volume covers a period of time, usually five years (except that volume two is devoted to the Paris Peace Conference in 1919), and the papers in each volume are arranged chronologically with sections dealing with certain general topics. Because the material in the early years was sparse, because problems were then fewer and less complex, and perhaps also, because, as has been said, personal letters were excluded, the early volumes are straightforward and readable as they stand. But the consequence is that they are of less value to historians for research purposes, especially as they repeat familiar themes. In later volumes several problems are often included within the general topic headings, and, as the papers within the section appear in date order, continuity of subject is lost. The later volumes are thus

less likely to appeal to the reading public. However, as the number of documents that can be collected between hard covers is strictly limited and as minutes and various inservice position papers are often not included in these published volumes this means that the serious scholar must usually still go to the public archives. Unfortunately, these published volumes also fail to give the preliminary help he needs to shorten his labors there because the editors do not give file numbers or any other form of reference for the documents that they have published, a serious omission that may have been a result of the fact that when the publications project was initiated it was thought that it would be a long time before access to department papers was permitted to the public.

Another weakness detracts from the convenience of using these books as source material for teaching purposes or preliminary research. The indexes not only vary from one volume to another, but are also quite inadequate. Lack of conformity is demonstrated by the fact that the index in volume two refers to documents by number, but indexes in other volumes refer to pages. The indexes in volume one and six include subjects, instruments, and the names of principal persons, but the indexes in the volumes in between omit names. The index in volume five is even briefer than the others. Another inconsistency is that in volumes two to six the documents are preceded by a list of principal persons and a list of documents. People of lesser importance, and some famous persons who appear only incidentally, are not identified in any way.

Introductions are short and thin and deal with little more than editorial practice and method. Later introductions included some interpretive comment but are still only two or three pages long. Volume five, covering the Bennett years, reveals a little more of an editor's personal opinion, especially when he says that he hopes that the reader will find that the volume establishes R. B. Bennett's place in Canadian development by showing that he was something more than a mere diversion from the direct road to national status. But volume six seems to demonstrate Canadian supersensitivity about national independence when the editor bridles because Roosevelt de-

scribed Canada in 1938 as "part of the sisterhood of the British Empire." These rare glimpses of editorial opinion are no substitute for an outline of the content of the documents or some other helpful guide to the material like a full index.

Volumes one and two were published both in French and English with the text in the original language as used by the writer. French originals are hard to find. In later volumes a bilingual form is used with English and French appearing on pages side by side in the introductions and with separate indexes in English and French. Is there any significance in the fact that the French version comes first in titles and everywhere else?

One of the editors indicates that the choice of material for publication in this series was a highly personal operation. He said that a different editor could have made a quite different selection. Without a long survey of the archives it would be impossible to attempt to evaluate the quality and nature of each editor's selection. Judging only by the appearance of the series itself, on the whole it is remarkable how far the project appears to have succeeded in its first objective, that of making available to the public documentary evidence of the main basis of Canadian policy, and this is so despite difficulties induced by having a secondary conflicting aim, the desire to provide scholars with a resource tool.

Official series of documents produced by other foreign sources in other countries always seem to end up in the prisoner's dock charged with having doctored the evidence if only by selection and exclusion, but often even by tampering with texts. Great precautions were obviously taken to ensure that Canada did not sin. The result may seem bland and unexciting in some areas, but, if so, that is probably as much due to the nature of the material as to the painstaking efforts to demonstrate objectively. The net result is a reference work that no library that deals with international affairs can afford to be without. Probably nowhere in so short a space can one find as instructive a demonstration of the way in which the foreign policy of a country evolves. Canada's relatively limited interests make it possible to provide this in a form that can be

easily absorbed without being distracted by the multitude of events and problems.

RICHARD A. PRESTON  
Duke University

#### LATIN AMERICA

ANN PESCATELLO, editor. *Female and Male in Latin America: Essays*. [Pittsburgh:] University of Pittsburgh Press. 1973. Pp. xx, 342. \$9.95.

A serious collection of essays by talented women scholars and a token male, this is definitely not a superficial work hastily put together to capitalize on a fashionable theme. The nucleus of the book is a series of papers presented at the Third Biennial Meeting of the Latin American Studies Association at the University of Texas at Austin in December 1971. In *Female and Male in Latin America* the authors strive to capture the Latin American female—in image and reality—in past, present, and future. Their task was a formidable one in which they were destined to fail, but in failing, they make a significant contribution. Utilizing literature, history, and social sciences they give us the "first-ever collection of essays" devoted to the female in Latin America. Using an interdisciplinary approach they analyze various cultures, including socialist Cuba, traditional Columbia, modernizing Chile, and conservative Peru. They deal with such subjects as economic-sexual and sociosexual equality, behavior, attitudes, and class conflict. They describe various female roles from different perspectives: women as literary archetypes in novels, as sex objects, as frustrated politicians, and as human beings. Approaches vary from Nora Scott Kinzer's excellent study, "Women Professionals in Buenos Aires," which gives the reader insight into the personal lives of individual women, to chart-filled essays in which the woman is presented as an exploited statistic. The final two essays attempt to give insight into future prospects for Latin American women by looking primarily at the Cuban example.

Reading *Female and Male in Latin America* many historians may be bored at times by repetitive discussions of the stereotyped role of Latin American females and males and of what it means to have *cojones*. They will lament that

there is no historical study developed to the outstanding contributions of Latin American women. Some may charge that there is feminist bias citing such examples as a writer's use of the terms "traditional" and "modern" with the latter being synonymous with the ideals of the current feminist movement. These objections are minor compared to the assets of this work. This collection provides much new information and a fresh approach to the study of women in Latin America. The authors are careful to document their findings and they readily admit the limitations of their studies as well as their inability to make broad, definitive generalizations for all of Latin America. One of the most welcomed contributions of the book is the extensive bibliography on the female in Latin America by editor Ann Pescatello.

JOEDD PRICE  
University of Delaware

THOMAS O. OTT. *The Haitian Revolution, 1789-1804*. [Knoxville:] University of Tennessee Press. 1973. Pp. x, 232. \$8.50.

Professor Ott justifies his new history of the Haitian revolution by claiming for it two unique characteristics. He shows that he has explored and used hitherto ignored materials, such as the Rochambeau Collection at the University of Florida Libraries and the large corpus of United States newspaper accounts of the era. Although some materials mentioned are not as unknown as the author supposes, nevertheless, their extensive use by this author is new and valuable to other scholars. Ott's other purposes, the writing of a plain, factual, narrative account, is at once the advantage and the main fault of the book. He hopes to avoid what he feels to be the extremism of a racist such as T. Lothrop Stoddard or a Marxist such as C. L. R. James, and both are attacked on their facts and interpretations throughout the book.

The result of this middle road is a plain, somewhat old-fashioned, narrative account. The story is dominated by events, battles, and personalities. Trends and synthesis do not emerge clearly. Much of the presentation is biographical, presenting the deeds and public utterances of such figures as Léger Félicité Sonthonax, General Charles Leclerc, and, above all, Toussaint Louverture.

Louverture is the central figure and Ott's admiration for him is clear, and, following his intention of telling all without bias, Toussaint's failures, weaknesses, even his rare brutalities, are described.

Ott has no axes to grind, except that having no axe to grind is almost an ideology in his case. The great advantage of the book, then, is that it seems to be factually trustworthy once the revolution begins. Scholars of all methodologies and ideologies can rely on its presentation of events.

The failure of the book lies in its lack of interpretation or even analysis. The introductory background is elementary and drawn from the most general sources. The geographical and historical descriptions of Saint Domingue before the outbreak of the revolts are simplistic (and sometimes even inaccurate—"There are three major . . . administrative divisions of Haiti."). People and events are introduced summarily and with only minimal attempts to place them in their context. The author's determination to give a plain, accurate account has, in other words, led to a lack of ideas. There is nothing of theoretical interest, no new interpretations of part or all of the revolution to stimulate, annoy, or inspire the reader.

With these strengths and weaknesses Ott's work is likely to become a useful source book, often dipped into in almanac fashion by scholars interested in the Haitian revolution. It will not create debate or excitement. There is little here that will open up new avenues of research or lead to new interpretations.

MURDO J. MACLEOD  
University of Pittsburgh

JESÚS DE GALÍNDEZ. *The Era of Trujillo: Dominican Dictator*. Edited by RUSSELL H. FITZGIBBON. Tucson: University of Arizona Press. 1973. Pp. xxvii, 298. Cloth \$8.95, paper \$4.50.

Historians and political scientists can only be grateful to Russell H. Fitzgibbon and the University of Arizona Press for making available this version of the doctoral dissertation that the ill-fated Jesús de Galíndez successfully defended at Columbia University on February 27, 1956. Thirteen days later, it will be recalled, Galíndez disappeared without a trace—presumably abducted and killed on orders of



Trujillo, whose infamous tyranny the study had so fully described and carefully documented.

Professor Fitzgibbon has reduced the 689-page dissertation to a 298-page book (including his own epilogue and supplementary bibliography) by eliminating repetitious material, omitting detailed treatments of minor points, and otherwise effecting the editorial improvements that Galíndez himself would doubtless have undertaken if he had had an opportunity to revise the manuscript for publication. Unlike the much abbreviated Spanish and French editions the Fitzgibbon edition contains Galíndez's extensive documentation. "In every chapter, section, page, and paragraph of this study," as Galíndez says in his bibliography, "the source from which the data were obtained is mentioned, except in cases which are sufficiently obvious as to require no mention" (p. 276). In at least this respect *The Era of Trujillo* is superior to Robert D. Crassweller's fascinating and much more detailed *Trujillo: The Life and Times of a Caribbean Dictator* (1966), which contains no citations whatever.

The first chapter is a 73-page running account of events in the Dominican Republic during the first twenty-five years of "the Era of Trujillo" (1930-55). The remaining ten chapters consist of a sometimes repetitious analysis of those events under such topical headings as the constitution, elections and resignations, the party system and labor unions, social institutions, and Trujillo's personal style. In these chapters Galíndez exposes in detail the elaborately maintained but thoroughly perverted constitutionalism of the regime, the façade of regularly held but uncontested elections, the total suppression of individual rights, the Dominican party as one of Trujillo's many tools for imposing absolute personal control over the polity, the unscrupulous use of humiliation even more than terror to reduce the political elite to sycophancy and servility, and finally the megalomania, cruelty, and rapacity of the "Benefactor" himself.

Despite its devastating evidence the tone of the work is sober and dispassionate. Writing at a time when Trujillo's power was at its apogee Galíndez must have known the risk he was running; his section on "Murders Abroad" (pp. 134-37) attributes to Trujillo's henchmen the murder of three critics of the regime in

foreign countries. His fate is a testimony, not only to his courage but also to his commitment to the highest ideals of scholarship.

HENRY WELLS

University of Pennsylvania

SELWYN D. RYAN. *Race and Nationalism in Trinidad and Tobago: A Study of Decolonization in a Multiracial Society*. [Toronto:] University of Toronto Press, 1972. Pp. xiv, 509. \$15.00.

I first encountered this book in dissertation form while meandering my way through the Trinidad Central Library in 1970. About to begin anthropological fieldwork, I was dissatisfied with most of the historical accounts I had seen and was hoping to find something refreshing in Port-of-Spain's rather humble library. I found Ryan's thesis and for two days read it, becoming more and more delighted by its clarity, completeness, and insight. This book is largely a polished version of that dissertation, expanded to cover a description of the interesting (sometimes extraordinary) events that have occurred in Trinidad during the past few years. Ryan's book is certainly the best history of modern Trinidad available and an important addition to Caribbean historical literature. It is excellent political history, detailing the intriguing and dramatic arenas of political activity that have given modern Trinidadian history its unique and peculiar style and substance. It is superb sociology, illuminating the intricate links between ethnicity, class stratification and political action that are so characteristic of the Caribbean region. Ryan, himself a Trinidadian, displays throughout the book an acute sense for the minutiae of the Trinidadian situation, elucidating that situation through his careful description of the complex texture of political events. He is particularly informative on the central importance of racial and ethnic divisions in the island's history and the ways in which these divisions have been defined by politically interested parties so as to become features of political strategy. He succeeds in clarifying the ways in which ethnicity and class stratification have served as models for each other and describes how issues of race and class have been used by politicians to exacerbate situations or to obscure the definition of events and to control

the coalitions and political strategies that flow from these definitions.

Much of the book is a description of electoral politicals, and a careful analysis of postindependence national elections takes up much of the text. But Ryan does not simply provide electoral data and attempt to order it—as is so often the case with the interpretation of electoral material—through a hackneyed quantitative analysis. Instead, he discusses the political organization of elections in an imaginative manner, penetrating the complex dimensionality of elections and viewing them as scenarios revealing a variety of not simply political ideas, but more broadly sociocultural themes.

As an anthropologist I am especially pleased by this book, as Ryan throughout seems to display an acute ethnographic sensibility. This is not merely a diluted account of historical events, but an attempt to illuminate the very intricate social and cultural texture of a complex society. *Race and Nationalism in Trinidad and Tobago* will of course be of great interest to historians of the Caribbean region and to any others intrigued by the politics of ethnicity.

MICHAEL LIEBER  
Wellesley College

BENJAMIN KEEN. *The Aztec Image in Western Thought*. New Brunswick: Rutgers University Press. 1971. Pp. xviii, 667. \$20.00.

The discovery of the sophisticated Aztec civilization of central Mexico baffled Europeans of the middle sixteenth century and provoked controversies that were to persist for hundreds of years. This impressive volume by Benjamin Keen traces the development of Western attitudes toward the Aztecs and documents the struggle to understand and interpret this fascinating native American culture. The author begins his study with a concise summary of pre-Conquest Aztec society and a brief discussion of the events that transpired during Cortes's subjugation of the Aztec Empire. Most of the remainder of the book is a detailed examination of the literary disputes that raged in the centuries following the Conquest over the nature of Aztec culture. Was Aztec life typified by a barbaric religion, cannibalism, drunkenness, and sodomy, as vehemently as-

serted by Oviedo, Sepulveda, Gomara, and other sixteenth-century writers? Or, were the *Mexica* a rather enlightened people, with a developing civilization that, according to Las Casas, was superior to that of ancient Greece and Rome? There were more cautious and moderate appraisals of the Aztecs by Motolinia and Sahagun, whose writings epitomized the activities of the Franciscan ethnographic school and who used native informants as sources for their histories. Of course, much of the early controversy in sixteenth- and seventeenth-century Spain was related to the status of the conquered Indian peoples. What were their rights, were they truly "human," were they inferior to Europeans, was it proper to force them into vassalage under the harsh *encomienda* system? The Church was a central figure in these disputes, and while most clerical scholars denounced the inhumane treatment of the Indians, they could not agree on a remedy to the situation. While both Catholics and Protestants condemned, in large part, the Spanish behavior toward the Indians, they shared the belief of Thevet that "sodomy, idolatry, and other enormous impieties were the fashion in those regions before the Spaniards set foot there" (p. 156).

During the seventeenth century an Italian nobleman named Boturini made valuable contributions to the study of the Aztecs. He had traveled widely in Mexico and had amassed a collection of over five hundred Mexican documents. Keen states that Boturini's work "represents the first effort to construct a developmental sequence for the history of ancient Mexico, a history viewed as a succession of stages, with movement from one stage to the next caused by internal changes and struggles" (pp. 236-37).

A recurrent theme noted by Keen in the early literature is the concern over the origins of the Aztecs and other American aborigines. Were they Chaldeans, Babylonians, or Phoenicians? Had the Egyptians a role in the development of Toltec culture, or had Saint Thomas or the Knights Templar a hand in structuring Aztec life? Those interested in this subject are also referred to Lee Huddleston's *Origins of the American Indians: European Concepts, 1492-1729* (1967).

With the culmination of the Mexican struggle for independence in 1821, the Aztecs assumed

a new importance, that of helping to build a nationalistic spirit. Archeology played a major role in this effort, and in 1822 a museum of antiquities was opened in Mexico City. It was at this same time that the first warnings about the export of Mexican antiquities to foreign countries were voiced by Bastamante. He contended that "gold was more powerful than laws or love of country" (p. 321), yet antiquities continued to leave the country, a situation that persists relatively unabated up to the present day, despite stringent antiquities statutes (see "Ripping Off the Past," by S. Williams, *Saturday Review of the Sciences*, 55 [1972] 44-53).

The nineteenth century saw yet another fierce conflict about Aztec culture. A new romanticism had surfaced, raising Aztec society to glorious heights. This revival of pro-Aztec literature had been stimulated largely by archeological discoveries in Meso-America. An intense reaction soon developed, led by Morgan and Bandelier, seeking not only to halt the romanticist trend, but to also downgrade the level of Aztec achievement. Morgan, for example, insisted that Aztec culture had been roughly equivalent to that of the Iroquois peoples of the Northeastern United States.

The final chapters of Keen's book survey the increasing interest of nineteenth- and twentieth-century Mexicans in the Aztec past. Political leanings led to differing interpretations of Aztec life and their importance to modern Mexico. The opposing views of *indigenistas* and *hispanistas* are expressed in novels, plays, poems, and art (as in the works of Orozco, Siquieros, and Rivera). Siquieros believed that Aztec culture had no contemporary relevance, unless it was the lesson learned from the futile, but heroic, Aztec resistance to foreign domination.

Keen's book is notable for its thorough documentation, a trait that at times makes for laborious reading. It is, however, a very important contribution and is most highly recommended for historians and anthropologists concerned with the Meso-American region.

THOMAS R. HESTER  
University of Texas,  
San Antonio

HERNÁN CORTÉS. *Letters from Mexico*. Translated and edited by A. R. PAGDEN. With an introduction by J. H. ELLIOTT. (Orion Press Book.)

New York: Grossman Publishers. 1971. Pp. lxvii, 565. \$15.00.

G. MICHEAL RILEY. *Fernando Cortés and the Marquesado in Morelos, 1522-1547: A Case Study in the Socioeconomic Development of Sixteenth-Century Mexico*. Albuquerque: University of New Mexico Press. 1973. Pp. 168. \$10.00.

This pair of volumes, one a new translation of the *cartas de relación* of Hernando Cortés and the other a study of the Cortés estate, located in what is today the Mexican state of Morelos, comes at a time of renewed interest in and speculation about the conquest period of Mexican history. While neither provides any earthshaking new revelations both are welcome additions to the literature in the field.

The Pagden translation of the Cortés letters is a particularly good one. Not only is the translation more accurate, complete, and readable than the outdated J. Bayard Morris one, but it is far more richly annotated, and it provides a very useful survey of the biographical and bibliographical controversies concerning Cortés and the letters that have arisen over the past several decades. For instance, Pagden concludes in his introduction that the missing "first letter" of Cortés, cited by Gómara and Sigüenza, was probably a personal letter rather than a *carta de relación*—a personal plea to Charles V rather than a historical account. He also makes some interesting comments on Cortés's education, concluding that he probably had no formal training in the law—in fact, that he was poorly educated in any formal sense.

The second volume, *Fernando Cortés and the Marquesado in Morelos, 1522-1547*, by G. Micheal Riley, is more difficult to assess. It is slim—less than seventy-five pages of text, excluding the introduction and summary—and about half of this previously appeared in three articles published in *The Americas* over the past six years. What it amounts to is a very brief summary of the formation and operation of one part of the Cortés estate in New Spain, "the richest part," according to Riley, although he provides no documentation to support the statement. In one place he estimates, again without documentation or further explanation, that the Morelos portion of the estate produced between 50 and 53 per cent of its total revenues after 1531. Ignored in this study, then, are

seventeen encomienda towns, containing about one-half of Cortés's tribute-paying vassals, his mining interests, his Vera Cruz sugar estates, his urban real estate interests, and his commercial and shipping enterprises in Mexico, Vera Cruz, Acapulco, and Tehuantepec.

The reader is left with mixed feelings about Riley's book. At times stimulated by what is presented he is more often frustrated by what is skirted or left unsaid. One example of this is his commentary on textile tributes, by far the most significant revenue source of the Morelos portion of the marquesado. In the space of less than three pages devoted to tribute cottons Riley touches on some very interesting and complex matters: *tasación* rates and the changes they underwent; the value of goods received as tribute, their fluctuation, and the reasons for such fluctuations; and the partial conversion of in-kind payments to monetary ones. One would like to learn a great deal more about all of these things, at least as they relate to the Cortés estate, but also in the broader context of the Mexican economy as a whole. For instance, in connection with monetary payments in lieu of the actual cotton goods, Riley states that "many surviving encomienda Indians sought and were able to earn their wages and pay their tribute assessments in money rather than in textiles and other goods. Some even began to buy the clothing they needed." This would indicate a rather rapid transformation of Morelos into a monetary and wage economy, and it raises several questions. In what occupations and where did Indians earn wages in Morelos? With only three Spanish landholders in the area, and with the Cortés estate dominating the others with more than two-thirds of the total (1,402 acres out of a total 2,085), the opportunities had to be limited. Did Morelos Indians (and therefore Cortés tributaries) find temporary work outside of Morelos in sufficient numbers and with sufficient remuneration to pay a significant portion of the annual \$27,640 *pesos de oro de minas* in textile tributes in the 1541-44 period? These are some interesting questions, and I think they deserve at least a few comments if not an attempt at some answers.

LESLIE J. ROYAL  
California State University,  
Sacramento

ROBERT E. QUIRK. *The Mexican Revolution and the Catholic Church, 1910-1929*. Bloomington: Indiana University Press. 1973. Pp. 276. \$10.00.

In a scholarly and readable style the author examines the causes of the conflicts between the Church and the state from 1910 to 1929. Quirk proves that the Cristero rebellion of 1926-29 was inevitable since both the Church and the state wanted absolute dominion over the other; even when their goals were the same neither was willing to subordinate itself to the other.

Unfortunately the book contains flaws, none grievous enough to destroy the solid base of his excellent work. Contrary to his assertion (p. 7), Church lands *did* revert to secular ownership, as Taylor's work on colonial Oaxaca has shown. The attempts by the Spanish government at the end of the colonial period to usurp Church wealth and the role of the clergy in the independence movement are not touched upon even though those actions greatly influenced future relations between the two entities. Díaz's government was not "free from revolt, free from internal dissension" (p. 15), even after 1884, as Cosío Villegas has shown. Why the Church "opposed Madero as a matter of course" (p. 25) is never made clear. How many priests (at least approximately) were murdered by Villa's men (p. 54)? The reader cannot judge the importance of the state laws limiting the number of clerics unless he knows how many priests there actually were (pp. 149, 152, 155). Should not the assertion that "the vaunted wealth of the Church had simply disappeared, swallowed up in the nineteenth-century confiscations under the Reform Laws" (p. 121) be modified to point out the recuperation of some of that wealth through settlements with the owners? It was Calles and not Obregón (p. 112) who issued the Plan de Agua Prieta. Why Valverde Téllez's biography of Jesuit Alfredo Méndez Medina is cited to support a paragraph on the two political currents of the Revolution (p. 23) is inexplicable unless it was due to sloppy editing as in the case of missing page citations in three of the items in footnote 4, page 264.

In other cases one wishes assertions were based on direct primary evidence. How true is it that the attempt to consecrate Mexico to the Sacred Heart of Jesus was "the beginning

of a movement . . . to supplant the Indian Christ at Chalma and the dark Virgin of Guadalupe with the more theologically respectable and thoroughly Europeanized Sacred Heart of Jesus" (p. 131)? The *New York Times* is certainly not the best source for proving that "the bishops were concerned by reports from priests throughout Mexico that the Catholics were not as staunch as the clergy had expected or hoped" (p. 183). Perhaps British and French diplomatic sources are adequate, but direct Vatican sources would strengthen the statement that "It is clear that by the end of 1924 the Vatican was far from pleased with the state of the Mexican Church, especially with the aristocratic and reactionary social attitudes of prelates such as Guadalajara's Orozco y Jiménez, and that reforms would be effected by Rome as soon as possible" (p. 137). What basis does Quirk have for asserting that after 1929 "the Vatican was determined to change the complexion of the Mexican Church, to bring able and ambitious priests from the non-elite [mestizo and even Indian] classes into places of leadership" (p. 245)? The sources he cites for the paragraph certainly do not support the statement.

Quirk's work would have been strengthened had the archives of the National League for the Defense of Religious Liberty been available to him and had he used Vicente Camberos Vizcaino's two-volume biography of Orozco y Jiménez. The former, fortunately, was utilized in Alicia Olivera Sedano's *Aspectos del conflicto religioso*, which Quirk mentions in his brief bibliographic essay. Olivera Sedano deals with the class origins of the participants, which Quirk disregards, and she plots the geographical extent of the rebellion in great detail. Any future edition of Nicolás Larín's Marxist analysis, *La rebelión de los cristeros* (the first one was written before Olivera's work was available) will have to take into consideration that many of the participants who actually took up arms were from the rural element, and that they joined the Cristeros out of a desire to install a government favorable to agrarian reform.

The definitive history of Church-state relations in Mexico from 1910 to 1929 will not be written until the archives of most of the participants are available. In the meantime

Quirk's detached analysis of the relations of the federal and, peripherally, a few of the state authorities with the ecclesiastical hierarchy will remain the best work in English on the subject.

JOSÉ ROBERTO JUÁREZ  
University of California,  
Davis

WILLIAM H. BEEZLEY. *Insurgent Governor: Abraham González and the Mexican Revolution in Chihuahua*. Lincoln: University of Nebraska Press. 1973. Pp. xiv, 195. \$7.50.

The full title describes the contents of this book. Though Abraham González stemmed from a well-connected family he did not fit in during his mature years with the increasingly powerful Terrazas-Creel clique incarnating Porfiriismo. A Madero lieutenant from 1909 to 1911, he served as governor of Chihuahua, 1911-13, very briefly as minister of Gobernación, 1911-12, and died sadly at the hands of Huerista officers shortly after Madero's assassination.

Several problems confronted the author. First, does a Maderista leader, important only at the state-level, deserve a full-length study? On the whole results bear out the publisher's faith in author and subject. Second, a two-pronged methodological hurdle faced Beezley. Unpublished documents were scarce and scattered. The writer merits commendation for thoroughly exploring all possible data in both the United States and Mexico. The absence of any González private papers compounded his difficulty, making a full-scale biography virtually impossible. Consequently, the author was circumscribed in presenting González's reasons and motivations. The danger of too close an overlap with the work of the Chihuahuense historian, Francisco R. Almeda, constituted the other dimension of the methodological problem. Beezley is forthright in paying tribute to his Mexican counterpart, both formally and by reference to the other's work, yet his study is as independent of Almeda as he can make it. By providing added breadth and balance Beezley's monograph stands on its own merit.

With Chihuahua a focal point during the Revolution, and with regional history in vogue, the undertaking needed to turn into a significant contribution to sectional history. Here

lies its signal success. The inner workings of Chihuahuense politics during the Madero period are not readily discernible. In his concentration on González's role Beezley considerably clarifies the muddled waters of 1910-13.

A final challenge lay in fitting the Chihuahua tableau into the larger Madero scenario. In this endeavor the author seems least successful, which is regrettable, because troubles in Chihuahua contributed significantly to Madero's downfall.

A satisfactory index and bibliography, several illustrations, and a map of the state in 1910, with the railroads drawn in, are provided.

LOWELL L. BLAISDELL  
Texas Tech University

MURDO J. MACLEOD. *Spanish Central America: A Socioeconomic History, 1520-1720*. Berkeley: University of California Press. 1973. Pp. xvi, 554. \$20.00.

Any effort to periodize the erratic course of colonial Latin American history may well be applauded. The author periodizes Central American history to 1720 by boom and bust cycles that run the gamut from conquerors, gold, and Indian slavery to about 1550; cacao plantations based on forced labor for the next fifty years; and indigo plantations during the seventeenth century. The latter enterprise was accompanied by a growing depression because of the inability of Spain to supply adequate markets or transportation. A moderate recovery marked the thirty years prior to 1720, which the author ascribes with probable correctness to contraband trade. The cycles of prosperity varied by regions, resulting in a more complex pattern than can be presented here.

During these booms the Indian population sharply declined owing to epidemics, removal to different climates, and exportation as slaves. Central America may thus be likened to Europe, the author suggests, when population scarcity following the Black Death altered the economic and social structure. Though this comparison proved illuminating it is arguable whether the shift from mining to various forms of agriculture was dictated by labor scarcity or by depleted natural resources such as exhausted placers. Excellent statistical charts document

the rise and fall of commodity production and of the Indian population.

No doubt, as the author states, the Indians suffered from intensive labor demands during boom periods. However, he seems uncertain whether labor pressure was relaxed during the seventeenth-century depression. He states that Indians were better treated (p. 118), but speculates (p. 301) that they could be exploited on haciendas unobserved by the authorities. Indeed, the author seems to champion the currently popular "lower-class backlash" in Latin American historiography. Where well documented, no objection need be offered, but repeated generalizations concerning the callousness, covetousness, and dishonesty of Spaniards, juxtaposed against the statements (pp. 191, 225) that the castes rustle, steal, and cheat employers because of alienation and disaffection does sound like a double standard of judgment. The work would have profited from sharper editing, for it contains treatises on the Black Death, the laying out of towns, how to raise crops, how to kill cattle, etc., all of which obscure by an uncritical thoroughness the otherwise relevant themes.

Nonetheless, the writer has presented the main lineaments of Central American economic history to 1720 so well and so thoroughly that his work is unlikely to be surpassed for many years. The social structure and human relations need more research and less assumptions.

TROY S. FLOYD  
University of New Mexico

NORMAN ASHCRAFT. *Colonialism and Underdevelopment: Processes of Political Economic Change in British Honduras*. (Publications of the Center for Education in Latin America, Institute of International Studies.) New York: Teachers College Press, Teachers College, Columbia University. 1973. Pp. ix, 180. Cloth \$8.50, paper \$3.95.

NARDA DOBSON. *A History of Belize*. [London:] Longman Caribbean; distrib. by Longman, New York. 1973. Pp. xiv, 362. £3.50.

British Honduras, or Belize, illustrates the Third World complaint. Itself a creation of empire, its capital surplus systematically transferred to a distant metropolis throughout most of its history, the colony now seeks viability in a postcolonial environment. Founded for the

exploitation of its forests and as a strategic base in the Central American trade, it was effectively deforested by the beginning of the twentieth century and deprived of all commercial leverage. It is, in short, underdeveloped. The two works under consideration, by authors who have done substantial field work in British Honduras, represent opposite approaches to understanding this condition and process.

Dr. Ashcraft examines the culture of poverty, seeking its internal and external context. Anthropologist, good observer, and disciple of André Gunder Frank, he makes a unique contribution of detailed social information. He labors to explain how the present situation came about and what it means. Unfortunately the book is not as effective as its substance might indicate. The interior chapters contain much interesting and important material, both contemporary and historical, which will remain largely inaccessible. Specifically the introductory chapter, "The Meaning of Underdevelopment," contains a number of good insights but is very difficult for the reader and creates the impression that the author is trying to explain the material to himself by trying on several conceptual hats. More damaging in a work of this sort is the inexcusable absence of an index. Sad to say, not even a detailed table of contents was provided. Among puzzling bibliographical choices it is a mystery how the author could fail even to list N. S. Carey Jones, *Pattern of a Dependent Economy: The National Income of British Honduras* (1953).

Narda Leon Dobson's work spans the entire history of the colony. Intended to be suitable for a textbook, it is more. The author contributed previously to the social and administrative history of British Honduras in her Oxford thesis. She builds upon that expertise and upon her period of residence in Belize by surveying the colony's entire historiography, and she has produced the best effort yet toward a general history of the colony. The scholarly apparatus is truly helpful and ample, as is a section of photographs. Writing from a viewpoint acceptable both in London and in Belize the author strives for balance in interpretation. In the stylistic sense this work is clearly a compromise. It is designed to be read either by a citizen of Belize, a foreign tourist, or a scholar.

The compromise succeeds, and *A History of Belize* is dignified and vital.

WAYNE M. CLEGERN

Colorado State University

WILLIAM PAUL MCGREEVEY. *An Economic History of Colombia, 1845-1930*. (Cambridge Latin American Studies, number 9.) New York: Cambridge University Press. 1971. Pp. xiv, 330. \$10.95.

William McGreevey has combined the methodology of the new economic history with traditional treatment to produce a major work in Latin American history. Setting his scene with a review of the institutional and policy framework within which Colombia emerged from the Spanish Empire into independent status, McGreevey continues his narrative into what he argues persuasively was a period of economic decline, from 1850 to 1885. The nature of his quantitative material and approach precludes him, he tells us, from continuing "the kind of exciting narrative which the reader has enjoyed up to this point" (p. 100). The graphs, charts, tables, and analyses that follow are, in fact, only slightly less exciting than McGreevey's earlier narrative, but they are decidedly less enjoyable. Nevertheless, for the serious student of Latin American economic and social history they offer not only great insight into the Colombian economic experience, but also a variety of models and hypotheses to test in other parts of the region. In his final section McGreevey explains and analyzes the period from 1890 to 1930 as one of economic and social growth, brought on principally by the transition to coffee cultivation, especially in the Antioquia region.

As with most attempts at synthesis in Latin American history the paucity of monographic research for much of the area with which he deals has restricted McGreevey. Thus he has been forced to base much of his work on a relatively small number of secondary and theoretical works whose models are not uniformly applicable. McGreevey has supplemented these with a considerable amount of his own research into primary sources, but it is small wonder that in places his evidence appears flimsy and his assumptions too large. He frequently makes comparative analysis, particu-

larly with Mexico and Brazil. Unfortunately, he almost entirely avoids comparison with areas having a more similar experience of development and dependence. Central America, for example, where the liberals' efforts on behalf of economic growth, development of coffee cultivation, and foreign investment, has almost entirely escaped his notice. McGreevey appears unaware of the isthmus's similarities to the pattern he describes for Colombia and gives no indication that he is aware of the growing data—much of it as yet in unpublished doctoral dissertations—that exists on nineteenth- and early twentieth-century Central American economic development.

Such criticism, however, is not intended to obscure the truly laudable aims and contributions that this book makes. No future work on Colombian economic history—can ignore McGreevey. He has opened the way for a multitude of studies into many facets of Colombian development as well as stimulated honest controversy among Latin Americanists regarding his methodology and conclusions. The results can only be salubrious and stimulating.

RALPH LEE WOODWARD, JR.  
Tulane University

JAMES LOCKHART. *The Men of Cajamarca: A Social and Biographical Study of the First Conquerors of Peru*. (Latin American Monographs, number 27. Institute of Latin American Studies, University of Texas at Austin.) Austin: University of Texas Press, for the Institute. 1972. Pp. xvi, 496. \$10.00.

Over a period of some years James Lockhart has skillfully honed his talent for social history. The results are evident in the insights and precision of his *Men of Cajamarca*, a group portrait of the 168 Spaniards who captured the Inca emperor, Atahualpa, at Cajamarca in 1532. Lockhart has an affinity for plumbing the secrets of early colonial Spanish American society through the ferreting out of isolated details from a multitude of archival and published sources. To his advantage is the fact that the men of Cajamarca constitute a larger sample for one category than those used for the group portraits contained in his earlier work, *Spanish Peru, 1532-1560: A Colonial Society* (1968).

*Men of Cajamarca* is as much historiography

as social history, not only in its review and evaluation of the sources available concerning the topic, but also in its methodological approach. The author suggests two ways of exploring topics such as his: the studying of the daily activities of individuals at several levels of society through their officially recorded actions and the subsequent reconstitution of the main societal types and processes, or the intensive examination of individuals composing a group whose role in society serves to substantially reveal the nature of that entity.

Hence this book begins with a number of analytical chapters detailing general patterns—social, collective, or organizational—and concludes with short biographies of the conquistadors, divided according to type or function. The initial analysis is highlighted by over two dozen tables examining such variables as experience in the Indies, regional origins, social rank, rate of return to Spain, residence patterns of repatriates, and Peruvian municipal offices held. One of the author's conclusions is that no truly essential elements of Spanish culture were lost in the New World, a reassertion of one of the main findings of his earlier work. He also argues that there was an impulse toward standardization of society in America as regional groups from the peninsula clashed and as early arrivals socialized later ones. Lockhart also holds that the characteristics of the areas settled and traditional social ideals brought from Spain were more influential in determining what occurred in America than ideologies, such as utopianism, and individual and group psychology. The author emphasizes the *ad hoc* nature of conquest and colonization, the prevailing self-interest of the conquistadors, and their acceptance of existing society both in Spain and the Indies. Lockhart contrasts the temperaments of the conquerors with those of their creole successors, characterizing the latter as idlers and talkers as a consequence of their experience having been akin to sons who have had everything done for them by their fathers. For one normally so judicious it is surprising that Lockhart lapses into a conclusion that is not only unsupported by the available psychological data concerning the phenomenon, but is also a stereotype. This latter, however, is a tendency he displays in his descriptions of regional characteristics. Nevertheless, the book



is skillfully researched, felicitously written, and stimulating in terms of the new research areas it suggests.

MARGARET E. CRAHAN  
Herbert H. Lehman College,  
City University of New York

PETER F. KLARÉN. *Modernization, Dislocation, and Aprismo: Origins of the Peruvian Aprista Party, 1870-1932*. (Latin American Monographs, number 32. Institute of Latin American Studies, University of Texas at Austin.) Austin: University of Texas Press, for the Institute. 1973. Pp. xxiii, 189. \$8.50.

Between 1870 and 1930 the Peruvian north coast sugar industry underwent a complete transformation. Originally made up of small- and medium-sized haciendas, worked largely by imported Chinese labor, a process of progressive consolidation took place until the industry was totally dominated by gigantic foreign-owned corporate plantations, particularly Gildemeister and W. R. Grace. These plantations used indentured Indian labor, drove small truck farmers and local merchants out of business, legally and illegally monopolized precious water supplies, turned flourishing commercial and farming centers into ghost towns, and made and broke governments in Lima.

It is Peter Klarén's thesis that it was this process that turned Víctor Raúl Haya de la Torre from a dilettantish member of a circle of young Trujillo literati into the radical, antiimperialist founder of the *Alianza Popular Revolucionaria Americana* and the Peruvian *Aprista* party, which dominated that country's civilian politics for almost forty years.

Apparently Haya de la Torre does not agree with this thesis, and neither do I after reading Klarén's book. Klarén himself admits that Haya's political ideas were vague and diffuse until he spent some time as aide to the military prefect of Cuzco, where the plight of the highland Indians shocked him into becoming a revolutionist. An application of Occam's razor suggests a simpler explanation for Aprismo's relatively greater electoral strength in the north; Haya and his principal lieutenants were northerners, and in a highly regionalized country such as Peru, as elsewhere in Latin America, that counts.

Despite some question as to whether Klarén

has supported his thesis, however, his highly competent, readable, scholarly, and fascinating narrative of the destruction of a functioning rural/urban society by the introduction and application of the techniques and organization of extensive capitalist agriculture should be read by anyone interested in the social processes involved in the transformation of traditional societies.

NORMAN A. BAILEY  
Queens College,  
City University of New York

CHARLES D. CORBETT. *The Latin American Military as a Socio-Political Force: Case Studies of Bolivia and Argentina*. (Monographs in International Affairs.) [Coral Gables, Fla.:] Center for Advanced International Studies, University of Miami. 1972. Pp. xx, 143. Cloth \$4.95, paper \$3.95.

One should not expect to find in this monograph the kind of depth in historical background that scholars such as Professors Lyle McAlister, Robert Potash, or Robert Gilmore bring to their studies of politico-military relations. Nonetheless the author of this monograph has certain advantages over many other investigators of the roles of the military leaders in Latin America. Charles D. Corbett is both a Latin American specialist and a colonel in the United States Army. His military assignments, which have included postgraduate studies in Argentina during 1964-65 and service as director of instruction at the United States Army School of the Americas in Panama, have afforded him opportunities that few other North Americans have had to observe the military institutions of Latin America. As a result of his military training and experience Colonel Corbett is unusually well equipped to deal with the key question of the level of professionalism in the military establishments of Argentina and Bolivia. The results of his examination of each establishment are carefully and clearly presented. Furthermore he does not make the mistake of trying to draw extended comparisons between Argentina and Bolivia or with other Latin American military forces.

The author makes it clear that to measure the development of a military establishment and its role one must also analyze the other institutions of the society. Thus those who prefer to see the

pernicious influence of "the military" as the root cause of a country's socioeconomic and political problems will find no solace but possibly some enlightenment in Colonel Corbett's summing up of why the military has played so prominent a role in Argentina. "The weakness of other political institutions in Argentina is the single most important factor contributing to the military's strong role. Political parties, the labor movement, business groups, and the university community are all factionalized and historically have been unable to find overlapping areas of common interest" (p. 123).

Similarly, the author's evaluation of the role of the Bolivian military establishment, vis-à-vis other Bolivian institutions, bears careful consideration by anyone trying to understand *cosas de Bolivia*. "The sense of discipline that exists in the army—imperfect as it may be—stands in stark relief to the virtual anarchy that obtains in political parties and even among civil servants. In a word, no resource is in shorter supply in Bolivia than disciplined administrative talent, and the officer corps provides an immediately available pool" (p. 68).

In sum, this relatively brief monograph is an excellent contribution to the growing number of serious studies of the military establishments of Latin America.

JOSEPH R. BARAGER  
McLean, Virginia

VINCENTE BARRETTO. *A ideologia liberal no processo da independência do Brasil (1789-1824)*. (Câmara dos Deputados, Diretoria Legislativa.) Brasília: Centro de Documentação e Informação, Divisão de Publicações. 1973. Pp. 160.

In the composition of this prize-winning monograph on the political thought of Brazilian independence Professor Barretto utilized standard references and documentary collections with imagination and diligence. After defining his terms and sketching the liberal idea in Western Europe from the Middle Ages forward, he explains the introduction of scientific and economic aspects of the Enlightenment into Portugal by the marquis de Pombal, who ruled from 1750 to 1777. The dictator, however, would not permit the discussion of current political and philosophical thought, thus weakening the development of liberalism in the

Portuguese-speaking world. Barretto then evaluates the liberal spirit in such Brazilian happenings as the Guerra dos Mascates (1710) in Pernambuco, the Inconfidência of Minas Gerais (1789), the Bahian Conspiracy of 1798, the Pernambucan revolts of 1817 and 1823-24, and the independence movement itself (1821-22). The author finishes with a brief analysis of the Assembly's 1823 project and the Constitution of 1824. At all times he applies the standards of English and French liberalism and is impressed with the middle-class objectives of Brazilian revolutionaries.

In my opinion, however, he weakens his interpretation by the constant reference to a European model, which he acknowledges was not always germane to the Brazilian setting. Differences are explained away in almost simplistic fashion, in which he underscores the villainy of the Inquisition and the Jesuit order. The Jesuits, for example, destroyed the healthy humanism that emerged in early sixteenth-century Portugal. Also, Barretto ignores certain developments in nearby Spain: the contributions of the Jesuits Francisco Suárez and Juan de Mariana to the doctrine of popular sovereignty, as well as the ideas of Francisco de Vitoria, a Spanish Dominican, on international law. Thus, he fails to see the projection of Christian medieval contractual law into the modern era, a movement that flourished again in the Spain of Charles III (1759-88) and prepared the ground for the Cádiz Constitution of 1812. The Spanish model of liberalism would have been more applicable to the Brazilian scene, rather than overemphasizing the French example. Moreover, it was the negative example of the French Revolution that affected Hispanic liberalism, a point that Professor Barretto mentions, but does not stress. His interpretation, nevertheless, is still plausible until further research appears.

MARIO RODRÍGUEZ  
University of Southern California

ROBERT CONRAD. *The Destruction of Brazilian Slavery, 1850-1888*. Berkeley and Los Angeles: University of California Press. 1972. Pp. xvii, 344. Cloth \$14.50, paper \$4.25.

Just a decade ago students interested in an English language account of the abolition of

slavery in Brazil suffered from a paucity of materials. Now, thanks to several important new articles and books by North American, Brazilian, and British scholars, the Brazilian abolitionist movement appears in clearer historical perspective. Robert Conrad's new book represents an excellent contribution to this scholarly revolution in fact-finding and interpretation.

Professor Conrad is particularly strong in his criticism of the recently popular thesis that planters from São Paulo were progressive in their attitudes toward emancipation and became leaders in the struggle for abolition. Conrad shows that most Paulista slaveholders converted to the emancipation cause only in the last years of controversy, after holding the questionable distinction of remaining among the planters who were most reluctant to liberate their bondsmen. The planters' push for change developed especially out of the north, not the south, argues Conrad. When economic dislocations weakened the viability of slavery in the northeastern provinces, planters there sold bondsmen to the south, converted to free labor, and became less defensive about Brazil's peculiar institution. In the important legislative battles over slavery reform in 1870-71 and 1884-85 several representatives of the northern provinces pressed for change, while spokesmen for the coffee-rich southern provinces resisted emancipation. Other pressures, too, bore down on Brazil's slaveholding aristocracy during the years of growing controversy. Emancipation in the United States, followed by abolitionist gains in Cuba, left Brazil isolated as the last slaveholding country in the Western Hemisphere. Then abolitionism gathered momentum in the early 1880s, as urban citizens formed antislavery clubs and emancipationist members of parliament pushed for reforms. The coffee planters'

resistance to significant change finally broke in the 1886-88 period, when radical abolitionists and restive slaves brought chaos to the southern region.

Conrad's study touches on many important issues that need more attention in future research. For example, was the north-south distinction in attitudes toward emancipation as significant as Conrad argues, or was the division between urban and rural attitudes a more important demographic factor in the conflict of viewpoints? Did Emperor Dom Pedro II contribute as much to abolition as Conrad suggests, or did Pedro's cautious posture operate as an obstacle to change? Was Brazilian slavery already declining significantly by the 1880s due to economic unprofitability, or did the system still have several years of life left in it—years cut short by the pressures of abolitionism? How much did social and political factors motivate proprietors to continue defending slavery even where the institution was relatively unprofitable? In what ways did abolitionists differ among themselves about the speed of change, the potential for legislative reform, extra-legal tactics, and the role of Afro-Brazilians in post-abolition society? To what degree was racism a factor in the antislavery controversy, including the attitudes of abolitionists themselves? And, finally, how many slaves rebelled against their condition in the 1880s, independent of influence from the abolitionists? These and other important questions are presently being investigated by a new generation of scholars, individuals who should appreciate Robert Conrad's helpful survey of the principal developments in the abolition of slavery in Brazil.

ROBERT BRENT TOPLIN  
*Denison University*

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## Communications

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*A communication will be considered only if it relates to an article or review published in this journal; publication of such a communication or of any part of it is solely at the editors' discretion. Limitations of space require that a communication, whether concerning articles, review articles, or reviews, be no longer than 300 words. The schedule of publication and the time needed to send a communication to the author of the article or review in question for such reply as he may care to make virtually preclude the possibility of publication in the issue following that in which the original article or review appeared. Unless, in the editors' judgment, some major scholarly purpose is served, rejoinders will not be published.*

### TO THE EDITOR:

Gerard Braunthal, in his review of Klaus Schönhoven's *Die Bayerische Volkspartei, 1924-1932* (*AHR*, 79[1974]:175-76), writes that the Bavarian People's party was Catholic, anti-Prussian, and separatist. It is, on the whole, correct to describe the party as Catholic and anti-Prussian, but separatist it was not, and such a statement is not supported by Schönhoven's material. Most probably the BVP contained some crypto-separatists, but the party as such never went beyond a strong advocacy of states' rights (*Partikularismus*) within the framework of the Reich. At the utmost—and this only occasionally between 1920 and 1923—the idea of a temporary separation was countenanced by some party leaders in the event that, as Braunthal puts it, "anarchists and Bolsheviki" were to take over the North, but always with the implication that, as soon as possible, the *Ordnungszelle Bayern* should take measures to restore

order in the North and reunify Germany. Even if the leaders of the BVP had been separatists at heart, a separatist policy would have been impossible because it would have found support only in very limited rural areas of Southern Bavaria and would have been vigorously opposed in Munich and other cities and in Franconia and the Bavarian Palatinate; in other words, a separatist policy would have disrupted Bavaria itself. As Schönhoven and others have explained, the conduct of affairs by the leaders of the BVP was not exactly statesman-like, but they were intelligent enough to see that separation was impossible, even if they had considered it desirable, which hardly any of them did.

CARL LANDAUER  
*University of California,  
Berkeley*

### TO THE EDITOR:

I would like to take exception to Professor Woodford McClellan's hostile review of my *Russian Police Trade Unionism: Experiment or Provocation?* (*AHR*, 79[1974]: 539-40).

I had no ulterior motives in limiting the statistical tables in chapter 1 to the years prior to 1900, for they appear only to show the socioeconomic background of the Zubatov experiment and become irrelevant to the context of the book thereafter. The same is true of his other reproaches of omissions. A book should be judged on its contents, not on its omissions.

McClellan finds three or four factual mistakes of detail in my study, for which I apologize, but is there a single scholarly work by authorities much greater than myself that does not contain errors of fact? His claim of in-

sufficient bibliography in the book is even stranger in view of the fact that it contains over 10 pages of bibliography for 169 pages of main text.

Finally, I should reproach Mr. McClellan for misrepresenting two subjects in my book. One refers to the dedicated Jewish Social Democrat Mania Vil'bushevich, who turned a Zubatovist, then a Zionist, and later became one of the pioneer Israeli settlers. Surely McClellan knows that she was not just an ordinary "police informer," when he cites out of context a quotation from her letter to Zubatov: "an honest person can be a traitor only if he betrays others into the hands of another honest person." The quotation appears in the original text to show how the mind of a dedicated revolutionary worked, the mind of a person who accepted the principles of a relativist morality: whatever is good for the cause of my party is moral.

The other point is on the difference between anti-Semitism and anti-Judaism. Nowhere did I say that the tsarist government was "merely" anti-Judaic. It was anti-Judaic, not anti-Semitic, because a Jew received equal rights with all other citizens of the Empire once he changed his religion, which has not been the case in either anti-Semitic Nazi Germany or in the currently anti-Semitic Soviet Union. Boris Slutsky, a contemporary Soviet poet of Jewish background, wrote an unpublished poem about a Russian Orthodox priest in the Soviet Union. Just as before the Revolution, this converted Jew was, in the eyes of the local Russian bishop, now a Russian priest; but to the "commissar," for whom blood is what matters, he remained "a dirty Jew." This is the distinction between the anti-Judaic prerevolutionary regime and the anti-Semitic Soviet regime.

Ironically, a Soviet reviewer of my book proved to be freer from clichés than some Western colleagues. The review in the *Istoriia SSSR* (no. 5, 1972) gave full justice to the book, showing the forest rather than the trees, by presenting a comprehensive précis of its main theses. This indicates to me the timeliness of the book, which is the greatest gratification for an author.

DIMITRY POSPIELOVSKY  
University of Western Ontario

#### TO THE EDITOR:

In his review of several books about the Bolshevik Revolution (*AHR*, 79[1974]:546-49) Professor Robert C. Williams states that "émigré historians," while emphasizing the weakness of Bolshevik forces in October and the lack of broad popular support for Lenin's uprising, "fail to explain why" it succeeded.

The author of the book most immediately at issue, S. P. Melgunov, in his *Bolshevik Seizure of Power* explicitly avoids interpretation of what he calls "the dynamics of history" and focuses on a description of the crucial three weeks in October and November of 1917. However, in my editorial introduction, it seems, the reasons for the victory of the Bolsheviks are made sufficiently clear.

They lie precisely in the failure of their opponents, even if Professor Williams considers this reason insufficient. Conditions in the army and the country were chaotic, people were tired of the seemingly endless war and confused by domestic politics. The Provisional Government was so discredited that nobody saw it worth their while to defend it—save the few hundred military cadets and the 150 members of the Women's Volunteer Battallion.

The ease with which power—which no one held in fact—was seized was recognized several times by Lenin. His resolution, adopted by the Petrograd Soviet in the meeting of October 26, hailed "this unusually bloodless and unusually successful uprising" (*Lenin's Collected Works* [Moscow, 1964], 26:241). In his speech before representatives of the Petrograd garrison on October 29 he stated: "We took power almost without bloodshed" (*ibid.*, 270), which means without serious armed struggle. Thus, Lenin himself "debunks" the later Soviet legends of a bloody and heroic storm of the Winter Palace. In his report to the fourth All-Russian Congress of Soviets on March 14, 1918, he said that the Bolshevik struggle against the ruling classes "was not so much a military operation as agitation" (*Works*, 27:174).

This brings us to the second cause for victory, namely, that nobody saw a clear way out of the confusion of the Revolution; only Lenin had the plan—a dictatorship of the Bolshevik party under the guise of popular government by Soviets. Neither the advocates of strong govern-

ment on the right, nor advocates of a far-reaching democracy on the left could match that combination.

The power in Petrograd was seized with the army and the country as passive onlookers, helping neither the Provisional Government nor the Bolsheviks. The power spread to the provinces, albeit very slowly, as Melgunov shows, because of successful "agitation": emphatic promises to give "all power to the Soviets" of elected popular deputies, "land to the peasants," and "peace."

One should not underestimate this factor, which is easily forgotten: the power of demagoguery and grand-scale political deceit. It worked not only in the half-illiterate Russia of 1917, but also in more advanced countries, not limited to Germany and Italy.

SERGEI PUSHKAREV  
New Haven, Connecticut

*The following letters have been received in connection with the publication of C. Richard Arena's review of Warren Cook's Flood Tide of Empire: Spain and the Pacific Northwest, 1543-1819 in AHR, 79 (1974): 569-70.*

TO THE EDITOR:

C. Richard Arena's review of my book claims that I attempt "to show that Spain was not waging a losing defensive action from 1790 on." Quite the contrary, on page 533 and elsewhere I maintain that "no matter what loopholes Floridablanca thought he perceived in its terminology, the Nootka Convention of 1790 drove a fatal wedge into Spain's position in the Pacific Northwest."

"It is hard to accept his main point," Arena remarks, "that a serious effort was made by Spanish royal forces to save either the Pacific Northwest or Louisiana." Arena cannot have read pages 446-85, documenting persistent Iberian efforts to "apprehend" Lewis and Clark, which came perilously close to success. Nor chapters 8 and 9, showing the apogee of Spanish activities on the Northwest coast to have been 1790-92.

Arena berates an anthropological approach that examines Spanish efforts in competition with Indian, Russian, British, and American

life styles striving for hegemony over the Northwest coast. He censures a textual quote on cannibalism for not "possibly determining, as Cook claims, the outcome of the Nootka crisis." I made no such assertion, but considered relevant the impact of Spanish reactions upon native culture. Abraham Nasatir (*The History Teacher*, 7 [1973]: 144-45) deemed this but one virtue of "a work of exceptional character, . . . eye-opening to one who has spent a lifetime in research in foreign archives."

An unfair review in the *AHR* can blight the results of years of worthwhile effort. Arena dismisses what E. W. Giesecke (*Oregon Historical Quarterly*, 74 [1973]: 353-54) calls "a superb book, a major publication event," and W. Michael Mathes (*New Mexico Historical Review*, 49 [1974]: 79-81) "monumental . . . a fine contribution to the history of the Spanish borderlands [which] should be in the library of all persons interested in that field." For Murray Morgan (*Seattle Post Intelligencer*, Apr. 15, 1973), it "stands as a high water mark in Northwest historical writing."

Reviewing a book entails a commitment to read it, and Arena quite obviously did not fulfill his obligation. He should heed Theodore Treutlein's recommendation (*California Historical Quarterly*, 52 [1973]: 371-72): "This massive book should be required reading for those brought up on the inaccurate generalization that Spain's 'decline' set in during the days of Ferdinand and Isabella. Contrarily, Spain continued to develop, and the empire reached its greatest extent in the year 1789. . . . The volume's subtitle, 'Spain and the Pacific Northwest,' is too modest, since this impressive study considers in great detail the complicated international struggles of two continents which swirled about the coasts and waters from San Francisco Bay north to Alaska."

For Arena, the Spanish Empire of Jefferson's time was incapable of defending its holdings "against even a flotilla of Western Yankees in flatboats whenever it suited their fancy to float down the Mississippi." "This traditional perspective," he feels, "still seems quite healthy in spite of his [Cook's] concentrated assault."

Reluctant to weigh the evidence, Arena only prolongs an ethnocentric tunnel vision of competition for the American West that, ironically,

shortchanges his own ethnic ancestry's important role.

WARREN L. COOK  
Castleton State College

TO THE EDITOR:

Several rereadings of Professor C. Richard Arena's review have confirmed my dismay over the impression created that Cook attempts to unseat earlier scholars from their merited eminent positions. The fact is that this temperately written study repeatedly incorporates the findings of Cook's academic predecessors (e.g., p. 43 n. 2; p. 268 n. 46; p. 460 n. 72), presents new evidence (e.g., p. 116 n. 76; p. 286 n. 33; pp. 591-95), and notes needed modifications in earlier interpretations (e.g., p. 462 n. 77; p. 472 n. 96; p. 469 n. 82). He consistently cites others as authorities, not antagonists.

The difference between Cook's presentation and Professor Arena's reactions to the vivid description of the cannibalism of Ma-kwee-na, the Nootka chief (p. 190), is a case in point. Arena remarks that Cook claims this cannibalism as "possibly determining the outcome of the Nootka crisis." One might reach this conclusion from a general observation in the preface (p. ix) that "Spanish alternatives, choices, successes, and failures . . . often hinged upon cultural differences as much as on economic and political factors." Cook dispels any possible ambiguity by his later specific statement that "Don Juan's [Juan Francisco de la Bodega y Quadra] tolerance for Ma-kwee-na's customs drew the line at anthropophagy, but the patience, wisdom, and justice displayed in his treatment of the Indians contrasts notably with the behavior of his contemporaries and added measurably to Spain's hold over Nootka Sound." Cook ascribes no significant importance to Ma-kwee-na's dietary preferences at Nootka. He does put the determining factors of the entire incident in bold relief on page 533.

The reader of *Flood Tide of Empire* is struck by the meticulous research and matching style that mark both text and notes. The masterful use of sources and a comprehensive bibliography contribute to the unusual importance of this latest elucidation of Spanish activity in the Pacific Northwest.

W. N. BISCHOFF, S.J.  
Seattle University

TO THE EDITOR:

I appreciate Professor von Klemperer describing my book, *Hahnenschwanz und Hakenkreuz: Steirischer Heimatschutz und österreichischer Nationalsozialismus, 1918-1934*, as the most scholarly one of the four he reviewed (*AHR*, 79[1974]:179-81). Nevertheless, I must take exception to several of his statements.

Although it is true that the two chapters on the Austrian *Heimwehr* were based largely on secondary sources this was intentional, as I saw no need to retrace the path followed by Professor C. Earl Edmondson in his excellent monographic study of the *Heimwehr*. It was for this reason that the name *Heimwehr* was deliberately excluded from the subtitle of my book. The *Heimwehr* is brought into the narrative only because (as is explained on p. 47) the Styrian *Heimatschutz* was a member of the whole Austrian *Heimwehr* (albeit an autonomous one) during the late twenties and early thirties.

Professor von Klemperer's charge that I also used secondary materials for the *Heimatschutz* is nothing short of incredible. Except for the *Pfimer-Putsch*, where Josef Hofmann's book is in many respects definitive, my chapters on the *Heimatschutz* are based almost exclusively on unpublished and never before used documents found in Graz, Bonn, and Koblenz, as well as on Styrian newspapers. Equally misleading is the statement that I quote Ludwig Jedlicka on the Korneuburg Oath. What is quoted is simply Jedlicka's verbatim translation of the oath; the analysis that follows is entirely my own.

I fail to understand why Professor von Klemperer finds my use of Nolte's definition of fascism "reprehensible." The fact is that anti-Marxism was one of the few elements common to all Austrian fascists. In any event my use of Nolte's definition is by no means "uncritical" since I point out (p. 161) that the Styrian *Heimatschutz*, despite its ostentatious anti-Marxism, did not disband once the Austrian Social Democratic party was outlawed in 1934.

Even more perplexing is von Klemperer's assertion that I make an "ill-founded and irresponsible warning that economic crisis coupled with a 'new movement against the Parteienstaat and parliamentarianism' would be tantamount to a revival of fascism in Austria." Such an equation is neither stated nor implied anywhere in my book. As for my warning, it has

been well received by my Austrian reviewers and indeed Karl D. Bracher makes the identical point concerning Germany in *The German Dictatorship* (p. 487).

Finally, it is true that I find Austrian fascism "understandable," but only in the sense of it being subject to rational analysis (p. 11). I have been at pains to show that although the Austrian NSDAP and Styrian *Heimatschutz* did at least recognize some of the weaknesses of Austrian democracy they were not the only groups to do so and that "their solutions only would have made matters worse" (p. 208). Their opposition to the "Parteienstaat" was little more than a demagogic battle cry and certainly was not the sole cause of their existence.

BRUCE F. PAULEY

*Florida Technological University*

PROFESSOR VON KLEMPERER REPLIES:

Mr. Bruce F. Pauley's letter to the editor has just reached me. Since I do not have his book with me and was unable to find it in the

Cambridge University Library, I have to fall back on recollection for my response.

As for Mr. Pauley's excessive reliance for vital information on secondary materials, I have documented it amply in my review. Otherwise the chief source for Mr. Pauley is periodical literature, which I do not consider sufficient for this kind of study. In the Vienna Staatsarchiv and the Graz Landesarchiv Mr. Pauley undoubtedly would and should have found primary materials that would have made his work more substantial, original, and convincing.

As for Mr. Pauley's use of Ernst Nolte's definition of fascism, I find it reprehensible because he adopted it without considering the fact that Nolte carefully confined fascism to "its epoch." In any case, the prognostication of a revival of fascism in Austria in the form of a "new movement against the 'Parteienstaat' and parliamentarism" violates the standard of rational analysis.

I cannot alter my opinion that Mr. Pauley's book, important though its topic is, falls short of a perfect piece of research and conceptualization.

KLEMENS VON KLEMPERER  
*Smith College*



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## Recent Deaths

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CATHERINE DRINKER BOWEN, scholar, musician, and one of America's foremost biographers, died of cancer on November 1, 1973, at the age of seventy-six. Mrs. Bowen was born on January 1, 1897, in Haverford, Pennsylvania. Her father, Henry Sturgis Drinker, was a prominent lawyer and later became president of Lehigh University. Mrs. Bowen received a teacher's certificate from the Institute of Musical Art in New York and in 1919, gave up a career in music to marry Ezra Bowen, a professor. Her writing career began with music criticism articles and later she created a series of enormously well-received biographies including *Yankee From Olympus*, a fictionalized narrative of Oliver Wendell Holmes. She was also noted for her biographies of John Adams and Sir Francis Bacon. In 1957 she won the National Book Award for *The Lion and the Throne*, a biography of Sir Edward Coke. Her last book, *Family Portrait*, was published in 1970.

FRANK T. NOWAK, professor of history at Boston University until his retirement in 1965, died February 21, 1974, at the age of seventy-eight. Professor Nowak was born in Elmira, New York, on February 22, 1895. He was a graduate of the University of Rochester and received his M.A. and Ph.D. degrees in Polish history from Harvard University in 1920 and 1924, respectively. During World War I Professor Nowak served as a captain in the United States Army and later became a member of the Inter-Allied Commission and the Hoover Commission in East-Central Europe, mainly Poland. He was a member of the history faculty at Boston University for forty-three years from 1922 until his retirement in 1965. While there he was a mem-

ber of Phi Epsilon and was Phi Beta Kappa president for twenty years. From 1944-61 he also taught Russian and East European diplomatic history to the graduate students of the Fletcher School of International Law and Diplomacy. Professor Nowak was the author of the recently republished monograph, *Medieval Slavery and the Rise of Russia* (1930, 1957), and contributed several chapters to the *Great Men and Women of Poland* (1941) and to the *Cambridge History of Poland*.

JAMES WELCH PATTON, director emeritus of the Southern Historical Collection and professor emeritus of history at the University of North Carolina, Chapel Hill, died in his sleep on May 17, 1973, while visiting in Charleston, South Carolina. Born near Murfreesboro, Tennessee, on September 28, 1900, he received his bachelor's degree from Vanderbilt University with membership in Phi Beta Kappa. He completed both his master's and doctor's degrees at the University of North Carolina.

Professor Patton's teaching career took him to Georgia State Woman's College, the Citadel, Wittenburg College, Converse College, and in 1942 to North Carolina State University as chairman of the department of history and political science. In 1948 he returned to the University of North Carolina to become director of the Southern Historical Collection. From 1948 until the fall of 1967 Professor Patton served in this capacity while teaching a class in North Carolina history. Upon retirement from his directorship he devoted his full time to the department of history and continued to teach with a reduced load until complete retirement in May 1973.

Published works by Professor Patton include *Unionism and Reconstruction in Tennessee* (1934) and *Women of the Confederacy* (1936), written jointly with Francis B. Simkins. He contributed numerous articles and reviews to professional journals, wrote articles for the *Dictionary of American Biography* and the *Dictionary of American History*, and edited several volumes.

In 1959 Professor Patton was elected a fellow by the Society of American Archivists. He was president of the South Carolina Historical Association, the North Carolina Literary and Historical Association, and the Historical Society of North Carolina; he served as secretary-treasurer and then president of the Southern Historical Association. Though recognized as a careful and conscientious scholar and a superior teacher, his greatest contribution to the profession was the leadership he gave the Southern Historical Collection during the nineteen years under his direction.

J. ISAAC COPELAND  
*University of North Carolina,  
Chapel Hill*

MARGARET LEECH PULITZER, historian, died February 24, 1974, at her home in New York City, following a stroke. She was eighty years old. Miss Leech was born in Newburg, New York, and was graduated from Vassar College in 1915. She started her career as a writer with the Condé Nast Publishing Company and during World War I served with the American Com-

mittee for Devastated France. Miss Leech won the Pulitzer Prize for history twice, in 1942 for *Reveille in Washington*, an account of the nation's capital during the Civil War, and in 1959 for *In the Days of McKinley*, which also won the Columbia University Bancroft Prize. She was the widow of Ralph Pulitzer, former publisher of the *New York World*, whose family established the Pulitzer awards. At the time of her death, Miss Leech had finished the basic research for a biography of James A. Garfield.

DAVID HARRIS WILLSON, professor of English history at the University of Minnesota until his retirement in 1969, died December 11, 1973, at the age of seventy-two. Professor Willson was born in Philadelphia in 1901 and was graduated from Friends Select School and Haverford College. He received his Ph.D. from Cornell University in 1925. Joining the University of Minnesota faculty in 1924, he was awarded the distinguished teacher award of the College of Liberal Arts. A specialist in the field of Tudor and Stuart England and a leading authority on James I, he was the author of *King James VI & I* (1955) and *A History of England* (1967). He was also editor of *The Parliamentary Diary of Robert Bowyer* (1931) and author of *Privy Councillors in the House of Commons 1604-1629* (1940). In addition to his forty-five years spent at the University of Minnesota he also was distinguished visiting professor at the University of Texas from 1966 until 1968.

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## Festschriften and Miscellanies

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These volumes, recently received in the *AHR* office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other *Festschriften* and similar volumes that are amenable to reviewing will be found in the review section.

BRADEEN, DONALD WILLIAM, and MCGREGOR, MALCOLM FRANCIS, editors. *Phoros—Tribute to Benjamin Dean Meritt*. Locust Valley, N.Y.: J. J. Augustin, Inc., Publisher. 1974. Pp. 187, 27 plates. \$18.00.

Bibliography of Benjamin Dean Meritt. A. ANDREWS. The Survival of Solon's *Azones*. DONALD W. BRADEEN, An Athenian Peltast? PAUL A. CLEMENT, L. Kornelios Korinthos of Corinth. GEORGES DAUX, Notes d'Epigraphie Attique. C. W. J. ELIOT, Hay—A Mason's Mark on the Parthenon? DANIEL J. GEAGAN, Ordo Areopagitarum Atheniensium, MARGHERITA GUARDUCCI, L'offerta di Xenokrateia nel santuario di Cefiso al Falero. MICHAEL H. JAMESON, A Treasury of Athena in the Argolid (*IG* IV, 554). L. H. JEFFERY, *IG* I<sup>2</sup>, 1037: An Aiginetan Grave-Inscription. MABEL L. LANG, Again the "Marathon" Epigram. DAVID M. LEWIS, Entrenchment-Clauses in Attic Decrees. HAROLD B. MATTINGLY, Athens and Eleusis: Some New Ideas. MALCOLM F. MCGREGOR, The Join in *IG* I<sup>2</sup>, 55. FORDYCE W. MITCHEL, Three Bars or Four? T. B. MITFORD, A Note from Salamis. MARKELLOS TH. MITSOS, Some Lists of Athenian Ephebes: VI<sup>1</sup>. WERNER PEEK, Epigramme von der Agora. DINA PEPPA-DELMOUSOU, Three Inscriptions from the Epigraphical Museum. ANTONY E. RAUBITSCHKE, Koliéis. O. W. REINMUTH, The Ephebic Dedications to Hermes. WESLEY E. THOMPSON, Tot Atheniensibus Idem Nomen Erat . . . JOHN S. TRAILL, Some Revisions in the Late Roman Archon List. EUGENE VANDERPOOL, The Date of the Pre-Persian City-Wall of Athens. MICHAEL B. WALBANK, Criteria for the Dating of Fifth-Century Attic Inscriptions. A. GEORGE FREY WOODHEAD, West's Panel of Ship-Payers. R. E. WYCHERLEY, Poros: Notes on Greek Building-Stones.

LANDSBERGER, HENRY A., editor. *Rural Protest: Peasant Movements and Social Change*. New York: Barnes & Noble. 1973. Pp. x, 430. \$17.50.

HENRY A. LANDSBERGER, Peasant Unrest: Themes and Variations. RODNEY H. HILTON, Peasant Society, Peasant Movements and Feudalism in Medieval Europe. BETTY H. LANDSBERGER and HENRY A. LANDSBERGER, The English Peasant Revolt of 1381. E. J. HOBSBAWM, Social Banditry. MIKLÓS MOLNÁR and JUAN PEKMEZ, Rural Anarchism in Spain and the 1873 Cantonalist Revolution. PHILIP LONGWORTH, The Pugachev Revolt: The Last Great Cossack-Peasant Rising. GEORGE D. JACKSON, JR., Peasant Political Movements in Eastern Europe. DYZMA GALAJ, The Polish Peasant Movement in Politics: 1895-1969. YU. G. ALEXANDROV, The Peasant Movements of Developing Countries in Asia and North Africa after the Second World War. GERRIT HUIZER and RODOLFO STAVENHAGEN, Peasant Movements and Land Reform in Latin America: Mexico and Bolivia.

THACKRAY, ARNOLD, and MENDELSON, EVERETT, editors. *Science and Values: Patterns of Tradition and Change*. New York: Humanities Press. 1974. Pp. viii, 251. \$11.00.

ARNOLD THACKRAY, The Industrial Revolution and the Image of Science. CHARLES E. ROSENBERG, Science and Social Values in Nineteenth-Century America: A Case Study in the Growth of Scientific Institutions. ROY M. MACLEOD, The Ayrton Incident: A Commentary on the Relations of Science and Government in England, 1870-1873. D. V. A. SEGRE, Social Marginality and Political Legitimacy in Nineteenth-Century Madagascar. JAMES BARTHOLOMEW, Japanese Culture and the Problem of Modern Science. PETER BUCK, Western Science in Republican China: Ideology and Institution Building. CHARLES WEINER, Institutional Settings for Scientific Change: Episodes from the History of Nuclear Physics. YARON EZRAHI, The Authority of Science in Politics.

## Other Books Received

Books listed were received by the *AHR* between May 1 and July 15, 1974. Books that will be reviewed are not listed, but listing does not preclude subsequent review.

### GENERAL

- ADAMS, HENRY. *Lettres des Mers du Sud*. Tr. from the American with notes and an introd. by EVELYNE DE CHAZEUX. Publications de la Société des Océanistes, no. 34. Paris: the Société. 1974. Pp. xxxii, 444, 1 map. 70 fr.
- ALLAN, TED, and GORDON, SYDNEY. *The Scalpel, the Sword: The Story of Dr. Norman Bethune*. Rev. ed.; New York: Monthly Review Press. 1973. Pp. xii, 320. Cloth \$8.95, paper \$3.95.
- BAINTON, ROLAND H., assisted by SUMATHI DEVASAHA-YAM. *Behold the Christ*. New York: Harper and Row. 1974. Pp. 224. \$10.00.
- BOGGS, JAMES and GRACE LEE. *Revolution and Evolution in the Twentieth Century*. New York: Monthly Review Press. 1974. Pp. 266. \$10.00.
- BURLING, ROBBINS. *The Passage of Power: Studies in Political Succession*. Studies in Anthropology. New York: Academic Press. 1974. Pp. xiv, 322. \$11.50.
- BURNS, EDWARD MCNALL, and RALPH, PHILIP LEE. *World Civilizations: Their History and Their Culture*. 5th ed.; New York: W. W. Norton. 1974. Pp. xxvii, 1319. 1-vol. ed., cloth \$10.95; 2-vol. ed, paper \$6.50 each.
- CANO, MELCHOR. *L'autorità della storia profana (De humanae historiae auctoritate)*. Ed. by ALBANO BIONDI. Preface by LUIGI FIRPO. Pubblicazioni dell'Istituto di Scienze Politiche dell'Università di Torino, vol. 28. Turin: Edizioni Giappichelli. 1973. Pp. lx, 196. L. 2,800.
- CLARK, KENNETH B. *Pathos of Power*. New York: Harper and Row. 1974. Pp. xviii, 188. \$7.95.
- DRUCKER, H. M. *The Political Uses of Ideology*. The London School of Economics and Political Science. New York: Barnes and Noble. 1974. Pp. xiii, 170. \$19.00.
- EARLE, PETER (ed., for the Economic History Society). *Essays in European Economic History, 1500-1800*. New York: Oxford University Press. 1974. Pp. vi, 273. \$16.00.
- FLOUND, RODERICK (ed., for the Economic History Society, and with an introd.). *Essays in Quantitative Economic History*. New York: Oxford University Press. 1974. Pp. viii, 250. \$17.75.
- GILLISPIE, CHARLES COULSTON (ed. in chief). *Dictionary of Scientific Biography*. Vol. 9, *A. T. Macrobius-K. F. Naumann*. Published under the auspices of the American Council of Learned Societies. New York: Charles Scribner's Sons. 1974. Pp. xiii, 620. \$35.00. See rev. of vols. 1 and 2 (1970), *AHR*, 78 (1973): 65.
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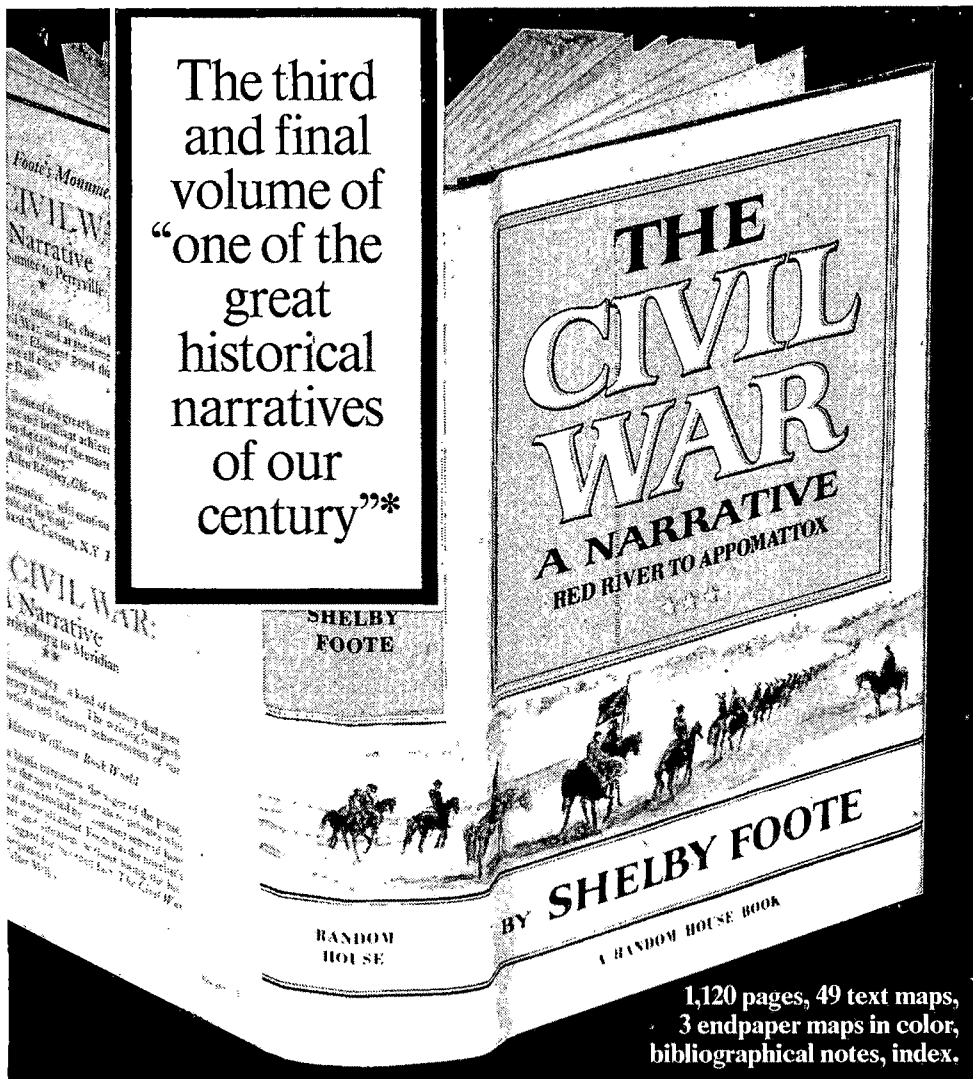
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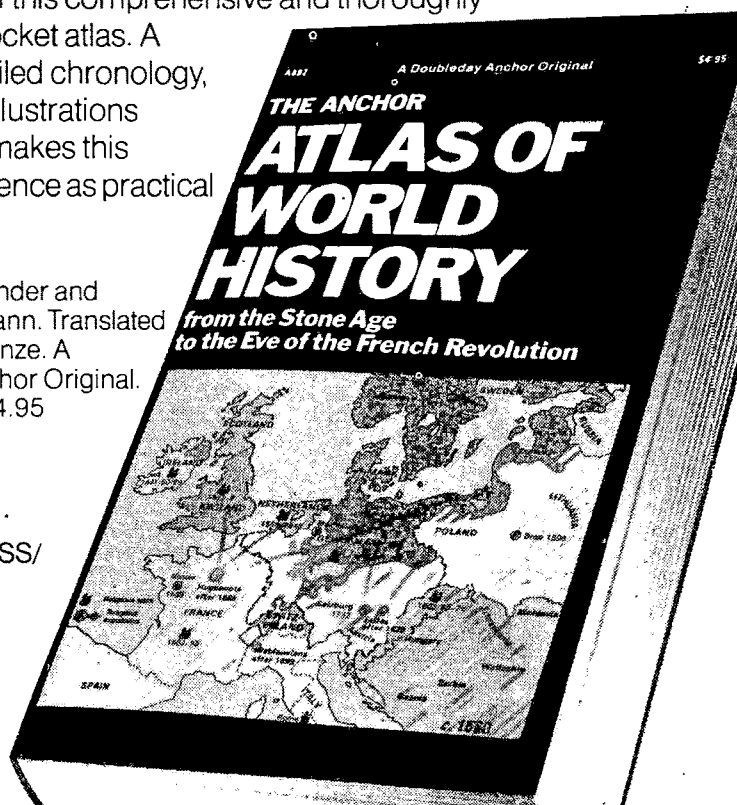
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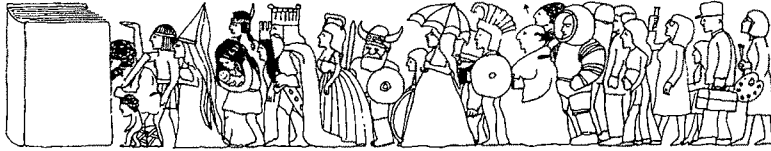
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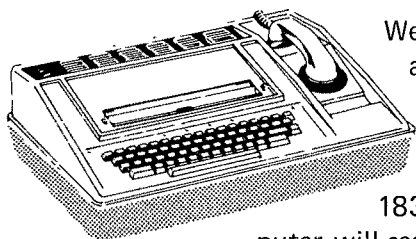
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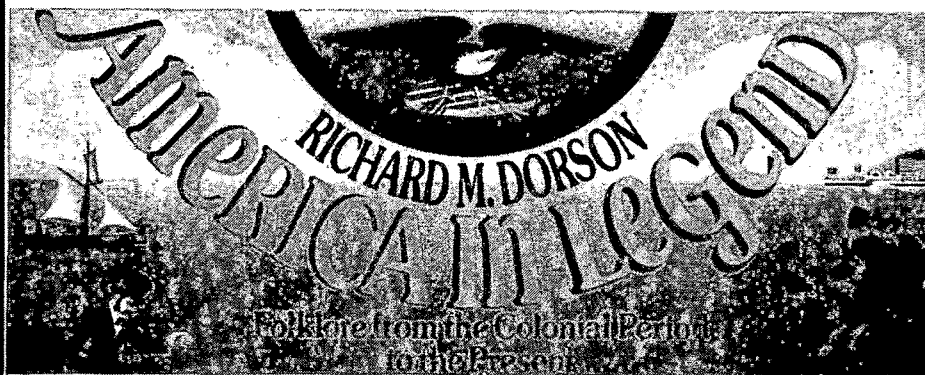
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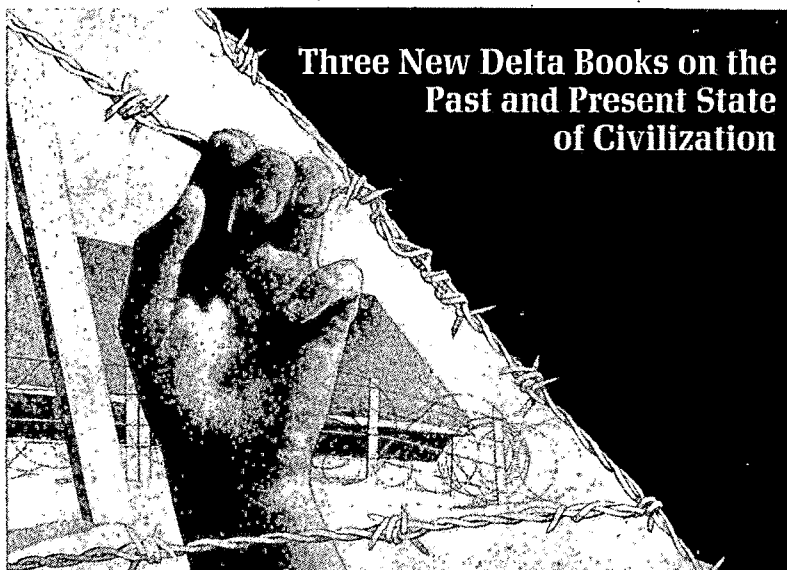
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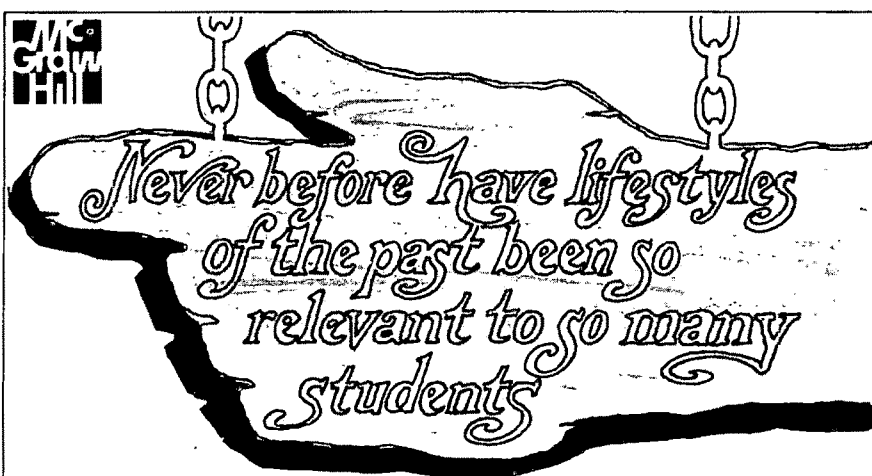
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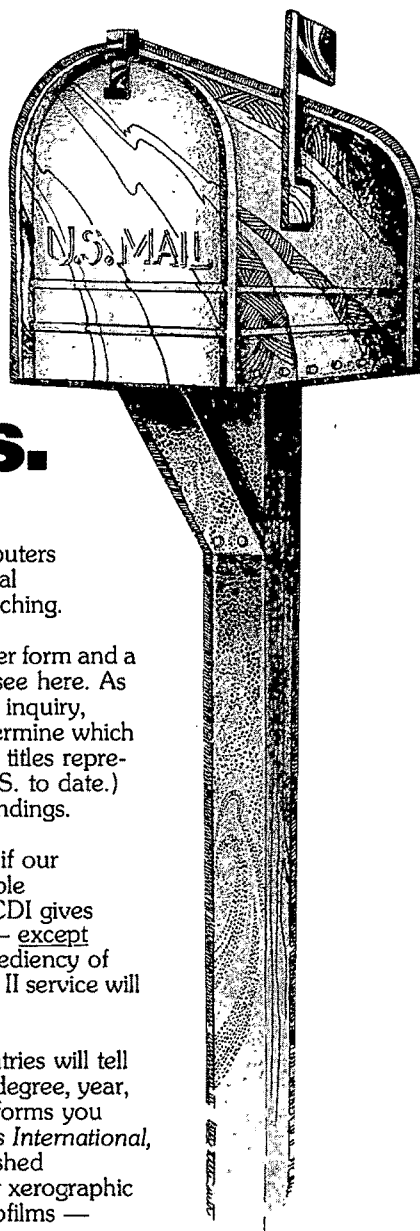
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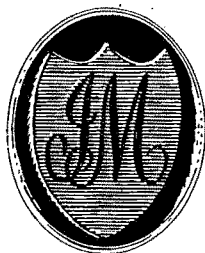
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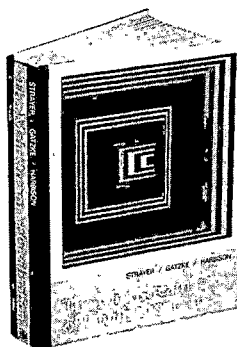
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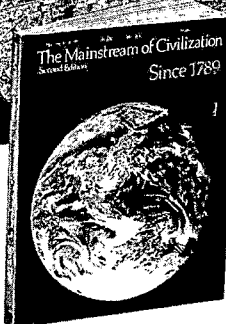
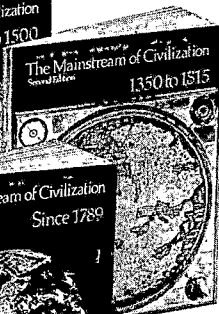
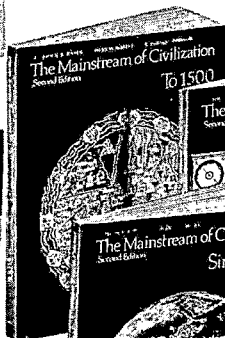
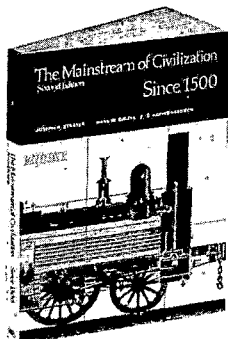


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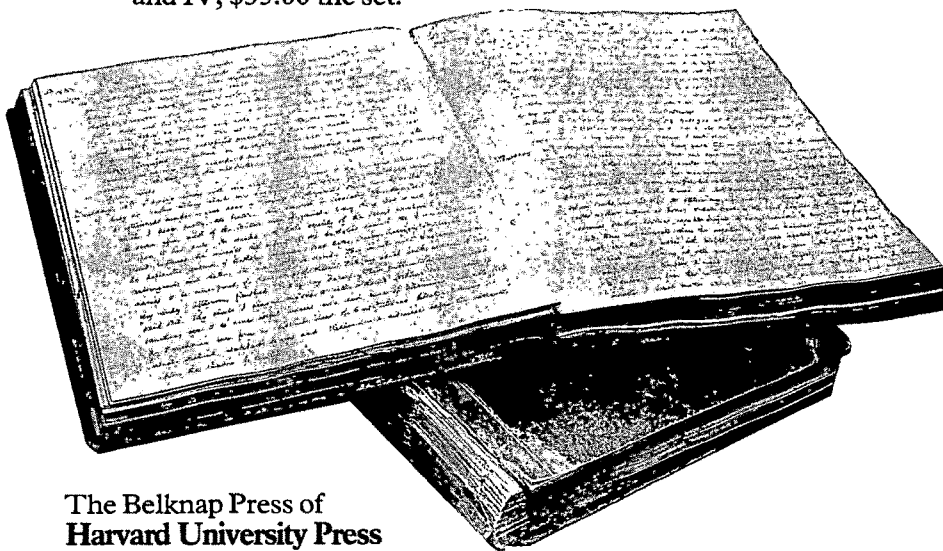
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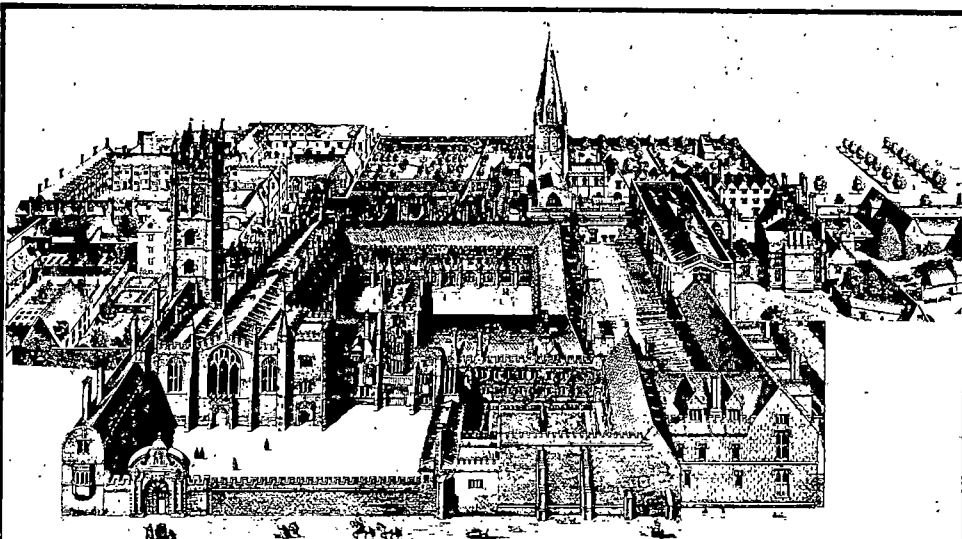
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